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JULY 1847.

No. 13. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Scurvy.* By ROBERT CHRISTISON, M.D., &c.

PART II.—*Account of Scurvy as it has lately appeared in Edinburgh, and of an Epidemic of it among Railway Labourers in the surrounding country.*

(Read to the Medico-Chirurgical Society of Edinburgh, May 5, 1847.)

AMONG the co-operating circumstances which favoured the development of Scurvy in the General Prison at Perth, the "Epidemic Constitution," which prevailed at the time in different parts of Scotland, has been suggested in the preceding part of this paper as partly concerned. This epidemic constitution, as exhibited in Edinburgh and the neighbourhood during last autumn, was marked chiefly by an increase in the general mortality, by a tendency to most acute diseases except those which are infectious, but especially by the prevalence of diarrhœa, cholera, and dysentery on the one hand, and of pneumonia, bronchitis, pleurisy, and acute rheumatism on the other. An increase in the frequency of acute inflammations took place remarkably about the close of the unusual heats in the month of June; and the tendency to bowel-complaints began to show itself in the course of July. Both peculiarities have continued more or less ever since. It is also not unimportant to observe, that about the time this epidemic constitution established itself in the human race, an epidemic of great virulence, and which has been commonly considered to have been pneumonia with typhoid exhaustion, prevailed extensively among milch-cows,

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carrying off a vast number of them in this neighbourhood, as well as in many other parts of Scotland.

It was to be expected that this increased tendency to disease should become stronger as winter advanced, and the failure of the potato-crop, involving dearth of all other provisions, led to a considerable change in the ordinary diet of the working-classes. Accordingly, so far as may be judged by experience in the Royal Infirmary, affections of the bowels have prevailed more than usually throughout the whole winter. Enteric typhus, or dothineritis, in general a rare disease here, has been by no means uncommon. Ordinary continued fevers, both simple typhus, and the short inflammatory fever, have also been gradually increasing, especially since the beginning of February. But by far the most remarkable circumstance connected with the health of the community has been the appearance of scurvy.

From opportunities of personal observation, corroborated by that of various medical friends, it appears that a very few scattered cases of scurvy had commenced so early as in the months of September and October last, throughout the general population of the city, both among the working-classes, and even in the middle ranks. The disease, however, did not attract particular attention till after the commencement of February; about which time a considerable number of cases, chiefly from the sedentary trades, were noticed in Dispensary practice, or admitted into the Infirmary. About the same period a few cases began to arrive at the hospital from the various railways now in the course of construction around Edinburgh. And since that time the number admitted from both quarters has gone on steadily increasing till the present date, the 1st of May.

The first case which occurred to me, and the first I have been able to hear of, was that of a master upholsterer in town, whom I saw with Dr John Brown about the middle of September. There were anomalies in his case; so that the true nature of the disease was not at first manifest. But after seeing the Prison Epidemic at Perth, I became satisfied that he had Scorbutus. Before the end of December, I saw two other cases in the middle ranks of society. In all three the patients had been living for many months, on account of stomach complaints, almost entirely upon bread and meat, coffee and tea, without fresh vegetables, milk, or malt-liquor. A few other instances of the like kind have been occasionally reported to me, by my professional friends, as occurring in their practice. But I have not heard of any in the same station of life, since the disease became prevalent among the working-classes, in February and March. All these cases have been mild in degree, but obstinate of cure.¹

¹ Since this paper was read to the Medico-Chirurgical Society, I have met with

The first case which came under my notice in the working classes of the community, was that of a shoemaker, (Cromarty, TABLE II. 12), who was admitted into the Infirmary, under the care of Dr Paterson, on the 17th February, and was transferred to my charge on the 1st March. This was a very characteristic case. He was about 36 years of age, and a sober, industrious, but rather dull workman, who for twelve months had been earning only eight shillings a week of wages, on which he had to support a wife and five children besides himself. He was taken ill in the month of September with sore gums, and some time afterwards with brown spots on his legs. But, his occupation being a sedentary one, he continued at work till not long before admission into the hospital. He then had spongy, livid, bleeding gums, pains, stiffness, and œdema of the limbs, purple petechiæ on the arms and thighs, extreme feebleness, and remarkable waxy paleness of complexion. The cause was apparently a pure saccharo-farinaceous diet, differing from the dietary of the General Prison in being very defective in quantity, but not materially different in other respects. For seven months before being taken ill in September, he had lived entirely on bread and coffee made with sugar, but without milk; of which fare he partook only twice a day. The daily solids, in rough weight, were $21\frac{1}{2}$ ounces of bread, and about an ounce of sugar, for the first six months; but during the next months, and subsequently until admission into the hospital, the bread, as it rose in price, was reduced by him in quantity to sixteen ounces only. The treatment adopted by Dr Paterson consisted of the administration of lemon-juice, a supply of turnips and carrots, half a pound of meat daily, a little wine, afterwards exchanged for porter, together with the Infirmary Common diet, viz. bread and coffee for breakfast, bread and broth for dinner, and bread and tea for supper. Under this treatment he made a steady recovery, and though he was able to be little out of bed for the first four weeks, he left the hospital quite well in forty-four days.

The next case was a blacksmith (M'Ghee, TABLE II. 15), who was admitted into the Infirmary on the 22d February. He was thirty years of age, and had been a muscular man; but much intemperance, low living, and in November an attack of dysentery, had reduced him greatly. In the end of December he was attacked with pains and stiffness of the limbs, and in the middle of January with sore gums. When admitted into the hospital he had some swelling, tenderness, and lividity of the gums, and great soreness, tenderness, hardness, stiffness, and considerable yellow discoloration of one leg. This man's diet, for three months before he took ill,

two very severe cases, in persons in easy circumstances. One of these had been living as described in the text. The other had been living all winter on bread, butter, tea with sugar and cream, and two eggs every week, for his only animal nutriment. (*May 31.*)

had consisted of bread, tea with sugar but no milk, salt-fish two or three times a week, and on Sundays skimmed-milk at breakfast and fresh meat at dinner. The quantity of food was not defective; for he had on a daily average forty ounces of bread, an ounce of sugar, an ounce of meat, an ounce of milk, and two ounces of fish; so that the total anhydrous nutriment was about $25\frac{1}{2}$ ounces daily. The treatment in this instance consisted of the administration of lemon-juice, together with the hospital Full diet, that is, porridge and milk at breakfast, broth, bread, and boiled meat at dinner, and bread and milk at supper. In 41 days he went out nearly well, a little stiffness and yellowness of the ankle alone remaining.

Analogous to these was the case of a shoemaker (Bannatine, TABLE II. 15), admitted into the Infirmary on February 9th, under the care of Dr Andrew. He was an industrious temperate man, 38 years of age, and much confined by his occupation. He was taken ill in the beginning of December, first with soreness and stiffness of the legs, and soon afterwards with tender gums. At admission the gums were severely affected; both legs were stiff, hard, tender, and somewhat blotched; he had bloody stools, and the complexion was very bloodless and dingy. For three months and a half before he took ill his diet had been intermediate between that of the last two cases, viz. bread and tea at three meals every day, and in addition a herring at dinner three times a week. The daily weight of nutritive articles was 24 ounces of bread, an ounce of sugar, and scarcely two ounces of fish. The nutriment was therefore defective in quantity, as it amounted to $16\frac{1}{3}$ ounces only. This man was treated by dietetic means only, and artificial lemonade made with bitartrate of potash. He was allowed the hospital Full diet, and a pint of porter daily, but got no lemon-juice or fresh vegetables. In 40 days he went out quite well.

These were among the first cases that attracted particular attention in the Infirmary. But others speedily followed from among the ordinary inhabitants of the city; and cases of the same denomination have continued to present themselves occasionally ever since. Summing up those which have occurred in my own wards, and the lists which have been communicated to me by the Ordinary Physicians of the hospital, it appears that up to Monday the 26th April 18 cases of this kind had been admitted into the Royal Infirmary. Only two of these were females. Six belonged to the sedentary trades of tailor and shoemaker; three were ordinary labourers; and the remainder were a barber, a collier, a tanner, a refiner, a blacksmith, a porter, and a traveller. Only a few of them had used a moderate proportion of meat at dinner, but scarcely any of them milk; an article which almost all had been accustomed to in previous seasons. In by far the largest proportion, and worst denomination of cases, the food had been almost purely saccharo-farinaceous, and in general also defective

in quantity. The proximate composition of their food will be found in the Table. It is sufficient to mention here that the nitrogenous part of it was in general scanty; and that gluten was almost the only nitrogenous principle present.

[*May 31.* Since these observations were read to the Medico-Chirurgical Society, I have received many communications from my friends in town and country practice, detailing interesting cases which have occurred in analogous circumstances. I can only venture to allude to one of these which has been mentioned to me by *Dr Thomas Lee*, and which singularly illustrates the relation of this epidemic to milk as a dietetic agent. A young shoemaker and his wife, inhabiting an elevated, dry, airy locality on the Castle Hill, had been compelled by deficiency of employment to live during most of the winter on bread, tea, and coffee, without milk or sufficient vegetables. When Dr Lee first saw them on the 23d of April, the man was pale and weakly, but free of specific disease. The woman was suckling a strong healthy infant, five months old. Towards the close of March the woman herself was seized with pains chiefly in the sacral region, and two weeks afterwards with tender, spongy gums. Subsequently she became liable to slight bleeding from the mucous passages, and to pains in the loins, legs and feet. When first visited she had dark livid puffy gums, soreness of the extremities, some swelling, ecchymosed patches, and petechial spots on the legs and feet, general pains on motion, great weakness, and diminished appetite; but she had an abundant secretion of milk for the child. On the 5th May she was sent to the Infirmary, where she is now far advanced in convalescence.

The number of patients, belonging to other trades besides that of Railway labourer, admitted into the Infirmary up to the 31st of May, has been 35. Three only have been females. The average duration of their treatment in hospital has been 28 days.]

About the time when the cases just described began to present themselves for admission into the Infirmary, a few scorbutics also arrived from various country districts around; where the disease had originated apparently in the same dietetic cause, but in collateral circumstances extremely different. These persons were labourers, employed on the various railways now in the course of construction near Edinburgh.

One case of the kind was admitted so early as the 18th of January from near Melrose on the Edinburgh and Hawick Railway. A month however elapsed before any other appeared. At length a second arrived on the 18th February from Crichton-Moss on the same line. A third followed from the same place on the 24th February; a fourth two days afterwards from the Central Railway near Stirling; and on the 27th February two arrived from Middleton-Moor on the Hawick line. After this the number of applicants rapidly increased. The greatest number have come

from the Hawick Railway, and from almost every accessible station of it,—from Crichton-Moss and Borthwick, about 14 miles off, Middleton-Moor, 20 miles, Galashiels, 32 miles, Melrose, 37 miles, and Eildon, 42 miles south from Edinburgh. A considerable number have also come from the nearer stations of the Caledonian Railway, especially from Cobenshaw-Bog, 18 miles west from Edinburgh. The next locality in point of productiveness has been that part of the Northern Railway in Fife, between Burntisland and Cupar. Up to Monday the 26th April, that is, in the course of two months, no fewer than 108 railway labourers in all have been admitted into the hospital on account of scurvy alone, viz. 76 from the Hawick line, 17 from the Caledonian, 3 from the Central, 9 from the Northern, and 3 from various other railways. A considerable proportion of these poor people have been transported a distance of twenty or even forty miles to hospital, on account of the impossibility of finding proper accommodation and food for them in the thinly-peopled districts where they happened to be working. [May 31. The total number admitted up to this date has been 149, viz. 1 in January, 3 in February, 34 in March, 76 in April, and 35 in May. There is no reason to suppose from the last number that the disease is on the decrease. For in consequence of the great pressure lately occasioned by the increase of fever, the managers of the Infirmary have been obliged to intimate to the railway companies that it is no longer possible to admit scorbutics. The number dismissed on 31st May was 112. There has been no death unless from typhus supervening on incipient scurvy. The average residence in hospital has been 25 days. The most tedious case continued 60 days. There now remain 37 cases of simple scurvy; and there are also 13 cases of typhus occurring in railway scorbutics.]

The list comprises only one Englishman and two or three Scotchmen. The rest are Irishmen; who, with few exceptions, had arrived from Ireland early last summer expressly to work on the railways.

The disease seems to have commenced among them between the middle and end of January. One or two individuals indeed began to suffer during the previous month; and one man, from the Central Railway at Larbert, near Stirling, became slightly unwell even so early as October. But the greater number of the early cases dated their illness not farther back than the middle of January. Some of them had been working at the places where they were taken ill for nearly twelve months, but by far the greater number for periods varying between six months and six weeks. It was only towards the close of October, however, that they became exposed to the apparent cause of their illness; so that they were under its operation for a period varying from six weeks to three months before the first symptoms manifested themselves.

The symptoms have not been in any material respect different

from those observed among the prisoners at Perth. The attention of the men themselves has for the most part been drawn in the first instance to the affection of the limbs; and naturally enough, as it interferes most with their occupation. In general, too, this affection has been in a more advanced state at admission in comparison with that of the gums; and the latter affection has been severe only in those who remained some time under the care of their companions, after the state of their limbs compelled them to quit work. More frequently than among the prisoners at Perth, the ecchymosed patches have put on the black elevated appearance of ordinary contusions; from which indeed it was impossible to distinguish them. In a considerable number, the back of one or both hands, most generally the left, has presented the same firm solid tumefaction which has been described as affecting the limbs of the prisoners at Perth; a peculiarity to be ascribed to the effects of frequent pressure, or slight occasional blows, too insignificant to attract attention at the time. One man, recently admitted, has scarcely any other mark of the disease on the external surface, except this tumour on the back of each hand, and great ecchymosis of the sclerotic and eyelids of the right eye, which suddenly arose after a severe fit of coughing, but which has exactly the appearance of a contusion inflicted with the fist.

Hemorrhagies have been far from frequent. I have often examined the urine without ever having seen it red, and once only I have found it coagulable, in a patient who had general œdema, but who eventually recovered from scurvy, dropsy, and all signs of disease in the kidneys. Ulcers have occurred more frequently than among the prisoners at Perth, but only on the seat of cicatrices from old wounds or sores. In one case only did the affection of the limbs pass on to deep-seated suppuration; and the abscess was very small, and apparently occasioned by poultices applied before I saw him, the disease having been mistaken for local inflammation.

As with the prisoners at Perth, so among the Railway labourers, the disease has been seldom complicated with disorder of the bowels. But several labourers, affected with obstinate diarrhœa or chronic dysentery, have been admitted into the Infirmary along with their scorbutic companions.

A complication by no means unusual has been the concurrence of continued fever with scurvy. Throughout the whole winter cases of simple typhus and enteric typhus have been admitted from the near stations of the Hawick Railway, especially from Middleton-Moor. But the number has increased greatly since Scurvy made its appearance. On Monday last, 3d May, there were 69 fever cases in the Infirmary from the railways. And it appears from an hospital return on the 26th April, that of 61 Railway Scorbutics then in the Infirmary, six were labouring under fever as well as under Scurvy. The cases so affected have perhaps been

too few to justify any deductions as to the influence of the scorbutic diathesis in modifying the fever. But I may mention, that the only case of the kind which has come under my own care was one of the worst cases of typhus I have seen during the winter; and that Dr Robertson, who, having charge of the fever hospital, has more opportunities of judging, informs me he has found scorbutic patients to sustain worse attacks of typhus than others. [May 31. There are to-day 13 railway scorbutics, and 12 scorbutics belonging to other trades, who are ill of fever occurring in the early stage of scurvy.]

It is also worthy of mention that several railway labourers, with merely incipient or even doubtful symptoms of scurvy, have been admitted on account of an anomalous febrile affection, to which there is some difficulty in attaching a name. These have presented extreme exhaustion, faintness, and depression of mind, a very languid pulse, frequent in some, in others unusually slow; a disinclination for food; and scarcely any other disturbance of function. In the course of three or four days the appetite returns; but at the same time the pulse rises, the skin becomes warm and usually moist, the tongue looks dry and whitish, in short, general fever sets in; which in the circumstances is naturally supposed to threaten an attack of typhus. In three or four days more, however, the fever, at no time high, gradually subsides, without any critical evacuation; and then convalescence goes on, though slowly. And meanwhile, the slight scorbutic symptoms, which may have been present at first, are found to have disappeared. It is not improbable that this affection is referable to the same circumstances which seem to have engendered the epidemic of scurvy.

In the course of my professional observation, I have seldom met with an incident so startling, and, on a superficial view, so unaccountable as this epidemic among railway labourers. During thirty years which have now elapsed since I have been attached, with little intermission, in one capacity or another, to the Royal Infirmary of this city, I have not seen a single case of scurvy in its wards until February last. The experience of my colleagues is to the same effect. And in further evidence of the rarity of the disease in the surrounding country, it may also be remarked, that in not a few instances its effects on the limbs had been treated, before admission into the Infirmary, by such active topical means as blisters, leeches, scarification, or cupping,—the true nature of the local disease having been misunderstood, and its constitutional character overlooked. And indeed who could have expected such a disease as scurvy among the labourers on our railways, men mostly in the prime of life, engaged in an athletic occupation, working in the open fields and breezy moors, earning ample wages, and whose extravagance in good living was a frequent subject of remark in their neighbourhood even so lately as last autumn.

It was with no little surprise and interest therefore that I received the following account of himself from the first railway man who was admitted into the University Clinical Ward of the Infirmary. This was an Englishman (Buckland, TABLE II. 7) much older than the generality who succeeded him, and the only Englishman yet admitted. He was a man of 50, temperate in his habits, originally foreman at an iron-foundry, but who betook himself last June to railway work for variety. At the railway where he was engaged as labourer, although there was no want of ordinary provision-shops in a large village hard by, he was compelled, under the "truck system," to buy all his provisions at the contractor's store, or "tommy shop." That is, being paid his wages only at the end of every month's labour, and possessing no ready money at the outset, he obtained his food by means of contractor's orders, which were available only at the contractor's own store. And when pay-day came at the end of the month, the orders he had made use of were checked off against his wages, so that he had little to receive in money at the beginning of a new month beyond what sufficed to pay for his lodgings and clothes. The principal articles of food to be obtained at the store were bread, salt-pork, salt butter, cheese, coffee, tea, and sugar. The bread was described by him as being often wet and doughy, irregularly kneaded, and with little lumps of flour and fragments of rice in it. The salt pork, too, was seldom good, and often so tainted as to be uneatable. The salt butter likewise was frequently rancid, so that after buying it, he could not touch it. The cheese was seldom such as he cared to use. Even the coffee was inferior, although sold at the cost of the finest Mocha coffee. He complained repeatedly of the bad quality of the provisions. At first an improvement was promised. But the amelioration never lasted long; and at length he was told that, if he could not eat the provisions, he might leave the works. His usual allowance of food was a half-quartern loaf daily, a pound of salt pork in three days, a pound of butter every week, as much sugar, and coffee without milk. Hence he had daily 32 ounces of bread, 2·3 ounces of butter, 2·3 ounces of sugar, and 5·3 ounces of pork. The daily nutriment was $26\frac{1}{2}$ ounces, of which $4\frac{1}{2}$ were nitrogenous, and of this $1\frac{1}{4}$ animal in nature. The nutriment was not deficient in quantity, but, for a labourer, deficient in nitrogenous ingredients, and especially in animal nitrogenous principles. It was very different from what he had been accustomed to in the iron-foundry, where he had meat daily, potatoes, milk, and ale. Towards the end of October he was attacked with tendency to faint, swelling of the gums, and bleeding from them after meals; and in the middle of January his limbs became blotched, stiff, and painful, after which he was unable to work, except at short intervals. When admitted into the Infirmary his gums were livid, swelled, fleshy, overhanging the teeth, tender and prone to bleed. The teeth were many of them loose, and although

all sound, he eventually lost six of them. The legs were painful and tender, swelled, cedematous, spotted with a few purple petechiæ and blotched with several large black, elevated, ecchymosed patches, exactly like contusions. The skin was very leucophlegmatic, and his strength much reduced; but his appetite was good, the tongue clean, the pulse natural, and the bowels regular, or rather constipated. He was put upon the Full diet of the hospital, with a pint of porter daily. The warm-bath was also ordered every other evening; but as he disliked it, it was soon discontinued. Under this dietetic system, without any medicinal antiscorbutic, the gums in a day or two became red, an alteration which has been almost invariably the first sign of returning health in these cases. His recovery then went on quickly; and in 27 days he left the hospital perfectly well. This case may serve as an example of the severe form of the disease in railway labourers.

The next will illustrate the cases of average severity, which constitute a large proportion of the whole. This was a muscular, active-looking, young Irishman (Sherry, TABLE II. 8,) 23 years of age, and temperate in his habits. He arrived in Scotland last spring, to work on one of the Ayrshire railways, but in the beginning of December he went to one of the lines near Edinburgh. There he was obliged to purchase his provisions at a contractor's store, so that, although earning sixteen or seventeen shillings a-week, he got no greater variety of food than bread and salt-butter, coffee with sugar, but without milk, salt-pork and salt-fish. There was cheese, but it was generally so bad, that he seldom took it. The butter was tolerable. The salt-pork was often bad, giving out a fetid odour when fried. And the bread was not unfrequently ill-kneaded, doughy, disagreeable to the smell, and ill-tasted. During the first two months he took either salt-pork or salt-fish for dinner, and at breakfast and supper he had bread and salt-butter, with coffee, containing sugar but no milk. For three weeks before he took ill, as he worked far from his lodgings, and could not go home for his dinner,—a common occurrence on railways—he had seldom anything at that meal but bread and butter, which he took out with him in the morning. His allowance of food while he took meat, was $5\frac{1}{4}$ ounces of salt-meat or salt-fish, 32 ounces of bread, $2\frac{1}{4}$ ounces of butter, and the same quantity of sugar, together with coffee. During the period of three weeks before he took ill, when he had no meat or fish, his allowances were 48 ounces of bread, 3·14 ounces of butter, and 2·3 ounces of sugar. In the first period he had 26 ounces of dry nutriment, of which $4\frac{2}{3}$ were nitrogenous, and nearly one ounce of this was muscular fibre; and in the latter period he had 35 ounces of nutriment, of which 5 ounces were nitrogenous, but entirely consisting of the gluten of bread. This diet was essentially different from that to which he had been accustomed in Ireland, inasmuch as he had no milk. In Ireland he had always plenty of milk; and with this, and potatoes and oat-

meal porridge for his whole sustenance, he constantly enjoyed excellent health. In the end of February his limbs became painful; in ten days more he observed that his gums were tender and apt to bleed; and four days afterwards he was admitted into the Infirmary, on the 9th of March. The gums were swelled, red, on the edge livid, tender, and prone to bleed. The limbs were stiff and painful, especially the back of the left leg, œdematous at the ankle, speckled with lenticular brownish-red petechiæ around the roots of the hairs, and here and there mottled with broad livid red patches. There was some frequency of the pulse and heat of skin, no great leucophlegmatia, and an excellent appetite. The bowels were constipated; but not long before admission he had sustained a mild attack of dysentery. He was simply put upon the Full diet of the hospital, with a pint of ale at dinner; and in 29 days he left the hospital quite well.

In order to illustrate fully the dietetic system of these railway-labourers, the two following cases are added, in which the saccharo-farinaceous diet was carried to extremes. The first of these was the case of a robust young Irishman (Donegan, TABLE II. 10), aged 22, who had been about 12 months in Scotland, working on different railways. His usual food, like that of the last patient, consisted of potatoes, oatmeal porridge, and skimmed-milk. During last summer and autumn he was well fed; and in December and January he had porridge and milk, broth and meat, and bread, butter and tea, while he was working on the continuation of the Glasgow railway, through Prince Street Gardens, Edinburgh. In the beginning of January he went to another railway, twelve miles from town, where he had 14 shillings of wages; of which he had to save a portion for the maintenance of two sisters. Finding provisions dear at the store, where he was compelled to obtain them all by means of contractors' orders, he confined himself to bread and tea, with sugar, but no milk. He had meat only thrice during nearly two months that he worked there; and he took no butter, because it was always bad; but the bread was of good quality. This man's food, for six weeks before he took ill, consisted of 48 ounces of bread daily, and 3·14 ounces of sugar, with infusion of tea. The meat, which he took only thrice during that period, may be fairly left out of the reckoning. His nutriment, therefore, was 33 ounces of digestible principles daily, of which 5 ounces, all gluten, were nitrogenous. The mere quantity of total nutriment was quite adequate. Even the nitrogenous nutriment was scarcely defective in quantity. The food was, therefore, faulty in some other more special respect. After being six weeks on this diet his hands became painful and stiff, especially the right one; and soon afterwards his gums became swelled and tender. In fourteen days he was so ill as to be forced to give up work, and apply for admission into the hospital. When admitted on the 25th March, he had swelled, tender, red gums, with livid edges,

and fullness, hardness, livid discoloration, stiffness, and great soreness of the right ham. He was put upon the hospital Full diet, and got no other remedies whatever, not even beer; under which treatment he went out quite well in sixteen days.—Nothing in the history of this epidemic has appeared more remarkable than the prompt recovery of such mild cases under the simplest dietetic treatment alone, if the constitution had not been previously broken down by long confinement to faulty diet, or perseverance at work in spite of advancing disease.

The last case to be mentioned illustrates the same mode of living when superabundant, and shows that even a superfluity of it is no protection against scurvy. A young Irishman (Lawrence Campbell, TABLE II. 15), who had been working on various railways in Scotland for two years, made a practice during most of that period, of living, in a great measure, upon bread and butter, with tea sweetened with sugar, but without any milk; and he appeared not to have made use at any time either of meat, milk, or succulent vegetables, except at distant intervals. For several months before he took ill he had lived absolutely on three meals a-day of bread, butter, and tea. The quantity however was very large, viz., two pounds of sugar, and a pound and a half of salt butter every week, and a quartern loaf daily. His daily allowances, therefore, amounted to 64 ounces of bread, 4·6 ounces of sugar, and 3·14 ounces of butter; which contained no less than $47\frac{1}{3}$ ounces of nutriment, and $6\frac{3}{4}$ ounces of nitrogenous constituents, entirely consisting of gluten. He was attacked with the usual symptoms of scurvy about Christmas, continued to work however for more than two months, was then subjected to treatment, with some advantage, and was admitted into the Infirmary on the 21st of April. His case was one of the worst which had been received from the railways, although he himself stated that he had got considerably better since using meat for fourteen days before. Under the Full diet of the hospital he made a steady, though not rapid recovery; and he is at the present time almost free of scorbutic symptoms, but still very weak. [He went out quite well in 29 days.]

It is unnecessary to multiply such cases. Any addition which might be made, would be little else than a repetition of one or another of them; and they are sufficient to indicate the varieties in the mode of living among these workmen, and to illustrate the following general statement.

In the first place, the men had been living for some time on a different sort of food from that to which they had been long accustomed. While in Ireland, they had potatoes, oatmeal porridge, and milk. It has been commonly represented, and is generally understood in this country, that, until the late failure in the potato-crop, the food of the Irish peasantry consisted in most parts almost exclusively of that root. But this is one of the many vague statements

of practical men, which require to be received with some limitation. On questioning a great number of the labourers who have recently come from all parts of Ireland to work on the railways around Edinburgh, I find that to a man they had abundance of skimmed milk while in their native country, in general cow's milk only, but in some quarters with the addition of goat's milk. And, in the Parliamentary reports of the English Poor Law Commissioners for 1840, it is stated from information obtained in very many quarters of Ireland, that the food of labourers, who live at their own hand, comprised at that time on an average $9\frac{1}{4}$ lbs. avoirdupois of potatoes, and $2\frac{1}{3}$ imperial pints of skimmed milk daily. Since the partial failure of the potato-crop in 1845, more of the labourers than formerly seem to have added oatmeal-porridge to their dietary; and this practice necessarily extended last winter.

On arriving at the Scotch railways last summer, their food underwent material alteration. They had at first abundance of bread, no want of meat, oatmeal, too, when they preferred it, and even milk, though in diminished quantity; their dietary, therefore, was certainly in the first instance not deteriorated. But the destruction of last year's potato-crop, the epidemic among milch-cows, the dearth of all provisions, and the increasing distance of their place of work from their lodgings, which seem to have been chosen, or expressly erected, close to the commencement of their operations, all combined to change entirely their nourishment as winter set in. Potatoes were of course out of the question. Extremely few used milk, and only in very small quantity. Of those who took meat scarcely any had more than a pound and a half in the week, very few indeed above one pound, and many none at all for several weeks together.¹ The meat was generally salt pork, occasionally salt beef, seldom fresh beef or mutton. Cheese seems no favourite with these men, for not one in ten took it at all, and none regularly, or in adequate quantity. Fresh vegetables were never thought of, and were indeed in most places inaccessible. Beer, if taken at all, was used once a-week only, on Saturdays, in excess, and not for qualifying the food, but as a stimulant. Their dietary consisted, then, essentially of bread, butter, and sugar, with infusion of coffee or infusion of tea, and only a small allowance of meat, or none at all. The quantity of food, which they allowed themselves, seems to have been sufficient. The ordinary daily allowance of bread, the staple article, was 48 ounces, never less than 32, sometimes so much as 64. The nutritive principles amounted to at least 27 ounces daily, when a moderate quantity of animal food was used; and varied

¹ I speak here only of the labourers who were attacked by scurvy. I do not know the amount of animal food used by those who escaped; but the vague statements, which have reached me, represent the labourer generally as using much larger quantities of salt-meat, so much often as a pound daily.

from 33 to 48 ounces, when animal food was very scanty or altogether wanting.

In the next place, many complained of their provisions being of bad quality. The particulars have been stated above in describing the cases. Their representations on this head, however, must be received with some reserve. Many of the men complained bitterly of the quality of the provisions at certain of the store-shops; and it can scarcely be doubted that their statements were in some measure well founded. But at the same time it is fair to the accused parties to mention, that an inspection ordered by the Sheriff of this county at my suggestion, and subsequently repeated by order of the Directors of the Railway, at one of the stores most strongly denounced by the workmen, did not bear out their accusation. This is an important question in reference to any measures, which may be taken for protecting the workmen against the cupidity or neglect of their employers. But I apprehend it is of no great consequence to an inquiry into the dietetic cause of the scurvy among them; because the disease has been very prevalent at stations, where no fault was found by the men with the quality of the food supplied to them. In short, it is fully more the kind, than the quality, of their food, which has been faulty.

In its kind it has been saccharine, fatty, and farinaceous; abounding in unazotised principles, especially starch; abounding also in the vegetable nitrogenous principle, gluten; but very deficient in all nitrogenous principles of the animal world. Its composition will best appear from TABLE II., showing the quantity of nutriment of every description in the food of the four illustrative cases. On comparing these with the analysis of dietaries ascertained to be consistent with health, and not productive of scurvy (TABLE II—1 to 6), it will be seen—*First*, that the scorbutic dietary contains comparatively a small proportion of either vegetable albumen, or animal casein, or the mixed nitrogenous constituents of muscular fibre; *Secondly*, that it contains sometimes a deficient amount of nitrogenous principles taken collectively; *Thirdly*, that it is sometimes little, if at all, defective in total nitrogenous constituents; but that in this case the chief or sole principle of the kind present is gluten: *Fourthly*, that this principle is even sometimes superabundant, and yet the diet is scorbutic in its tendency.

It follows, that during a state of active bodily employment, the health cannot be long maintained by a saccharo-farinaceous and fatty diet, although abounding both as to quantity and in the nitrogenous principle gluten.

In regard to treatment, it is quite possible that, like the prisoners at Perth, many of these railway-labourers might have been cured by milk alone, together with the general comforts of an hospital. But it was thought advisable in every case to unite a moderate allowance of fresh meat; and in severe cases beer was also

given, or even porter and wine. In general the amendment began immediately. In a few cases little progress was made for six or seven days; but recovery then commenced and went on quickly. In such cases lemon-juice was sometimes resorted to, in addition to the usual diet; and occasionally with apparent effect on the symptoms; yet not invariably. Hence, as several cases, which advanced slowly at first, made a start in a short time, without anything else than perseverance in the dietetic treatment, the advantage of combining lemon-juice as an anti-scorbutic was doubtful. The duration of the disease in hospital varied in the great majority of instances between fourteen and twenty-eight days; and the average of 112 cases was twenty-five days.

In the present and preceding paper three appearances of scurvy have been described, in which the disease has been identical in character, and, although occurring in very different collateral circumstances, has apparently owned a common dietetic cause. In one set of cases, occurring among city-tradesmen, the food has been defective in quantity, as well as peculiar in kind. But among the prisoners at Perth, and the railway-labourers around Edinburgh, there has been no deficiency in the quantity of food. In all, the main peculiarity has been a great want of animal nitrogenous principles.

Epidemics of scurvy have appeared before now in similar circumstances. *Dr Trotter* mentions in his "Observations on the Scurvy" in 1786, that, when he was surgeon in a slave-ship, the disease broke out with great violence among the negroes a few months after they had been confined to food consisting entirely of beans, maize and rice boiled together, and seasoned with salt, cayenne, and palm-oil. The food therefore presented a similar dietetic error to that of the railway-labourers and others whose cases have been described above. It contained no animal nitrogenised principles, no nitrogenous principles except gluten and legumin. And the quantity must have been abundant; for the slaves got fat before they became scorbutic, and Trotter on that account diminished their rations as part of the treatment.¹ The facts brought forward in the preceding pages render it probable that this epidemic was owing as much to the want of animal nitrogenous constituents in the food as to the want of fresh vegetables; to which defect alone it was referred by Dr Trotter.

The Milbank Penitentiary Epidemic in 1823, described by Dr Latham, seems to belong to the same category. Some have as-

¹ The apparent paradox, established by the experiments of the "Gelatin Committee" of the French Institute in their Report in 1841, that animals may be fattened on particular food, and nevertheless not nourished, but on the contrary starved to death, has been in some measure anticipated by this remarkable observation of Trotter.

cribed it to the food having been too liquid; and there was an error in this respect. But the food also presented more important faults. Its nitrogenous constituents, as will be seen from the analysis of the dietary in TABLE I. 10, did not exceed four ounces, and was therefore a sixth part less than the lowest in the healthy dietaries for prisoners (TABLE I. 1 to 6.) Of this quantity only a third of an ounce consisted of animal principles. And, for a diet of such a kind, the total nutriment was somewhat scanty in quantity, being only 21 ounces, or near the lowest extreme compatible with continued health during protracted confinement. The diet was even more purely farinaceous than that which preceded the Scurvy in the General Prison at Perth in 1846. And accordingly the epidemic prevailed in the Milbank Penitentiary to a far greater extent and in a worse form; for in a population of 858 persons of both sexes, 448 were attacked, or 52 per cent.; and not a few died.

The appearance of Scurvy among the military prisoners in the Milbank Penitentiary in 1840 and 1841, which has been made the groundwork of some important observations by *Dr Baly*, in the London Medical Gazette, xxxi., on the antiscorbutic properties of the potato, may be likewise referred to the influence of a too pure farinaceous diet. *Dr Baly* has brought forward strong arguments in proof, that, while the military prisoners suffered, the civil prisoners or convicts escaped, in consequence of having potatoes for part of their food. It must be observed, however, that the convicts had also a larger amount of nutriment, the potatoes being an addition to a dietary otherwise similar, but still somewhat superior, to that of the soldiers. In fact the soldier's dietary was positively defective in quantity, the total nutriment amounting to only 19 ounces (TABLE I. 11 and 12.) It was also defective in nitrogenous ingredients, which did not exceed four ounces. It was defective too in animal nitrogenous principles, of which it contained only three quarters of an ounce. When the additional consideration is taken into account, that such a dietary differs greatly from the ordinary food of soldiers quartered in London, it cannot appear wonderful that Scurvy occurred among them. Without denying the proposition of *Dr Baly*, that the Scurvy might have been prevented by means of potatoes, it appears equally probable from the preceding investigations, that the disease might have been prevented also, had the too farinaceous nature of the food been duly qualified by animal nitrogenous nutriment, but especially by milk.

A remarkable epidemic of scurvy, which prevailed in 1843, in the Provincial Prison of Prague, and which has been minutely described by *Dr Cejka* in the "Vierteljahrschrift von der Medizinischen Facultat in Prag, 1844," was evidently owing to the same dietetic error. In the three months of March, April, and May, no fewer than 397 prisoners were attacked in a population of 777, or fully one-half. The food consisted of bread, farinaceous soups and

dumplings, peas, and groats; potatoes once a-week; and on Sundays animal soup, and four ounces of meat. The quantity of food was probably defective; but the data are not so particularly stated as to allow the nutriment to be calculated. It is plain, however, that another essential vice in the dietary was its too pure farinaceous nature, and the almost total want of animal nitrogenous constituents. This defect was not lost sight of in the treatment. But, as usual in epidemics of scurvy, reliance seems to have been mainly placed on fresh vegetables, and unfortunately at a season when they are not easily obtained. In following out this treatment, strange substitutes for ordinary vegetables were resorted to, such as the mingled juices of sorrel, dandelion, horehound, milfoil, ground-ivy, and beccabunga, as well as the acknowledged antiscorbutics, cochlearia and water-cress. The success of this treatment does not seem to have been very encouraging; for although it is stated, that the prisoners expressed wonderful relief in two or three days, nevertheless eleven per cent of the sick perished from first to last. Had a liberal allowance of milk and a little meat been given from an early period, the experience of the General Prison at Perth justifies the belief that the result would have been much more favourable; since, in that prison, not a single case of scurvy was lost.

I may likewise mention, that I have lately been informed by various medical friends in country districts at no great distance from Edinburgh, that they have met with scurvy in circumstances very similar to those of the railway-labourers and city-tradesmen. In particular Dr Brotherston, of Alloa, states to me in the beginning of May, that the disease had appeared among the workmen of an iron-work in his neighbourhood; that he had also seen it here and there among labourers; and that he had met with twenty-six cases in all during the three weeks previous. In every instance he found that the food had consisted for some time of bread, oatmeal-porridge, tea or coffee, sometimes salt-butter, in some instances a little salt-meat, but without milk or succulent vegetables of any kind. And one of the worst cases he had seen was that of a young man, whose diet had consisted of oatmeal porridge, bread, and tea, without any meat or milk. Dr Hope, of Polmont, has also informed me that he has lately met with several cases in his district. "These people," says he, "had been living entirely without milk; and the worst case occurred in a woman who had been living entirely on rice and treacle." "Acting on the hint I received from you; they were speedily cured by a plentiful allowance of fresh milk." On the other hand I have been assured by several medical practitioners practising in the finest agricultural districts of Scotland, that they have not met with a single case of scurvy; and on inquiry I have learned, that, although deprived of succulent vegetables by the failure of the potato-crop, the labourers there

have not been without their customary allowance of milk during the whole winter.

The general conclusion to be deduced from all these facts seems to be that no doubt can exist of a tendency to scurvy being engendered by a diet too purely farinaceous, saccharo-farinaceous, or saccharo-farinaceous and fatty; that this tendency cannot be counteracted by even a superabundance of the vegetable nitrogenous principle gluten; but that it may be effectually counteracted by milk, and probably also by other nitrogenized articles of food from the animal world.

This last proposition is opposed to the statements of some systematic authors on scurvy. The most recent English systematic writer, *Dr Budd*, in his excellent description of the disease in the Library of Medicine, says, "it is probable that antiscorbutic properties are possessed exclusively by substances of vegetable origin, v. 75." "Antiscorbutic properties reside exclusively in substances of vegetable origin, v. 77." "We have no evidence that milk is antiscorbutic, v. 94." The preceding observations on the contrary, seem to show, that, as scurvy may originate in various dietetic errors, so may it be advantageously treated by various dietetic remedies; and that milk is a powerful antiscorbutic when the cause is a too pure farinaceous aliment. These observations, likewise, show that treacle, which ranks among antiscorbutics in most recent works on scurvy, is quite the reverse in certain circumstances. *Dr Budd* quotes some remarkable instances of its apparent utility on shipboard, but admits that it has not always proved of service, v. 74. The facts in this paper indicate that it is not an antidote for the scorbutic tendency of a pure farinaceous diet.

In bringing to a conclusion these observations on the tendency of a saccharo-farinaceous diet to engender scurvy, and on the influence of a due admixture of animal food, and especially of milk, in curing the disease, it is necessary to advert briefly to certain other views as to its causes and treatment, in order that the precise object of my remarks may not be misconstrued.

1. In the first place, then, it must be acknowledged, that, notwithstanding the evident influence of deficient animal food in the great majority of cases of scurvy which have lately appeared in and around Edinburgh, a few cases have also occurred in which either meat, or milk, or both, have been taken apparently in adequate quantity. Such cases are more rare than they may seem at first sight. For I have again and again found that, through unwillingness to acknowledge the state of abject poverty to which they had been reduced, patients have at first misrepresented the nature of their food; or it has turned out that the food, though varied in kind and variety, was much too scanty for their occupation. But still, I have myself met with one or two cases, and a few others have been reported to me by my friends, in which fresh meat

seemed to have been taken in adequate quantity, and even milk also, though that more rarely, and the only ordinary article of food wanting was fresh vegetables.

2. But, in the next place, even the want of fresh vegetables could not always be assigned as the sole cause of scurvy in these cases. For in several instances of the kind succulent vegetables had not been used for several years; so that there had been no appreciable difference in diet during last winter. I have seen three cases of this nature in persons in easy circumstances, who for three or four years past had lived, on account of stomach-complaints, in a great measure on meat, fish, bread, tea and coffee, without potatoes or other succulent vegetables; and yet they did not suffer until the present year.

The same remarkable fact has been also observed in regard to some of the cases in which the diet had been saccharo-farinaceous and fatty; one of the worst cases I have seen, is that of a gentleman, whose food consisted all winter of three meals daily of tea, bread and butter, with an egg twice a week. But he had lived on nearly the same kind of food without injury for two previous winters, one of which he spent in Paris. It can scarcely admit of a doubt, that in previous years many other persons must have used continuously, for long periods, the same kind of diet under which scurvy has originated lately; and nevertheless, the disease has either never occurred at all, or so very seldom at least, as to have been unknown to the medical profession in and around Edinburgh, until the latter part of last autumn.

3. There is evidently, therefore, some other cause which has co-operated with faulty diet, in producing so many cases of scurvy for some months past in this neighbourhood. And after considering the question on all sides, no other mode will appear adequate to account for the facts, except the assumption of some particular "epidemic constitution."

4. Under the influence of some such concurring cause, the disease has originated in this neighbourhood in sundry faulty systems of diet; first, and most obviously, in a too pure farinaceous, saccharo-farinaceous, or saccharo-farinaceous and fatty diet; secondly, in what is practically called a dry mixed diet, consisting, that is to say, chiefly of bread, fresh meat, tea, coffee, and butter; thirdly, in a more varied diet, in which the preceding articles have been combined with milk, but without the total nutriment having been adequate; and lastly, but very rarely, under the same varied diet, taken in sufficient quantity, and where the only remarkable peculiarity was the want of succulent vegetables. Every one of the few cases of the last description, which I have had an opportunity of inquiring into particularly, has occurred in persons shaken by infirmities of one kind or another, and especially by dyspeptic complaints.

These are the dietetic circumstances which have come under my

observation as connected with the present epidemic scurvy. But there can be no doubt that it has arisen under other dietetic errors also; and probably any of the dietetic causes adverted to in the former part of this paper (p. 881), is adequate to engender it.

5. Lastly, as various errors in diet may occasion scurvy, so may it be treated by sundry dietetic remedies. In regard to milk, the importance of which has been dwelt on more particularly than any other remedy in these observations, it appears adequate singly to cure many cases originating in a saccharo-farinaceous food, destitute of animal nutriment. But, to prevent misconception, I must observe that, except in the instance of some of the Perth prisoners, treated by Dr Malcom, milk has seldom been trusted to singly in the cases which form the subject of this paper; and that, in all my hospital cases and others in Edinburgh, fresh meat at least has been conjoined with it.—Farther, although I am persuaded that very many cases, originating in the same dietetic cause, may be cured, as most of those under my care have been, by simply substituting milk and fresh meat for part of the farinaceous food, I by no means insist that all cases of the kind may be so cured. I have not myself failed in a single instance in which this method of treatment was employed; but I have seen enough to allow that the conjunction of farther means, such as succulent vegetables, lemon-juice, oranges, and malt liquors may accelerate the cure; and I have been assured by others of some cases having occurred to them in which milk and meat alone failed to bring about recovery.—Still less am I disposed to argue that scurvy, originating in a too farinaceous diet, may not be cured without milk and fresh meat. On the contrary, it is quite possible that, by adding to the farinaceous food, potatoes, or saccharo-mucilaginous roots, or any succulent articles of vegetable food which abound in albumen, a cure may be effected without animal food, if they are given in considerable quantity. But at the same time it seems by no means impossible that, when scurvy originates in a too pure farinaceous diet, these antiscorbutic vegetables are less useful taken alone, than when conjoined with a due proportion of animal food, and especially of milk.—Least of all would I maintain, that milk is an antiscorbutic in all circumstances. Dr Malcom's experience in the Perth prison, followed by further observation on an extensive scale in Edinburgh, proves that it is a powerful antiscorbutic, when the disease originates in a want of animal food. But it is most probably of no use in the opposite case, when the food abounds too much in animal nutriment. This is the general doctrine [Budd on Scurvy, *ut supra*]; and I have not met with any facts which call it in question.

TABLE II.

Showing the Nutritive Proximate Principles in various Dietaries of persons at large, both HEALTHY and SCORBUTIC. The numbers represent avoirdupois ounces.

I. HEALTHY.	Total	NON-NITROGENOUS.				NITROGENOUS.					
		Starch	Sugar	Fat	Beer	Gluten	Legum.	Album.	Casein	Mus.fib	Total.
1. Hessian Soldiers, . . .	32·3	25·7	...	2·31	...	3·78	0·12	0·61	...	2·08	6·60
2. British Soldiers' sea allowances,	26·6	13·9	2·57	2·84	1·0	2·73	0·45	3·04	6·22
3. Navy allowances, fresh,	27·9	9·6	1·5	3·06	8·0	1·84	...	0·16	...	3·71	5·51
4. Do. salt-meat,	26·0	15·8	1·5	2·42	...	2·74	0·83	2·78*	6·35
5. Labourers in Ireland, .	39·6	31·8	2·38	3·10	2·38	...	5·48
6. Berwickshire reapers,	41·8	30·0	1·60	...	1·6	7·04	1·60	...	8·64
II. SCORBUTICS.											
RAILWAY LABOURERS.											
7. Buckland,	26·5	16·5	2·30	3·1	...	3·36	1·23*	4·59
8. Sherry, first dietary .	25·8	16·5	2·30	2·7	...	3·36	0·86*	4·65
9. Do., second dietary, .	35·1	24·7	2·30	3·14	...	5·04	5·04
10. Donegan,	32·9	24·7	3·14	5·04	5·04
11. Campbell,	47·3	32·9	4·57	3·14	...	6·72	6·72
III. SCORBUTICS											
OF OTHER TRADES.											
12. Cromarty; shoemaker,	14·5	11·3	1·00	2·24	2·24
13. Do., second dietary, .	10·9	8·24	1·00	1·68	1·68
14. M'Shee, blacksmith, .	25·4	20·6	1·00	0·16	...	4·20	0·05	0·5	4·84
15. Bannatyne; shoemaker,	16·3	12·4	1·00	2·52	0·36	2·88

* Salt-meat.

NOTE.—1. Calculated from Liebig's Table. 2. From the Scale of Diet for victualling Troops on the voyage to the East Indies. 3, 4. From the present Navy Dietary. 5. From the diet described by the Poor-Law Commissioners in their Report for 1840. 6. From a statement supplied to me by a Berwickshire farmer.

It was my intention to annex to the preceding observations on scurvy a few remarks on certain applications which may be made of the facts stated in my paper to the modern theory of alimentation. But the extent to which the paper has already reached, prevents me from undertaking an inquiry, which is supplementary merely, and which would involve me in considerable details. Referring therefore to what has been said at page 887 and 888 on the modern methods of estimating the nutritive value of the food, I must be content with observing that the Tables in this paper prove, 1. That the value of the food cannot be estimated, as many sup-

pose, by the simple amount of nitrogen in it : 2. That the particular nitrogenous principles must be taken into account, and that in all probability several of them must be present in a certain amount and proportion : 3. That gluten cannot safely replace the whole nitrogenous constituents of the animal world, at least without the co-existence of other vegetable nitrogenous principles,—a conclusion which is singularly opposed to the results obtained by the French “Gelatin Commission,” who found gluten to be the only proximate nutritive principle on which animals might be nourished, to all appearance indefinitely, without any other food whatever.

It is impossible to carry these inferences farther with confidence; because there is a want of adequate facts as to the exact composition of dietaries productive and not productive of disease. But it is probable that a saccharo-farinaceous and fatty diet cannot be continued for a long time, however large the quantity of gluten in it, unless it be combined with articles of food containing either animal casein or vegetable albumen. It is probably to the latter principle, and not as some suppose, to small proportions of salts with vegetable acids, that the ordinary succulent vegetables owe their undoubted antiscorbutic properties.

Case of Delivery, without Operative aid, through a Pelvis extremely deformed by Malacosteon; Child, at the Ninth Month of Utero-Gestation, passing through an oblong Aperture under 1 inch in its narrow, and $2\frac{1}{2}$ in its long Diameter. By J. Y. SIMPSON, M. D., Professor of Midwifery in the University of Edinburgh.

PRELIMINARY REMARKS ON THE MODES OF DELIVERY REQUIRED IN DIFFERENT GRADATIONS OF PELVIC DEFORMITY.

DIFFERENT classifications of the morbid conditions and deformities of the maternal pelvis have been proposed by obstetric pathologists. Some have arranged them in relation to their causes, others in relation to their nature, seats, &c. But the most *practical* classification consists in dividing the morbid states of this part of the skeleton into several grades, according to the actual degree and amount of physical contraction in the pelvic apertures, which is induced by these states; and, consequently, according to the kind and modification of measures required to extract through the defective apertures, a child arrived at or near the full term of utero-gestation.

Drs Denman and Ramsbotham, and Professor Paul Dubois have, for instance, followed this principle of classification in discussing the effects of diseased and deformed conditions of the pelvis upon the process of parturition. Laying aside all pelves of the normal form and standard dimensions, as requiring no accessory aid from art for the transit of an infant through them, these authors have divided all varieties of pelvic contractions into the three following gradations:—

I. Pelves somewhat diminished below the natural standard, but still admitting of the child being expelled through them by nature, or extracted by instruments safe (such as the forceps and vectis) both to the mother and infant.

II. Pelves contracted to such a degree as not to allow a child, at or near the full term, to pass through their apertures till its head was reduced in size by craniotomy.

III. Pelves so very small as not to admit of the extraction through them of a child even after it was mangled and mutilated by embryulsio, and where that last resort of the obstetric art—the Cæsarean section—constituted the only possible mode of delivery.

It is evident that the reference of individual cases of labour to one or other of the preceding degrees of difficulty in the process of delivery, must be often regulated by other circumstances than the mere physical dimensions of the maternal pelvis. The strength and powers of endurance of the patient, the activity of the uterus, and above all, the dimensions and compressibility of the infant's head, as modified by its actual volume and by its state of ossification, the width of its sutures and fontanelles, the separation or not of the bones by previous death and putrefaction, &c., are so many matters forming important points and elements of difference in different instances. But still, however, most of our highest authorities in midwifery have attempted to lay down, in the form of general principles, more or less exact and fixed pelvic admeasurements between the several classes or gradations of pelvic deformity that I have mentioned. In other words, they have endeavoured to reduce to abstract arithmetical formulæ, as it were, the boundaries and limits between morbid states of pelvic contractions, admitting respectively of delivery at or near the full time, *first*, without the necessity of embryulsio; *secondly*, by means of embryulsio; and *thirdly*, where embryulsio is inadequate and insufficient for the purpose.

For the purpose of illustrating these points and statements, I shall collect and throw into a condensed tabular form the opinions of some of our most eminent British accoucheurs on the pelvic dimensions relatively fitted for these different modes of management and delivery. It is necessary, however, to premise one observation in order to understand the figures and formulæ which I shall quote. The apertures of the brim and outlet of the human pelvis are normally of an irregular ovoid form, and they maintain more or less of this configuration under all kinds of morbid contraction and deformity. Indeed it generally happens, more particularly in cases of rickets and malacosteon, that when any of these pelvic apertures is morbidly contracted in one diameter, it comes to be elongated in the other. One of the diameters, either of the brim or outlet, however, is always contracted; and hence in speaking of the relative size of different deformed pelves, accoucheurs often note their dimensions by simply stating (as in the first of the

following tables) the length of their shortest or narrowest diameter.

Smallest size of pelvis through which a child may pass without mutilation. In the standard and normally formed pelvis the narrowest diameter of the brim (the conjugate), and the narrowest diameter of the outlet (the transverse), are each, on an average, about four inches in length. A child, however, may pass through these pelvic openings, though their narrowest diameter be reduced very considerably. The following table shows the opinion of some of the most eminent British accoucheurs on this point.

Smallest Pelvic Diameter admitting of the Passage of a Child without Embryulso.

3 $\frac{1}{4}$ inches—	Joseph Clarke, ¹ Burns. ²
3 ,, —	Denman, ³ Davis, ⁴ Ramsbotham. ⁵
2 $\frac{3}{4}$,, —	Osborne, ⁶ Hamilton, ⁷ Barlow. ⁸

¹ "Having examined by dissection the bodies of many women, who died after tedious and laborious labours, I am enabled to state with some confidence that three inches and a quarter from pubes to sacrum, is the least diameter through which I have known a full-grown foetus to pass *entire*; but it was a very putrid foetus, consequently the head was soft and pliable."—*Dr Clarke in Transactions of the Irish College of Physicians*, vol. i. p. 374.

² Dr Burns considers Dr Clarke "correct when he says that the head cannot pass entire if the diameter be under 3 $\frac{1}{4}$, and even this will generally require the perforator."—*Principles of Midwifery* (1843), p. 471. See also p. 473.

³ "Should the capacity of the pelvis be reduced under three inches, we have no good reason to expect a living child of its full growth, however small, to pass through it, either naturally or by the assistance of art; though the head of one that is dead, especially if it be putrified, or one much below the common size, may be pressed through a pelvis of these dimensions, even without artificial assistance. Should the capacity of a pelvis not exceed, according to our judgment, two inches and a half, then the head of a child, unless the contents be evacuated, cannot pass or be extracted through it."—*Denman's Introduction to Midwifery*, p. 457–8. Edit. of 1816.

⁴ "Pelves of somewhat less dimensions than those of a standard pelvis may occasionally admit of children being born alive at the full period of gestation. For example, a living child of average size, at that period might be born alive, provided the conjugate diameter of the brim of the mother's pelvis was three inches and three quarters, and the head presented in the best possible position. But, if it amounted to no more than three inches, a well-grown child at full period could not be expected to pass without an operation to reduce the bulk of its head."—*Davis's Principles and Practice of Midwifery*, vol. i. page 25.

⁵ "Some practitioners have thought that a pelvis measuring only two inches and three quarters in the conjugate diameter would allow of the head passing whole, provided there was sufficient room laterally. My own conviction, derived from clinical observations, is, that the dimensions I have just mentioned (three inches in the conjugate by four in the lateral diameter), are the smallest which will grant the passage to a full grown foetus."—*Ramsbotham's Obstetric Medicine and Surgery*. Second edition. Page 30.

⁶ "Whenever the capacity of the pelvis is only two inches and three quar-

I am not aware that any English writer on Midwifery has stated it as practicable that a child could pass, without mutilation, through a pelvic diameter less than that mentioned in the last line of the preceding table, namely, two inches and three-fourths. Instances, however, in which children, at or near the full time, have been expelled by the uterus, or extracted with forceps, through pelves measuring only two inches and a half in their narrowest diameter, have been mentioned by some foreign authors. Solayres, Baudelocque and Capuron,¹ each advert to cases illustrating the possibility of this occurrence.

In an obliquely deformed pelvis in my Museum, the conjugate diameter of the brim is one or two lines below 3 inches; and yet through it a dead full-sized child passed as a head presentation, after a long labour, but without instruments. I extracted a second infant through the same pelvis footling. Further, I believe, that when the child presents by the feet, and thus the apex, instead of the vertex or base of the cone formed by the head and body of the fœtus, comes first, that it may make its transit *without* embryulsio through a pelvis, the smallness of which would otherwise have necessitated mutilation or the operation of craniotomy. In this way I

ters, or certainly less than three inches," there is, Dr Osborne conceives, "an utter impossibility for a child of ordinary size, at full time, being born alive by any means, either of nature or art, through so small a pelvis."—*Osborne's Essays on the Practice of Midwifery*. Page 223. At p. 194, he states that "when the bones approach much nearer than three inches, it is utterly impossible for a living child at full maturity, by any means to pass," and embryulsio should be performed early in the labour.

⁷ Dr A. Hamilton, in his letters to Dr Osborne, adduces several cases in which delivery took place safely and spontaneously, "where the deformity of the pelvis seemed to be such, that according to your (Dr O.'s) data, the head of the child should have been opened at the beginning of labour," p. 119. In the first case detailed by Dr Hamilton, a living child passed though the pelvic brim was ascertained by admeasurement after death, to be "sensibly under three inches." Page 101.

⁸ Dr Barlow, in his *Essays on Surgery and Midwifery*, p. 354, gives the following Synoptical Table of the various degrees of distortion of the pelvis and their appropriate treatment.

Degrees of Deformed Pelvis.	Conjugate Diameter of Brim.	Modes of Delivery.
First degree, . . .	From 4 to 3 or $2\frac{3}{4}$ inches.	Efforts of Nature, or assisted by forceps or lever.
Second degree, . . .	From $2\frac{3}{4}$ to $2\frac{1}{2}$ inches.	Premature delivery.
Third degree, . . .	From $2\frac{1}{2}$ to $1\frac{1}{2}$ inches.	Embryulsio.
Fourth degree, . . .	From $1\frac{1}{2}$ to the lowest possible degree of distortion.	Cæsarean operation.

¹ Baudelocque's *Midwifery* (Heath's Translation) vol. ii. p. 370. Capuron's *Cours des Accouchemens* (Brussels Edit.) p. 221.

have, through a pelvis probably not above $2\frac{1}{2}$ inches in its narrowest diameter, extracted a child by the operation of turning, the parietes of the skull becoming compressed and indented to allow of its passage. (See this Journal for March 1847, p. 722.)¹

Highest pelvic dimensions necessitating the performance of the Cæsarean section.—It is well known that Continental practitioners have sometimes recourse to the Cæsarean section under degrees of pelvic contraction that are not considered by British accoucheurs to demand an operation of such great severity and hazard.

In one of the last and best works upon Midwifery, published in Germany, the following observations occur in reference to the degree of pelvic deformity, indicating the Cæsarean operation.

“When the smallest diameter amounts to only $2\frac{3}{4}$ inches, the termination of labour is possible only by making an artificial passage, or by breaking up the child. The possibility of terminating it in the latter manner ceases whenever the small diameter amounts to only $2\frac{1}{4}$ inches or less, and the Cæsarean section is then the only possible mode of delivery, and that to which we must have recourse in all cases, whether the child be dead or not. If the contracted pelvis measures from $2\frac{1}{4}$ to under 3 inches, then the Cæsarean section is indicated when the child is alive, while, if it is dead, perforation is to be had recourse to.”²

Jacquemier, the latest French writer on Midwifery, states, that when the pelvis is below two inches in its narrowest diameter, the Cæsarean section is the only justifiable mode of delivery, even when the child is dead; and when it varies from two inches to two inches and a half, and the child is alive, the Cæsarean operation should be adopted in preference to embryotomy, not only for the sake of the life of the child,—but as, perhaps, not more dangerous to the mother than a protracted and difficult delivery by embryulso generally proves to be.³

As far as I am aware, Velpeau is the only author who has hitherto attempted to collect, in a generalized form, the measurements of the pelves of patients, who have been submitted to the Cæsarean section. He tells us that out of 80 cases in which the

¹ I have elsewhere stated the advantages, in deformed pelves, from turning in comparison with embryulso to be, that—1. It gives the child a chance of life; 2. It is more safe to the mother, because it can be performed earlier in the labour and more speedily; 3. It enables us to adjust and extract the head of the child through the imperfect pelvic brim in the most advantageous form and direction, the head flattening *laterally* under the traction; 4. The neck of the child (if it be living or only lately dead) is so strong as to allow us to exert such a degree of traction upon the obstructed head, that the sides of the cranium may become very greatly compressed, or even indented under it, and that without necessarily destroying the child; and, 5. It is a practice which can be followed when proper instruments are not at hand, and the avoidance of instruments is generally desirable when it is possible.

² Busch and Moser's *Handbuch der Geburtskunde* (Berlin, 1842) vol. iii. p. 108.

³ *Manuel des Accouchemens* (Paris, 1846) vol. ii. p. 162.

cause for the operation was specified, in 62 cases it was required by contraction of the pelvis, particularly in the antero-posterior diameter of the brim. Thus it was :

1 inch	- - - -	in	1 case.
1	to $1\frac{1}{2}$ inches	in	8
$1\frac{1}{2}$	to 2	in	23
2	to $2\frac{1}{2}$	in	25
$2\frac{1}{2}$	to $2\frac{3}{4}$	in	5 ¹

In a number of these cases the Cæsarean operation was adopted under degrees of pelvic contraction, in which delivery, by means of embryulsio, would have been followed by British practitioners. This remark specially applies to the thirty cases included in the two last lines of the table ; and it applies to them the more strongly when we further recollect that the sizes of these pelves in English measurements would have been somewhat more than they appear under French measurements, the French inch being about $\frac{1}{15}$ part longer than the English inch.

In choosing in any case of contracted pelvis, between the alternatives of craniotomy and the Cæsarean section, Continental practitioners generally look upon the life of the child, as well as the probable degree of difficulty and danger likely to ensue to the mother from a painful and protracted delivery by embryulsio, as important points and elements in deciding between these two methods of delivery. In this country, little, or indeed no attention has hitherto been given to these considerations in forming a practical conclusion on the question. In fact, British accoucheurs have never deemed themselves entitled to have recourse to the Cæsarean section, unless the pelvic apertures were so much reduced as to prohibit the practicability of the extraction of the child through them by embryulsio. With them the *propriety* of delivery by the Cæsarean section begins exactly with that degree of pelvic deformity at which the *possibility* of delivery by embryulsio terminates.

Hence in order to fix and determine the highest limit of pelvic contraction which necessitates the performance of the Cæsarean section, we have merely, in the first instance, to fix and determine the lowest limit of pelvic contraction at which delivery by embryulsio is capable of being effected.

The following table presents, in a condensed form, the opinions of various British and American obstetricians upon this question, namely, the actual degree of pelvic contraction *above* which it is considered still possible to deliver by embryulsio, and *below* which it is deemed proper, and absolutely necessary to extract the infant by the Cæsarean section.

¹ Traité Complet de l'Art des Accouchemens, vol. ii. p. 458.

Smallest Pelvic Diameters admitting of the Passage of a Child by Embryulsio.

$3\frac{1}{2}$ inches	by	2 inches	Dewees, ¹ Bedford, ² &c.
3 ———	by	$1\frac{3}{4}$ ———	Burns, ³ Hull, ⁴ &c.
3 ———	by	$1\frac{1}{2}$ ———	Barlow, ⁵ Hamilton, ⁶ &c.

During the last ten or twenty years, various improvements have been proposed in our embryulsio instruments, particularly with the view of rendering that operation more safe and easy in cases of unusual difficulty and deformity. Lest, with such additions and improvements, the preceding table be supposed to misrepresent the existing rules and doctrines of our modern British schools of Midwifery respecting the degree of pelvic contraction, necessitating the adoption of the Cæsarean section, I shall cite, in reference to the indications for this operation, the opinions expressed upon the subject in the three last obstetric text-books that have issued respectively from Edinburgh, Dublin, and London.

In his "Introduction to the Study and Practice of Midwifery," (Edinburgh, 1843) Dr Campbell observes, "unless we have a clear

¹ "By a sufficient diameter I mean, where there is at least two inches in the antero-posterior, and at least three and a half in the transverse; below this, delivery *pervias naturales*, I repeat, I believe to be impossible."—*Dewees' System of Midwifery* (1837), p. 579.

² "I do not believe it is possible to remove a child by embryotomy, when the antero-posterior diameter of the superior strait measures less than two inches, without subjecting the mother to severe hazard, provided the child be of the ordinary size. I am satisfied, that even with the space of $2\frac{1}{4}$ inches, all the dexterity which the operator can bring to his aid, will be required to protect the mother from serious, if not fatal inquiry. I, without hesitation, would prefer the Cæsarean section (if I had certain evidence that the child lived) to any attempt to extract it, *per vias naturales*, if the antero-posterior diameter measured less than $2\frac{1}{8}$ inches."—*Dr Bedford in his Translation of Chailly's Midwifery*, note p. 386.

³ "The crotchet cannot be used, when the head is of the full size, unless we have a passage through the pelvis and its linings measuring fully an inch and three quarters in the short diameter, and three inches in length; or if the child be premature and soft, an inch and a half broad, and two inches and three quarters long."—*Dr Burns' Principles of Midwifery* (1843), p. 508.

⁴ "I am of opinion, that it still remains to be proved whether a mature fœtus of the ordinary size has ever been extracted, with safety to the mother, through a pelvis in the superior aperture of which there was not in any point from the fore to the hind part a space equal to $1\frac{1}{2}$ inch."—*Dr Hull's Defence of the Cæsarean Section*, p. 391.

⁵ See Dr Barlow's Synoptical Table quoted in preceding page, or "*Essays on Surgery and Midwifery*," p. 354.

⁶ "Whenever the short diameter of the pelvis, either at the brim or at the outlet, will not admit above one ordinary sized finger, or really falls under one inch and a half, no other means are justifiable for the delivery, if the child be arrived at the full period of gestation, than that tremendous expedient, the Cæsarean operation."—*Dr A. Hamilton's Letter to Dr Osborne*, p. 139.

space of two inches, or nearly so in the transverse (conjugate), and fully three in the lateral diameter of the brim, embryotomy must be abandoned, as not likely to ensure the safety of the parent," p. 319.

In his work "On the Theory and Practice of Midwifery," (1843) Dr Churchill of Dublin, after an elaborate investigation of the subject, draws the following deduction. "We may therefore (he observes) safely conclude, that when from any cause, the antero-posterior diameter of the upper outlet, or the transverse diameter of the lower, is not more than $1\frac{1}{2}$ inches, there is no possibility of delivery *per vias naturales*, but that we must have recourse to the Cæsarean section," p. 314.

Dr Francis Ramsbotham of London, in his "Obstetric Medicine and Surgery" (London, 1844), observes when speaking of deformity of the pelvis (p. 30), "I am quite convinced, that unless there be at the brim one inch and three-eighths in the conjugate, by three and a half in the iliac (diameter), or $1\frac{1}{2}$ inches in the conjugate by three in the iliac, it would be useless to attempt delivery *per vias naturales*." And again, when treating of difficult labours he remarks, "If upon a measurement conducted with the utmost care, we find there is less space at the brim than three inches and a half laterally, by one inch and three-eighths in the conjugate diameter; or three inches by one inch and a half; we ought to consider it our duty, however painful and appalling, that may be, at once to propose the Cæsarean section as the only means by which it is possible to save the mother's life; and as offering also the sole chance of safety to the child," p. 179.

Our highest, as well as latest authorities in British midwifery, seem thus to have fixed upon a degree of pelvic contraction, in which the dimensions varied from $3\frac{1}{2}$ to 3 in the long diameter, and from 2 to $1\frac{1}{2}$ in the short diameter, as the lowest limit at which delivery by embryulsio can be performed,¹ and below which it is always and invariably necessary to have recourse to the Cæsarean section, when the child has reached the full time. In the case, which it is my object in this communication to detail, the inferior pelvic aperture of the patient was so deformed and contracted

¹ I do not stop to discuss the question whether, in all cases of great deformity, embryulsio is always *proper*, where it is *possible*. The difficulty attendant upon its performance in instances of marked contraction has sometimes been extreme. In a case where the narrowest diameter of the brim was thought to measure *two inches*, Dr Meigs required many hours to break down the cranium, and afterwards three or four hours' pulling to extract the child with the crotchet. The mother was altogether three or four days in labour (Philadelphia Practice of Midwifery, p. 322). In a woman with a pelvis, the conjugate diameter of which "was estimated at *little more than an inch and a half*," Dr Hamilton effected delivery by embryulsio. The extraction alone occupied four hours, and "required such an exertion of force, that he was literally obliged from exhaustion to be carried home in a sedan chair." (Practical Observations, p. 263). Lately, my friend Professor Murphy extracted a child

from Malacosteon, that it was considerably less in its dimensions than the lowest limit stated in the preceding table and extracts, as capable of permitting of delivery by embryulsio, and yet the mother after arriving at, if not past, the full time of utero-gestation was delivered not only without embryulsio, and without the Cæsarean section, but in fact without instrumental interference of any kind. Further, the child, after its birth, was passed through an unyielding aperture measuring under one inch in its short, and two and a half inches in its long, diameter; or, in other words, this aperture was fully half an inch in all its dimensions *below* the limits supposed to demand the necessity of the Cæsarean section. Before showing the solution of such a seeming obstetric enigma, I shall briefly state the anterior history of the patient.¹

DETAILS OF THE CASE OF MALACOSTEON, AND INFERENCES FROM IT.

Mrs D——, Cupar in Fife, now 34 years of age, was in early life regarded as a robust and large child. She became a dressmaker in Edinburgh, at fourteen years of age. The sedentary habits of this profession betimes rendered her catamenia very irregular, and brought on so much general delicacy of health that she was advised to forego the occupation. She went to Cupar and lived there with a relation. In 1837, when about 24 years of age, she married. Two years subsequently to that event she began to complain of pains in the back and sides, and stiffness about the knee-joints. From this attack she never recovered so fully as to be able to walk without support, and continued to suffer much with shifting pains. In 1840 she fell, while walking with a staff across her room, and after this the pains in the limbs were for a considerable time far more severe, and the lameness greatly increased. Since recovering she has been able to walk out of doors with the assistance of crutches. But various bones of the trunk and extremities have become shortened and deformed under the effects of the malacosteon. The spine is bent backwards and outwards in the form of a bow, with the ribs and sternum correspondingly displaced. Some of the phalanges of the fingers are bent; the right thigh bone is curved forward into a semicircular shape. And from being a hand-

by embryulsio in a pelvis, the brim of which measured *one inch and a half* in its conjugate, and four and a half in its lateral diameter. The operation required about seven hours' work on the part of Dr M. The patient was in labour from Tuesday to Friday (Lancet for April 3d, 1847). "I question much (observes Dr Burns) if extreme cases of embryulsio be not as dangerous to the patient as the Cæsarean operation, certainly they are more painful." (Midwifery, p. 501). And he elsewhere adds (p. 509), "I shall not be surprised, if in a few years, British practitioners come to resort more frequently, especially in extreme cases, to the Cæsarean section."

¹ For these particulars I am much indebted to the kindness of Mr Wiseman, of Cupar, the patient's medical attendant.

some and somewhat tall woman, she has shrunk down, during the course of the last seven or eight years, into a deformed dwarf-like figure, measuring about four feet in height.

From the time of her marriage up to last June (1846) Mrs D. had never become pregnant. Sometime, however, during the course of June—but she is not certain at what precise period of the month—the catamenia appeared for the last time. She was delivered on the 28th of April, or ten months after the last menstruation.

For some time after pregnancy commenced, Mrs D. feared that the swelling and increased size of the abdomen was the result of dropsy, and did not watch her own feelings so as to be aware of the period of quickening. At last, however, the motion of the child, &c. became so unequivocal, that she applied to Mr Wiseman to attend her in her approaching confinement.

On examining into the condition of the pelvis, Mr Wiseman at once found that its outlet was extremely contracted, but the pregnancy was already so far advanced as to preclude, under such a degree of deformity, the idea of delivery by the induction of abortion or premature labour. Besides, all attempts to touch the os uteri proved ineffectual, so that none of the ordinary means of arresting pregnancy, and exciting uterine action, could have been put into practice.

Early in March I visited Mrs D. along with Mr Wiseman, Dr Graham, and Dr Grace. She was quite incapable of moving or turning in bed without assistance. The uterine tumour was high, and pressed over to the right side. On applying my stethoscope to it, I readily detected the usual rapid pulsations of the foetal heart. I found fully borne out the correct description which had been previously given me of the excessive deformity and contraction of the pelvis. The sacrum was straight above, so that its promontory did not probably encroach on the brim; but its inferior extremity was strongly and anormally curved forwards. In front the walls of the pelvis felt doubled or collapsed together; and the outlet, which was the only part that could be very accurately examined, was exceedingly deformed and diminished in size. The transverse diameter was particularly contracted. I found it impossible to introduce two fingers between the tuberosities of the ischia. Hence this diameter of the outlet was evidently under an inch. Posteriorly, or opposite the sacro-sciatic ligaments, there was transversely more space, but the strong anterior curvature of the coccyx and lower end of the sacrum seemed to curtail the conjugate diameter of the opening, and to prevent the probability of it admitting, when fully dilated, more than three, or, at most, four fingers even in this direction.

Under these circumstances, with a living child advanced to the eighth month, and a pelvic outlet so extremely contracted, I had no hesitation in coming to the same conclusion as the patient's

medical advisers had all previously done,—namely, that the Cæsarean section was the only practicable mode of delivery. I was aware that Barlow, Conradi, Sprengel and others had seen and published cases of mollities ossium, in which the softened and still flexible pelvic bones had bent and yielded during labour so as to permit the passage of the child without operative interference; but in Mrs D. the osseous tissue appeared far too firm to permit us to indulge even in this faint hope. Nature, however, provided, and was perhaps already preparing for the mother, a mode of delivery that was still more safe and easy.

Having agreed to operate in case the Cæsarean section was required, I anxiously waited in the daily expectation of being called to Mrs D. At last, on the morning of the 28th April, I received from Mr Wiseman a note, dated the previous night, intimating that our patient had begun to complain of labour pains—that the os uteri could be felt projected low down into the vagina—that it was not opened more than two or three lines—and that the presenting part of the child could not yet be detected. Along with some professional friends who had agreed to accompany me, I forthwith proceeded to Cupar, a distance of about thirty miles. On arriving there, we were surprised to hear that the patient was delivered, and our surprise was only increased by learning that no kind of instrumental aid had been required. A visit, however, to the room in which the child was, readily solved the apparent riddle.

The infant had been dead for some time *in utero*. It looked nearly the natural length, and as it lay extended on the table, it measured $18\frac{1}{2}$ inches from the crown of the head to the heels. But its limbs and body were thin, lank, and atrophied; and its weight was only 3 pounds 2 ounces. Its head appeared very large and disproportionate in size, and indeed had been considered hydrocephalic. This seeming increased volume, however, was not the consequence of effusion, but the result of putrefaction. The encephalon was in a dissolved and semi-fluid state; and as the head lay on the table it was extended and flattened out laterally and superiorly, as if it consisted of a bag or bladder, half-filled with liquid or semi-liquid contents; and such in fact it really was. For all the bones of the arch of the cranium were separated from their attachments, and floated about in the dissolved and liquified cerebral matter. The bones of the basis of the skull were also loosened and more or less separated from each other—a rarer occurrence. On handling the vertex the first piece of bone which I touched was the orbital portion of one of the ossa frontis. The symphyses even of the inferior as well as of the superior maxillary bones were loosened and admitted of free motion. And the component parts of the head and face were so easily displaced and compressed that, on placing one of my fingers in the lower occipital region behind, and another on the nose or cheek in front, I found that, without

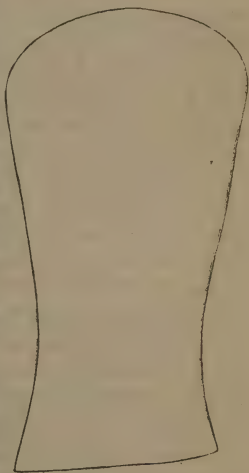
any considerable degree of exertion, the two could be made to approximate to within a few lines of each other. Yet the skin of the scalp and face was continuous and entire, the epidermis only being separated at different parts. The chest and abdomen of the child seemed quite soft and pliable, though not in so disintegrated a state as the head.

Thus far the putrid and diffluent state of the foetus appeared to afford an easy explanation of the possibility of its transit through the very contracted pelvic outlet of the mother. But I was anxious to have more full and complete proof that the foetus, even in this state, was capable of passing through an aperture of dimensions so small as we knew the pelvis to present in the case of Mrs D. In order to obtain this proof I got oblong openings of two or three different sizes cut in plates of white iron. The smallest of these perforations, however, though only $3\frac{1}{2}$ inches long by $\frac{7}{8}$ broad, proved unnecessarily large for the experiment. We diminished it by filling it up at one end with strong, thick, and perfectly unyielding sole leather, so that the whole opening measured only $2\frac{3}{8}$ inches in its largest, by $\frac{7}{8}$ of an inch in its shortest diameter; and yet through this aperture (of which the accompanying wood-cut gives the exact outline),¹ the child was pulled without any great degree of force or difficulty.

In dragging the infant through this aperture no particular resistance was met with from the bones of the head and face; but the size of the liver impeded its transit for a minute or two, as the lower part of the thorax was passing through the metallic opening. Dr Graham, Dr Ziegler, Dr Weir, Mr Wiseman, &c., witnessed these experiments with me; and, if our limited time had allowed us to remain longer in Cupar, and to have got another plate perforated, probably we would have found the child capable of passing through an aperture one or two lines smaller in some of its dimensions.

The placenta had been preserved. It was small and atrophic, and contained scattered through it a number of those white tubercles (as they are sometimes improperly termed), which we so often see connected with, and causing marasmus and death of the foetus in utero. Some of these tubercles or fibrinous deposits were of the size of hazelnuts, or larger.

The history of the delivery had been this. Slight labour pains



¹ The perforated iron plate and superadded piece of leather, are carefully preserved in the Obstetric Museum of the University.

had come on during the afternoon of the 27th. She was seen in the course of the evening by Drs Graham and Grace and Mr Wiseman, who found the os uteri beginning to dilate, but the pains were not severe, and the husband and attendants of Mrs D. all went to bed. About one o'clock in the morning of the 28th Mr Wiseman was raised, the waters having broken about an hour previously, and the uterine contractions having become strong and bearing down. When Mr Wiseman reached the house of the patient, he found the soft scalp of the child already bulging through the external parts. Some detached bones, included in the portion of scalp that had passed, allowed him to obtain a firm hold of the protruded portion of the head, and thus enabled him to use some extractive force. By thus assisting the effects of the pains, the child was entirely born about half an hour after Mr Wiseman's arrival. The mother has made a very good recovery, and declares that "having a child is nothing."

There are no sufficient data to determine at what precise time the infant died. Milk had been discharging from the nipples for three or four weeks before delivery; but whether the child had perished about that period it is impossible to say. Mrs D. deceived herself with the idea that she felt it living and moving up to the time of delivery.

The preceding case is, I believe, unique in the annals of midwifery. It will perhaps therefore require no apology if I add one or two brief inferences which the history and details of it appear to suggest.

1. It has taught me, and is, I conceive, calculated to teach others, a strong lesson of caution in regard to our *prognosis*, under apparently even the most desperate circumstances. In such rare forms of complication as this, we must depend for our prognosis, &c., upon the anterior observations and recorded facts of others. Here all such records led me to expect a very different result, and offered no hope whatever of such a fortunate termination as actually occurred.

2. The case affords a new and striking illustration of the just and well-known remark of Dr Denman, that "the resources of nature in every thing which relates to parturition, are infinite, and constantly exerted for the preservation of both the parent and child; yet when the two objects are incompatible, the life of the child is almost uniformly yielded to that of the parent."¹ And the mode and mechanism by which nature brought about this unexpected result in the present instance, are highly worthy of special notice. For, *first*, she set up a diseased condition of the placenta, which prevented the full and proper nourishment of the fœtus, and thus restrained as far as possible its development and growth. *Secondly*, she carried this state of marasmus to such a degree, as at last proved slowly fatal to the child, without superinducing that expulsive action which generally soon follows the death of the in-

¹ Practice of Midwifery, p. 415.

fant. *Thirdly*, the dead infant was subsequently retained for so long a time in utero, that not only the bones of the cranium, but the bones of the basis of the skull and face were loosened and separated from each other, and the head and other parts of the body thus rendered readily and easily compressible.¹ And, *lastly*, the emaciated, dead, and highly putrefied infant after being thus reduced to this diffuent and compressible mass, and now capable of being moulded to the contracted apertures of the pelvis, is ultimately and without difficulty expelled through them by the supervention of natural uterine contractions. Each stage and step in this mechanism was necessary for the success of that which followed it, and the imperfection or omission of any one of them, would probably have entirely subverted and prevented the very fortunate and very unlooked for result that occurred from the combination of the whole.

3. Does the mode in which the delivery was effected in this

¹ No individual case in midwifery has given rise to so much discussion, as that of Elizabeth Sherwood. Her pelvis was estimated by Dr Osborne as below the lowest standard which I have given in a preceding page, as capable of allowing of the passage of a child by embryulso, being only about $\frac{3}{4}$ of an inch from the sacrum to the pubis; and yet he delivered her successfully by craniotomy after working and pulling for three hours with the crotchet. Many authors have stated that Dr Osborne must have undoubtedly under-measured the pelvis of Sherwood;—and Drs Hull, Burns, Hamilton, &c., have denounced the operation of embryulso as “impracticable” if Dr Osborne’s measurements were at all true and accurate. Dr Campbell, for instance, observes “It would be idle to enter largely on the refutation of this extraordinary case, since Dr Osborne’s narrative of what he thought he had accomplished is irreconcilable with common sense; for how could the base of the cranium which is $1\frac{1}{2}$ inches in thickness, and nearly three in breadth, be brought through the aperture which he describes. A fair estimate” (Dr Campbell continues) “of the utter impossibility of effecting it may be afforded by the simple experiment of forming in a plate of hardwood, an opening in shape and size, exactly corresponding to the pelvis of Sherwood, and attempting to force through it the base simply, divested of the other portions of the skull.” (*Midwifery*, pages 317 and 318.) In the case of Mrs D— I obtained the corroborative evidence afforded by the very experiment which Dr Campbell here properly suggests; and I have already stated the facility with which the child was passed through the perforated plate. In Sherwood’s case there was, I believe, the same reason for the practicability of delivery, for the foetus seems to have been in the same putrid and decomposed state as Mrs D.’s child, and perhaps the bones of the face and basis of the cranium were in a similar way loosened and compressible. “The whole body” of the foetus was, to quote Dr Osborne’s own words, “in the most putrid and almost dissolved state.” See p. 101 of his *Essays*.—But, besides, the brim of the pelvis in Sherwood was in reality not so small as the measurement of its conjugate diameter would seem to indicate. During delivery the os uteri was pulled by Dr Osborne over to the right side, or to the space intervening between the line of the conjugate diameter and the right ilium. Here there was an oblong aperture 3 inches long (as measured from the ilium to the symphysis pubis), and $1\frac{3}{4}$ inches broad; and hence in fact an aperture as great as Drs Hamilton, Burns, Churchill, Ramsbotham, &c., deem necessary for the performance of embryulso; and greater than that through which we pulled Mrs D.’s child.—See drawing of the brim of Sherwood’s pelvis, in *Dr Hull’s Defence of the Cesarean Section*. Pl. v. fig. 1.

instance by nature suggest any measures of practice which, under similar complications, we could induce and imitate by art? I put this question, because, in the greater deformities of the pelvis all the standard operations and means which we employ for delivery, are in fact imitations of processes and operations which nature herself employs under the same conditions. When the pelvis has been much contracted, abortion has occasionally come on in the earlier months and saved the mother; or premature labour has supervened about the seventh month, and saved both the parent and child. These natural processes we imitate successfully in the artificial induction of abortion and premature labour. If, in morbid contraction and deformity of the pelvis, the pregnancy goes on to the full time, nature is still sometimes capable of delivering the mother by other and various measures. Occasionally, during labour, the symphysis pubis has been rent asunder under the intense and wedge-like pressure of the infant's head; or the uterus has lacerated, or, as has happened now in two recorded cases, both the uterus and abdominal parietes have simultaneously ruptured and allowed of the escape of the child through this double opening; or the bones of the child's cranium have become deeply compressed and fractured, so as at last to allow the reduced head to pass; or the same has been effected by the infant dying, putrefying, and at last its scalp and sutures bursting so as to produce the necessary diminution in the size and dimensions of the encephalon. These several operations of nature are all imitated by art in the respective operations of symphyseotomy, the Cæsarean section, cephalotripsy, and craniotomy. And while art thus adopts the operative principles of nature, she attempts to improve both upon their facility and safety, by selecting an earlier and hence less dangerous period for their performance; and by making the required openings and lesions by cutting instruments, instead of submitting to the chance of their being made by nature by means simply of an enormous and hazardous expenditure of muscular effort and compression on her part.

But, I repeat, does the mechanism of the delivery in Mrs D.'s case suggest any principles for imitation? Let us consider the answer, as it might be varied by the *date* of the pregnancy of the mother; and according as she had reached the periods, *first*, of artificial abortion; or, *secondly*, of premature labour; or, *thirdly*, had already advanced to the full time.

The case shows that through an opening of very small dimensions a child may pass, provided it be in a very compressible state. So far it evidently suggests that the induction of *abortion* at the 4th or 5th month, when the head of the fœtus is still small, soft, and very easily reducible would, as long ago proposed by Cooper, &c., succeed in such extreme deformities in saving the mother from many of the dangers accompanying delivery at a later period of utero-gestation. I have stated above that Mrs D. applied to Mr Wiseman at a time when it was already considered too late to have

recourse either to artificial abortion or premature labour; and further, the high position of the os would probably have rendered either of them impracticable.

The induction of *premature labour* at or about the seventh month would not of course have sufficed with a pelvis of such small dimensions, unless we could modify the operation so as both to produce the death of the child, and *retain* it in utero, to allow of its structures becoming dissolved and disintegrated before labour at last supervened. Now, we have no known means of inducing that diseased state of the placenta which produced the attendant emaciation and death in the case of Mrs D.'s infant; nor am I acquainted with any measures which would destroy the life of the child in the later months, without superinducing labour. The retention, however, in utero of the infant, and its putrefaction would be as necessary for success, as its death, under such great degrees of contraction. And the result shows, that when the pelvis is much deformed, and labour with a *dead* child is threatened, or the infant is destroyed by craniotomy, the longer we can retain it in utero without danger to the mother, and the more it thus becomes putrefied and disintegrated, the easier will its ultimate expulsion or extraction prove.

Lastly, suppose a patient with a very diminished and deformed pelvis to have arrived at the *full time* of utero-gestation, does Mrs D.'s case suggest any new principles or modifications of treatment for the delivery of the mother? I believe that, under these circumstances, our conduct and practice should be in a great degree regulated by the state of the child. If it be *alive*—as ascertained by auscultation, &c., and the pelvis is as small as in Mrs D.'s case, or even half an inch larger in its measurements, then I am decidedly of opinion that it is our duty to perform the Cæsarean section. We have two human lives committed to our charge, and it is our duty to try to preserve both, provided we can attain that object without subjecting the mother to a degree of danger much greater than she would otherwise undergo. Let us take, however, the other alternative, and suppose the child already *dead*. With this complication most British accoucheurs would attempt delivery by craniotomy, if the dimensions of the pelvis permitted at all of the possibility of it. And the case of Mrs D. seems to me to suggest one means of rendering it thus possible, under states of contraction, where it is at present properly regarded as in the highest degree unsafe or totally impracticable. The grand obstacle to the delivery of the child by embryulsio, in greatly contracted pelves, arises from our want of means of reducing the size, or altering the shape and compressibility of the bones of the base of the skull and face. In Mrs D.'s child, nature had disintegrated and separated these bones, removed in fact this obstacle, and thus rendered the delivery not only possible but easy. Could we imitate or induce this same fortunate result by artificial means? All our present

means of reducing the size of the foetal head in embryulsio are limited to the destruction of the arch of the skull. I am not aware that with any proposed form of osteotomist¹ we are capable of cutting or disintegrating the base of the cranium or face when the pelvis is diminished to $1\frac{3}{4}$ or $1\frac{1}{2}$ inches in its shortest diameter. Yet, probably, some modification of mechanical means would give us the power of effecting this desirable object. The common perforator might enable us to loosen and break up the bones of the basis in some cases, in the same way as with it we break up the arch of the skull. The disjunction or fracture of these bones, without their removal, might prove sufficient to permit the required degree of compressibility and alteration of shape. Or the common bone-forceps of the surgeon, or a modification of such powerful pliers as are used in dividing the needles in the operation of harelip, &c., might answer. At all events, the object seems anything but a hopeless one, more especially when we call to recollection that modern surgeons are now provided with mechanical means which sometimes enable them to seize, break up, and extract from the cavity of the bladder large and solid stones,—and that too through a canal relatively so small and elongated as the male urethra.

ARTICLE III.—*Contributions to the Pathology and Treatment of the Scorbutus, which is at present prevalent in various parts of Scotland.*
By CHARLES RITCHIE, M.D., one of the Physicians of the Royal Infirmary of Glasgow.

(Read to the Medico-Chirurgical Society of Glasgow, May 13, 1847.)

THE following observations appear under serious disadvantages. The disease of which they treat had attracted attention in the Glasgow Infirmary for a few weeks only previous to their being written out; and they were prepared at the request of the secretary of the Medico-Chirurgical Society at ten days' notice, and, mainly, to fill up an unforeseen chasm which had occurred in the line of the Society's essays. It need scarce be said how ill-fitted they must therefore be to bear comparison with the matured and scientific

¹ The Kephalepsalis of Dr Campbell appears to me to be an instrument preferable to, and more powerful than any of the forms of osteotomist invented by Dr Davis or others. Yet, as we have already seen, Dr Campbell believes embryulsio to be impracticable, even with its assistance, in a pelvis less than 3 by 2 inches. It is almost unnecessary to add that the French operation of Cephalotripsy is founded on the idea of crushing and compressing the bones of the base as well as of the circle of the cranium. It is used by some of the leading accoucheurs of Paris, instead of craniotomy, particularly in cases where the child is dead, and the pelvis below the dimensions that would admit of the use of the forceps. But the instrument with which the operation is performed—the Cephalotribe, is of such enormous dimensions (its blades being of solid iron $1\frac{1}{4}$ inch wide, the whole above 2 feet long and several pounds in weight) that it could not of course be applied in cases in which the pelvis was contracted to any extreme degree.

papers on the same subject, of the distinguished writer who, in the present and former number of this Journal, has given an account of the disease, as it prevailed during the last autumn and the present spring, in the north and east of Scotland. The reflection, that a faithful transcript of the affection by another observer, and in other aspects, might not, however, be wholly destitute of interest, has led me despite the inducements to suppress it, to offer it now as my contribution to the history of a disease seldom seen by the profession, and which my position has happened to give me facilities for observing.

The general features of the medical sick applying for admission at the Glasgow Infirmary this winter, have been peculiar. Their number, especially that of males, has much exceeded the average, and the severe forms of disease, the increased necessity for stimulus and nourishment in their cure, their more lengthened continuance, imperfect convalescence, and greater mortality than in former seasons, have all attracted notice. Gastro-hepatic, or bilious relapsing fever, enteric fever, or dothinerteritis, and typhus, have each been extensively epidemic, and the lesion of the vital powers in them all,—both within and without the hospital,—has been more than common. In the medical wards under my care, the rarity of sthenic inflammatory disease was remarkable, and even the pneumonic and pericarditic inflammations which we received, were of a marked cachectic character. They were often associated with albumenuria, and distinguished by an abnormal, insidious, rapid, and fatal course. The rheumatic cases were of a muscular and neuralgic kind rather than articular. They were attended by painful effusions under the fasciæ, neither did they admit of the active remedial means found beneficial in former years, and, more generally than is even their nature, and, as it seemed, in consequence of an increased irritability in the systems of the patients, they resisted the treatment best suited for them. From the earliest months of winter, there was an unusual prevalence of diarrhœa and dysentery. These were sometimes united with painful, swollen, desquamating legs, sallow, sunken countenance, systolic aortic souffle, and excessive debility. They did not yield to the combination of antiphlogistic, anodyne, and astringent treatment so suitable in general; nor, indeed, did they bear depletion at all; the mouth was very quickly affected by mercury, and in the employment of other measures, they demanded a much more patient, varied, and careful management than is common. They were intractable as symptoms, but especially so, as engrafted on a condition often of appalling cachexia, which long survived the cure of the intestinal discharge, and but too frequently failed to be removed within the short period for treatment, which our limited accommodation permits; and, when death occurred, the larger intestines, instead of the thickened, indurated, mahogany coloured, and sphacelating state of dysentery in the healthy subject, had their coats little altered in thickness, but deeply stained with a blue or

violet hue, from congestion of blood; at times blood was poured out into their cavity, and their inner surface was sprinkled with solitary glands in a state of enlargement or ulceration, and often of a deep black colour. I am now persuaded that the general pathological nature of these cases, as also that of some of the instances of rheumatism, and of numerous examples of cachexia succeeding a series of relapses of gastro-hepatic fever, were essentially identical with that of the scorbutus. It has become evident, besides, on a review of the whole admissions since November last, in the light of our present experience, that a few patients with real scorbutus came into the infirmary throughout the earlier months of winter. These had been regarded as cases of purpura, an affection which every one meets with as a sporadic disease in broken or cold weather, and which is seen sometimes, as in the present epidemic, combined with severe stomatitis and ptyalism, and, again, in its milder forms without either; but it was not till the middle of February, that we began to be aware of the prevalence among our land population of a distinct disease presenting the precise features, etiology, proximate nature, and general indications of treatment of the scorbutus of our navies.

The total number of well marked instances of the complaint, which have been admitted to the medical wards of the infirmary down to the 31st of May, is 83, of which 70 have been males and 13 females; besides which, 39 cases, or 33 males, and 6 females, have been prescribed for by Mr Lyon at the dispensary of the hospital. Of these, 9 cases have been under twenty years of age; 31 cases have been from 20 to 30 years of age; 25 have been from 30 to 40 years; 18, from 40 to 50; and 9 cases have been above this age. 57 lived mainly on bread and coffee or tea, porridge or brose of oaten, or of pease-meal, without either animal food or vegetables, and were underfed, most of them, even in what they did eat; being also exposed all of them to cold and dampness; 27 had the same food with occasional meals of ham or other butcher meat, but were entirely deprived of fresh vegetables, and were also to a greater or less extent exposed to cold and dampness; the food of 3 is not stated, and that of the dispensary patients has rarely been ascertained. One patient was in better circumstances, and had had better food, but he had been affected with spermatorrhœa for at least a couple of years, and laboured also under hemiplegia and extensive subcutaneous melanosis. The amount of fresh vegetables, which he had had, was that contained in the broth, which he said he had used several times a week. Another man aged twenty-one,—who, previous to coming to Scotland, had lived on oatmeal porridge and butter-milk, for breakfast; potatoes, eggs, and butter-milk for dinner (except on Sundays when he had pork-ham, and very rarely fresh meat and some vegetables); and who, for supper, had been accustomed again to take as at breakfast—continued for months previous to becoming affected with scurvy,

but while exposed to great labour, and constant wet and cold at the bottom of the open shaft of a mine, to consume as ascertained on good evidence, every week, about 6 lbs. of fresh butcher meat, $1\frac{1}{2}$ lbs. of cheese, 1 stone of wheaten flour, 1 peck of oatmeal, 4 half-quarter loaves, 2 lbs. of decorticated barley, $1\frac{1}{2}$ lbs. of fresh butter, $1\frac{1}{2}$ lbs. of sugar, 2 ounces of tea, and one penny worth of fresh vegetables, and sometimes a little porter. A second patient consumed 5 lbs. of cheese per week, and had fresh meat three times a week, besides butter-milk and porridge. A third had sweet milk and porridge morning and evening three times a-week; the other days, butter-milk, treacle or beer; cheese and bread or butter, and, once a week, animal broth for dinner. A fourth had either milk or animal food for dinner daily, and, once a week, some fresh vegetables. Several had had oatmeal porridge with milk morning and evening, and sometimes salt meat and bread daily to dinner. Others had, on occasions, porter instead of milk; and one man breakfasted and supped daily on porridge and milk, and dined either on milk and bread, or about four times a week, on fresh butcher meat; but, notwithstanding these apparent anomalies, the general fact in regard to the food of all was, *that it failed in variety, and in the quantity of its animal constituents*, and that, in all but a fraction of the cases, in which they were very deficient, *the patients had been exposed for months to a total deprivation of fresh succulent vegetables*.

I shall now speak shortly of the disease under the divisions respectively of its general symptoms, necroscopical appearances, its causes, and treatment. And first of its *general symptoms*. These were such as instantly served to suggest the idea of a lowered state of health, amounting often to a condition of absolute cachexia; the aspect was depressed and exhausted; there was a sallow, lemon-coloured, or leaden complexion, with sinking of the eyes, which were sometimes of a glossy whiteness, at others icteroid, and often encircled by a dark areola; the nose was pinched, the lips, inner surface of the cheeks, and the fauces and tongue were blanched, except where the latter were stained by ecchymoses, or occupied by ulcerations or chops; the breath was offensive, the gums were livid, boggy, or spongy, notched deeply into conical processes between each pair of teeth, and swollen out often to a great breadth from the edge of the jaw, their connexion with the whole circumference of individual teeth, being in general quite dissolved, leaving a gaping, flat honey-comb-like structure, in which these appeared as if loosely stuck, and which oozed out blood, more or less freely on the application of pressure. The patients were found either with decided emaciation, or greatly swollen; and sometimes the arms and trunk might be observed covered with large folds of loose unhealthy skin, while the lower limbs were distended by swelling. In a few instances this tumidity was œdematous, but in general it was of a rocky hardness, the skin was indurated and thickened, every structure of the limb appearing as if impacted,

and matted or glued together into a brawny solid mass. The skin was of various colours—sometimes of a scarlet red, more usually of a dark copper red, or of either a livid or a yellow, or of a yellowish livid hue; frequently also in the vicinity of variously sized blotches of these colours, brown coloured furunculi, small tumours containing only disorganized blood, rupia-like scabs concealing elevated tubercular ulcerations, the remains of former injuries having an acquired dark aspect, petechiæ, and, often, numerous small circular elevations or stigmata around the bulbs of the cutaneous hairs, of different shades of red, and accompanied by desquamation of the cuticle were observed. The joints and tendons, particularly those of the knees, were the most swollen, indurated, and painful localities. Sometimes the effusion was infiltrated into the cellular tissue around the joints only, and at others the articulations were occupied by it. In a few patients the periosteum covering the front of the tibia was elevated into an indurated, well-defined, and painful swelling, which was sometimes of several inches in extent, and constituted the most prominent symptom; but in the majority of the cases the greatest complaint was of the popliteal spaces and gastrocnemii; muscles. The pain here was often very acute, accompanied by a sense of distressing distention on the patient assuming the erect position; and when the strength was such as to permit of walking, which was in many not the case, this exercise was in general rendered impossible by the pain; or, when this was relieved, by the matting together of the fibres, or the adherence to their sheaths of the tendons of the muscles of the legs.

The mind was sometimes depressed and enfeebled: in two cases there was maniacal excitement, and in another dementia and paralysis. Tinnitus aurium, muscæ volitantes, and vertigo and deafness were occasionally complained of. In some patients it was frontal headache, in others acute hemicrania, and in others again it was either dental or facial pain,—sometimes of an intense kind, which were the most prominent disordered sensations. In a few, again, the most urgent pain was in the neck, from which it shot upwards into the articulation of the lower jaw, or downwards into the arm; while often it was found in the lumbar spine, from whence it extended, in some instances, along the ischiatic nerves; and in certain patients of the latter description, the sensation complained of was less of the nature of pain than of an overpowering feeling of weakness and loss of voluntary motion, and in others, again, an extreme coldness in the same localities.

In regard to the circulatory system, the heart's sounds were usually short and snapping; sometimes they had a ringing, metalloid character; often there was an obvious prolongation of the second sound, and on some occasions, the first sound was either much or quite extinguished. Sometimes there was a soft systolic murmur over the aortic valves, and just as often the diastole was marked by a harsh, rough, and grating noise, which in some in-

stances was heard also in the larger arteries. In many cases there was a loud, humming bruit in the external jugular; sometimes even when the cardiac sounds were good, there was violent throbbing, and a loud whirring noise in both the carotid and subclavian arteries, while in others there was only a short blowing sound, annihilated often on the gentlest pressure of the stethoscope, to be heard in the vessels of the neck. The pulse varied from 60 to 144. It was always easily accelerated, and sometimes it was so weak, that it could not be numbered either at the wrist or inner ankle, and in such cases the systole of an artery even so near to the centre of the circulation as the femoral, was found to be appreciably behind the diastole of the heart. The larger arteries had in some individuals much of the visible impulsive throbbing, which appears to indicate in other circumstances, as in regurgitation through the aortic valves, a want of the due distention of their walls. The peripheral capillaries, also, everywhere unless in the congested portions of the surface, were empty at least of red blood, and both this and the unusual action of the arteries might be seen at the same time with a comparatively shrunk, flattened state of the cutaneous veins. There was often in such, and in the exhausted patients generally, a disposition to syncope on their getting out of bed. The tract of the larger vessels of the limbs, and situations which were most dependent, or which were much exposed to pressure were the most frequent seats of ecchymosis, and the nostrils and gums were the most constant sources of hemorrhages, which sometimes were both frequent and profuse. In one young man the quantity of blood discharged on one occasion from the nose amounted to six ounces. The clot was large, slightly contracted in a portion of its margin, of a dark purple colour all over, except on its most depending part, where it was florid, and when it was turned out from the cup it separated spontaneously into soft but well defined and angular fragments. This specimen was afterwards subjected to analysis by Dr R. D. Thomson, but not before it was too much altered to permit of the precise proportions of its individual organic ingredients being determined. Its general constituents were found to be in 1,000 parts:—

Water,	807·82	} = 1,000.
Solid organic matter,	185·74	
Salts,	6·44	

It was therefore possessed of more water and of less salts than healthy blood; and judging from what was observed by both Professor A. Buchanan and myself on examining other specimens of scorbutic blood by the microscope, there appeared reason to presume a similar diminution in it of the red globules and some relative increase of the fibrine. The blood corpuscles were more irregular in their outline, more flattened in their discs, and more disposed to cohere with one another, and aggregate into large insulated masses, than those of young healthy blood.

In reference to the respiratory system, it is perhaps worth mentioning that of eleven men, whose respiratory powers were tested by Mr Hutchison's spirometer, the quantity of air expelled by one expiration from the lungs averaged on three trials:—

80	Cubic inches in	1	164	Cubic inches in	1
90	"	1	194	"	1
100	"	1	220	"	1
120	"	1	232	"	1
136	"	1	244	"	1
148	"	1			

The disease, as in those of low expiratory capacity in the above list, was found engrafted in some patients on a previous emphysema of the lungs, sometimes associated with phthisis pulmonalis, and at others with slight bronchitis, but it did not appear to have any essential connexion with these affections, unless it were in their relation as predisposing causes. In some there was a very peculiar and complete loss of resonance on percussion over certain portions of the thorax, which, as it disappeared without the aid of local treatment under the use of the general tonic regimen, may reasonably be presumed to have been produced by the effusion of scorbutus among the muscles. In others the dullness was accompanied by acute pain and a pleuritic friction sound; and in a third series there was diminished sonoriety, lancinating pain, and much constitutional disturbance, combined with coarse, moist crepitation in the corresponding lung, and with hæmoptysis.

In respect to the lesions of the digestive system, the tongue was commonly red or violet coloured, moist, and very clean; in other instances it was flabby and bloodless—sometimes it was tender, and in others again it was chopped and disposed to bleed. There was in general some ptyalism, and then the tongue was indented on its sides, and the swelling of the parotids and of the gums, gave the patient the look precisely of a mercurialized person. The saliva was usually acescent. There was often extreme thirst, the appetite was almost uniformly good, and it was not uncommon to see it voracious. The belly was at times enlarged, and either meteoric or obscurely fluctuant. The bowels in a large proportion were slow, but in some individuals, who were exposed to cold and wet before admission, there was diarrhœa; in others this symptom arose after they had been put on a full stimulating diet, and in both of these the abdomen was not rarely found shrunk and flaccid. Sometimes the dejections were pale, or nearly white; but in others they were of a bright orange colour, and attended by bilious vomiting. In the diarrhœas brought on by treatment, the stools passed rapidly into fermentation, and had in this respect, and in their general aspect, a strong resemblance to those of well fed diabetic patients. In some the evacuations were incorporated with blood; many complained of piles, and then the blood lay more on the surface of the excretions.

The urine varied in specific gravity from 10·10 to 10·28. In a few

instances it was reported to have been bloody; in one it was alkaline immediately on being voided, but in no one of the many specimens tested was it found to be albuminous or abnormal in other respects. There was retention in one case. Amenorrhœa took place simultaneously in one woman, with the first symptoms of the disease.

Necroscopical Appearances.—It gives me pleasure to state that there has not yet been a death among my patients by this disease. One old man, who was admitted to another part of the house with scorbutic cachexia and diarrhœa, I saw incidentally both before and after his death, and for the facts of his case I am indebted to Mr Steven.

Patrick Crombie, aged about 70.—Admitted March 5th, 1847, in a state of great destitution and exhaustion; the face stamped with famine, the clothes reduced to a few rags; the skin harsh and filthy; breath fetid.—P. 90. Heart sounds short and sharp; tongue brown, dry, furred and chopped. Much dysenteric purging. Died on seventh day from admission.

Inspection.—Intestinal canal healthy to about three feet from the cæcum, where the bowel began to assume a dark red colour, which was continued down to upper part of rectum. The large intestines contained about from three to four pounds of fluid blood. Many of the solitary glands in lower part of ileum were enlarged, and of a brownish or blackish colour. The mucous lining of the great bowels—of the *intestinum cæcum* in particular—was soft, friable, and delicately roughened; further down, as in the rectum, this state was better marked, and was obviously produced by the abrasion of the internal surface by superficial and minute ulcerations. The colour of the whole was a delicate bluish red.

In this individual the mucous membrane of the large intestines and its connections were in a similar condition to that of the mouth and throat in some of our cases of scorbutus. In another patient, in addition to the intestinal lesions, the lungs had become suddenly infiltrated with blood, just as is often seen in deaths from typhus fever after the twelfth day.

John Donaldson, aged 16, a miner, January 13, 1847.—Swelling of lower limbs and of right arm, preceded for six weeks by diarrhœa, which was sometimes bloody and painful. A few small ulcers on the legs. Dullness of left anterior thorax as low down as nipple, but without other physical sign, and there is no cough or dyspnœa. The dullness on percussion of the precordial region is greater than natural. The heart's action is irregular, and the sounds cannot be heard below the level of the third rib, where, however, they are loud, the first being longer than natural, and harsh. Pulse 60, and very feeble. Urine natural in quantity and acidity. Sp. gr. 1010; but not albuminous. He died on the sixteenth day from admission.

Inspection.—Five ounces of clear serum in pericardium. Heart natural. A little clear serum in each pleura. The whole upper lobe of the left lung was hard, heavy, and much more solid than natural, its section dense, firm, and of a dark red colour. The inferior lobe of the same lung was similarly affected, but in a less degree, and every part of both lobes floated. The right lung was also condensed, but not in a marked degree. Kidneys natural. The lower half of the ileum was occupied by several patches of arborescent congestion, and the Peyerian patches were darker and more distinctly seen than natural. Throughout the colon were several clusters of intensely black ulcers, each of about the size of a split pea, the white muscular coat being seen in the basis of a few of the larger ones. Towards the rectum, and throughout this bowel,

these black ulcers became more numerous, and the intervening portions of mucous lining were much congested and of a bluish colour.

In this patient, besides the pulmonary and intestinal congestion, there was effusion within the pericardium, existing, if we may found on the symptoms, even at his admission.

In another individual, a wretched-looking boy, who was received into the house in an apparently dying state, and never rallied, the only appearances of disease were in the brain.

Marcus Docharty, aged 11 years, a tobacco worker, March 22, 1847. Has never been healthy, and his aspect is cachectic. Has had diarrhœa for a considerable time. About three weeks ago was seized with head-ache, confined to left side and over ear. This still continues. Abdomen tender on pressure. Tongue pale. Heart's action strong. Pulse 150. Says he feels pain deep in the left ear, but not in the right. Is very drowsy. Eyes half-shut; pupils unsteady, mostly dilated, but frequently contracting. Stools are clay-coloured and very fetid. Has a slight cough. Stethoscopic signs of lungs natural. Died on 31st.

Inspection.—Body much emaciated. Nothing particular was observed about the intestines. The gall-bladder was distended with a large quantity of viscid dark coloured bile. A few miliary tubercles were scattered near surface of left lung. On opening the cranium, the dura mater was found of natural appearance. All the sinuses, however, were plugged with coagulated blood, so that they felt like cords under the finger. All the veins on the surface of the brain were of similar appearance. A space of irregular shape, but about $1\frac{1}{2}$ inches in diameter in all directions, at the base of the middle lobe of left hemisphere, corresponding in situation to the petrosal bone of left side, presented a beautiful bright red-colour from infiltration with blood. When cut into, this appearance was found to extend into brain for about an inch at the deepest part, and here it had a punctuated appearance.

A similar appearance was also observed on the upper edge of the middle of right hemisphere, but it was less extensive, and did not go so deep.

In the only other fatal cases which I shall narrate, the symptoms made very rapid progress latterly, and some may be disposed to regard them as being examples of purpura rather than of scorbutus. I will not enter here into this question, or into that of the generic distinction of these affections, but would only remark that instances of the disease which we were wont to term purpura are common at present in Glasgow and elsewhere, mixed up with those less acute cases which we term scorbutus, and that on examining the only two other individuals of which the family consisted, from which Margaret Mungal (the second of the cases which I am about to give) came, I found one of them with swollen, spongy, and painful gums, along with pains in the popliteal spaces.

Henry Hannoway, aged 70, March 9th, 1847, an Irish labourer. Twenty-one days ago, while, according to his own account, in good health, was seized with rigors, muscular pains, and pyrexia. Did not confine himself to bed till three days since, and now appears much attenuated, and complains of general pains, more acute in thorax, epigastrium, and forehead. The face is flushed, the skin moderately hot, and without typhus or enteric eruption; the scrobiculus cordis is tender on pressure, there is slight dry cough, the breath sounds are natural; the heart's action is irregular, and its second sound is long

and predominating. The pulse is 95, strong and full, there is mild delirium, the tongue is moist and white at the edges, but the centre is extremely dry, brown, and chopped, and, *as well as the gums, which are soft and tender, it bleeds often profusely.* This hemorrhage, and the exhaustion occasioned by it, continued the prominent symptoms till the 15th of March (five days after admission), when he died.

Inspection March 16.—Several ecchymosed and yellow spots over skin, and some blood issuing from the anus. The lungs adhered all round to the walls of the thorax by strong connections of long standing. The heart *in situ* was observed to be of great size, and its external surface, especially over the right ventricle, and also various portions of the costal pleura, and the pleura pulmonalis, in particular of the left side, were deeply stained by ecchymosis. The increased size of the heart was made up in the left ventricle, of increased thickness in the walls, which after a few days ceased to be so remarkable, and on the right side it was constituted by dilatation. The whole of the left lung and the lower lobe of the right, were in a state of splenization. The spleen was enlarged and softened. The liver also was increased in size. Its colour resembled that of the spleen, and it was so soft and diffuent, that it could not be suspended by its own structure. The bowels were congested, and Peyer's glands ecchymosed. The bladder of urine was soft and pulpy, but of natural colour.

This man had been a patient of Dr Perry's in the Fever Hospital of the Infirmary, and for an opportunity of examining his heart, and for the facts of his case, I am indebted to Mr Foucart. The last case which I shall refer to was a patient of Dr Easton's, also in the Fever House. I saw the inspection of the body, and had the principal parts of the history supplied to me by Mr Craig.

Margaret Mungall, aged 18, a domestic servant, 20th May, 1847.—Down to about seven months ago, when she removed to Glasgow, patient had suffered from starvation, and, although stout-looking, she complained always of headache and of indigestion, the latter being aggravated by her voracious appetite in her new situation. For the first few months of her service the intervals of her menstruation were about six weeks, subsequently about two weeks only; and her food consisted of tea, bread and butter for breakfast; pea-soup and animal food on two days; broth, made with barley, a little turnip and onions with beef on other two days of every week; the dinner of the remaining days having consisted of barley and milk or an egg; or of either the pea-soup or broth again made warm. For supper she had oatmeal porridge with milk, or tea and bread. Had been engaged with a washing on the 10th inst., and was exposed to cold in a drying-green during the flow of the menses on the 11th. On the 12th she was seized with diarrhoea, and continued to complain of weakness and great pain in the right arm till the 17th, when she remained in bed, and suffered from headache, sickness, and excruciating pain in the epigastrium and breast; general soreness, some vomiting, and frequent syncope, and on the 18th and 19th (yesterday), numerous red patches were observed on the skin. At present complains of languor, drowsiness, and nausea. Epigastrium is tender on pressure. Tongue is covered with a thick but very fine fur of a deep yellow colour, and mucous membrane lining mouth is of a pale bluish yellow hue. Gums are slightly spongy. Does not complain of sore throat. Bowels constipated. Urine is voided in the usual quantity, but of a much darker colour than natural. Face is flushed, and over its surface are several small elevated papules. There are also a few upon hands. Over abdomen are several red but not elevated spots, resembling those usually found in purpura. Has no chest symptoms. Pulse 120 strong and full. Eyes much injected. Is menstruating. Was vaccinated, but does not remember to have had either scarlatina or rubeola.

21st May.—This morning abdomen, chest, and back found covered with a rough elevated efflorescence of a dark scarlet colour. Throughout this are various dark-coloured petechiæ, which do not disappear on pressure—and also numerous very small papules. Over face the papulæ previously mentioned still remain, and there are several on the arms. The legs and feet are covered by a diffuse scarlatinous redness; the uvula is occupied by a large petechial spot, and a little blood has been discharged from the gums. Bowels slow, and there is much languor and drowsiness with great thirst.

22d May.—Patient got very delirious during the night, and about half-past five this morning had a convulsion, which terminated in death, in about two minutes after seizure

Inspection.—Thirty hours after death.

General surface.—Rigidity very slight. The whole trunk covered with a continued purple-coloured ecchymosis, and studded with numerous livid petechiæ. Over epigastrium, where a sinapism had been applied, the skin was of a crimson colour. Thighs presented the same appearance as the body, except that the number of dark bloody spots was less. The legs were marked by numerous petechiæ, and by patches of ecchymosis. Face was also marked with a number of livid spots; around eyes there was a dark purple coloured ring, about one and a half inches in diameter; and the albuginea of both eyes was marked with bright scarlet coloured effusions.

Head.—In the fasciæ of the muscles over parietal and temporal bones were several patches of effused blood, the largest about the size of a shilling, and situated over left parietal bone. On removing skull-cap, numerous purple and scarlet-coloured spots were observed shining through the dura mater and arachnoid, being situated in the pia mater. There was some serous effusion between the laminæ of the arachnoid membrane, but the brain itself was healthy. It did not seem to be more vascular than usual, nor was there any effusion into its cavities.

Thorax.—The pleura both on the lungs and walls was sprinkled thickly with crimson-coloured, and with florid and livid spots; some of which, especially on the diaphragm, were of the size of a sixpence, and others as large as a crown-piece. There was no effusion into the pleura, and about two drachms only into the pericardium. Both lungs were gorged with a frothy sanguinous fluid, and presented a dark-red appearance when cut. The bronchial membrane was of a red colour, and marked by numerous dark, purple stigmata. In the bronchial glands were several ecchymoses. Inner surface of pericardium mottled by about a dozen circular livid stains, each about half an inch in diameter, and formed by black fluid blood. Cavities of heart were filled with thin dark-coloured liquid blood. Endocardium much dyed, particularly over valves; in which situation was a livid ecchymosis of the size of a split pea. Heart healthy. Lining membrane of thoracic, and abdominal aorta of a bright vermilion colour, becoming fainter as the vessel descended; and the inner surface of the vena cava was marked with numerous red spots.

Abdomen.—Peritoneum speckled by many purple-coloured spots in every part. The mucous membrane of the mouth was of a light-blue colour. The fauces and inner surface of œsophagus were the seat of dark patches, and more minute red points extending as far as the stomach, the mucous coat of which was of a bright vermilion colour, and presented the appearance produced by some corrosive poisons, without the abrasion of its tissue. Small intestines were internally, of a pale red colour; but the colon for about 26 inches from the ilio-cæcal valve presented something of the aspect which might be occasioned by a quantity of ink falling from a height on a white, moist surface. The remaining part of the colon and the rectum were the seat of similar appearances. Liver externally very pale, and at the centre of the round ligament, there was an ecchymosed patch about half an inch in diameter, and several others over the surface of the organ. Its substance was of a pale, dirty straw-colour, and so soft that it broke into a paste on being handled. Gall bladder filled with black bile. The pancreas externally, both kidneys, inside

of ureters, and lining membrane of bladder, all more or less occupied by ecchymoses; and the uterus contained some blood. The fasciæ covering the muscles were also marked with red and purple-coloured spots; and the blood throughout the body, was dark-coloured, unusually fluid, and very thin.

In some of the scorbutic patients admitted in the present epidemic, the infiltration of the legs by fibrinous or other effusion, and the consequent tension of the fasciæ and obstruction to the circulation, have been so great, as to suggest the occurrence of gangrene; and it is not improbable that should we lose any by the disease, such may be one of the modes in which it will happen. At this moment there is a young man in one of the wards under my care with the scorbutus in his legs, and a cavity in one of his lungs.

(To be continued.)

Part Second.

REVIEWS.

Observations on the Treatment of Lateral Curvature of the Spine, pointing out the Advantages to be gained by placing the Body in a position to produce Lateral Flexion of the Vertebral Column, combined with the after application of firm Mechanical support; with Wood Cuts. By EDWARD T. LONSDALE, Fellow of the Royal College of Surgeons, &c. &c. 8vo. pp. 116. London: 1847.

WE shall endeavour in as few words as possible to put our readers in possession of the plan here proposed by Mr Lonsdale for the treatment of lateral curvatures of the spine. Under the head of general principles applicable to the treatment of such curvatures he says—"First, there is weakness of the vertebral column; it has then to be artificially supported. Second, there is displacement of certain bones; the vertebræ, the ribs, scapulæ, and clavicles, which lose their relative position to one another, at the same time that the ligaments on one side become shortened; these bones then have to be replaced, and the resistance of the ligaments has to be overcome. Finally, there is irregular muscular development of the two sides of the body, existing both as a cause and as an effect." He goes on to say that in his opinion all these three points of attention must be combined.

His first object then is to explain the means by which the spine can be well and efficiently supported. The intent of all spinal supports is, or ought to be, to raise the weak and depressed side of the thorax, and to keep up gradual and continued pressure on

the projecting or displaced parts; to combine mechanical pressure with mechanical support. We quote the following passage entire :—

“ The first point to look to, is to make a foundation which shall be strong enough for the rest of the apparatus to be attached, at the same time that it can be so applied and to such a part of the trunk as to be immovable. The most convenient shape for this part of the apparatus, is a broad hoop, and the most convenient part for its application, is the pelvis, making it grasp the broad portion midway between the crest of the ilium and the trochanter of the femur. It should be so shaped as to fit the pelvis *accurately*, to press equally and to lie in close contact at all points; it should be evenly padded, to guard against irritation of the skin or unequal pressure on the parts beneath. The importance of attending to these points must be evident, when it is considered, that upon this foundation the superstructure of the rest of the apparatus has to be built; if it be not then strongly made, and firmly fixed, all action on the spine above will be of no avail.”—P. 56.

His next object, namely, the support of the depressed side of the trunk, is attained by adapting a crutch to the most convenient part of the foundation pelvic hoop, first for the support, and then for the elevation of the depressed or sunken shoulder, so as to relieve the compressed ribs, and thereby remove one of the causes tending greatly to increase the curvature of the spine. As regards the third object, we give the following statement in the author's own words :—

“ To carry out the latter principle, namely, that of redressing the curve of the spine, pressure must be applied by means of a broad plate, evenly padded and shaped so as to grasp the projecting ribs and scapula, laterally as well as posteriorly, and so attached, that it will admit of being mechanically moved in the direction that will tend mostly to overcome the existing convexity; for, as already stated, the nature of the deformity is to produce increased convexity of the right side, and a corresponding concavity of the left. The kind of force requisite, and the direction of its application, are well illustrated in the following manner. Stand behind a patient with simple lateral curvature to the right side, place the left hand beneath the axilla of the left shoulder, and press the palm of the right hand against the projecting ribs of the right side of the chest; raise the two hands forcibly upwards at the same time, and in the large majority of slight cases it will be found that the deformity can be temporarily overcome, and the patient will at the same time express the great relief she will feel while this pressure is being so continued.”—Pp. 57, 58.

Mr Lonsdale next enters on a particular description of his apparatus, which is illustrated by cuts. Here we must refer to the work itself for particulars. We may add, however, that his pelvic hoop is of steel, from two inches and a half to three inches deep, for adults, covered with leather, and on the inner surface evenly padded; to render its application more easy, there is a hinge at either end, in front, at a short distance from the free extremities, and when applied, one of these free extremities slips into the other, and is fixed by a strap, which can be tightened or slackened at pleasure. From the posterior middle part of the pelvic hoop rises, at first vertically, and then curving off to the left side, a steel arm, ending in an upright forked crutch, for the support of the shoulder, and this arm may be shortened or elongated at plea-

sure, by means of a slide fixed by a screw. The broad curved steel plate pressed against the right side of the chest to redress the curvature of the spine, is attached by its posterior edge to a long lever, which being joined by a screw to the middle point of the vertical bar of the crutch, has there its fulcrum, and thence extends to the posterior surface of the pelvic hoop, on which it works by means of a screw and key, so that on being drawn at this lower end, towards the right side, its upper end, and with it the attached plate, is pressed strongly against the projecting ribs of the same side. Such then are a few points as to Mr Lonsdale's views of the kind of apparatus proper for lateral curvatures of the spine. It should be added that he does not claim for himself the merit of entire originality in the invention; all that he claims being an improvement on methods before followed, and particularly on an instrument employed by Mr Tamplin, at the Royal Orthopædic Hospital.

We confess we entertain a great jealousy of all such mechanical contrivances for the treatment of curvatures of the spine, considering their usual effect to be not to remedy the disease, but to make matters worse, and we should like to see better evidence of the benefits of Mr Lonsdale's plan, before we yield it our approbation. To do the author justice, however, he does not attach any efficacy to his plan, without the combination with it, of the means universally approved of for the treatment of such affections, namely, the recumbent posture, and exercise of the muscles. To each of these accessory methods of treatment, he devotes a separate section of his work.

Mr Lonsdale insists on the advantage of placing the patient on her side, rather than in the supine or prone position—on this point we cite the following passage:—

"I recommend that the patient should be placed on the side, on which the projection formed by the curve exists, instead of on the back, and allow the legs, the head, and upper extremities, to fall to a lower level than the trunk; by this means a sufficient power is at once gained, by the simple weight that is then exerted at either end of the trunk, to gradually act upon the spine and to regulate itself; imitating, in fact, the straightening of a bent rod or stick: no other mechanical means are required; the weight of the legs at the one extremity, and of the head and shoulders at the other, exert a force quite sufficient to redress any slight curvature that may exist, and as much as can be borne, or it may be judicious to apply in severer cases. The object is to stretch the ligaments, and so to overcome their resistance, at the same time that the bones themselves are pressed in a direction the opposite to that in which they have been displaced, and are thus rendered more moveable and more capable of being acted upon, by any apparatus that may be afterwards employed to give them support,—points to be more particularly referred to, and which I shall explain after describing the means by which this position is to be obtained."—Page 84.

"When the patient is placed upon the couch, the body is supported by a sling made of a broad belt, 6 or 8 inches wide, attached by a strong strap and buckle on either side to a perpendicular framework or yoke thrown across the couch, and fitting into it, nearer to the upper than the lower end, that it may be opposite to the thorax when the patient is placed within it. The belt admits of being raised or lowered, and can be made to act upon the curve of the

spine, by the straps or buckles with which it is attached to the perpendicular framework just described."—Page 86.

We cannot attempt a more particular account of the couch which our author recommends; in the work itself, there is a woodcut of the couch, showing the position of the patient, the supports, &c. We are not prepared to give a decided opinion on the effect of this attitude, but we cannot help thinking that Mr L.'s observations on this head deserve a careful consideration, and his precepts a fair trial.

Under the head of exercise of the muscles, he lays down some considerable limitations on received practices. On this head we extract the following passages:—

"I have already stated, that I believe the muscles can only be exercised with advantage in those cases where the spine has been brought by mechanical treatment nearly, if not quite, into the erect position; or in those cases where, from the age of the patient, or from the short duration of the curvature, the spine admits of being easily brought into its natural erect line with the aid of very slight pressure. Strengthen the muscles in these cases, and you will tend greatly to prevent the curvature of the spine increasing, and in many cases will be able to remove the deformity altogether." . . .

"The position that I believe to be the best, is one that throws the whole spine more backwards than forwards; which tends to redress the curvature, at the same time that the muscles of the spine are brought actively into play; and the following is the one I should recommend. Attach two pullies or hooks (and pullies answer the purpose better) to the ceiling of the room, or to an artificial frame-work placed in some situation about two or three feet above the head. The patient is to stand in a position, that the pullies may be about a foot and a half or two feet behind her. She is then with both hands to take hold of a stick or spindle, to which two ropes are attached, and which pass through the pullies, having weights fastened at the other ends sufficiently heavy to require some exertion to draw them up, the weights of course being increased or diminished according to the strength of the patient. I generally find six or eight pounds in each quite enough, and as much as the patient can raise without over-fatiguing herself. The ropes should be long enough to allow her to incline the body forwards on the hip-joints, without bending the spine itself, drawing the weights upwards as much as she can, keeping the arms extended above the head all the time, and bringing them as far forwards as the inclination of the body will admit of, without the necessity of moving the feet from the position in which they were originally placed. The body is then to be brought into the erect position again, by raising the trunk on the hip-joints, and letting the weights fall, and so to pull the arms behind and above the head. It may be as well to tie a knot in the ropes, to check the fall of the weights, to prevent the arms being strained beyond the point of extension to which they can easily be carried behind the head.

"This exercise may be repeated for a certain time daily, and as often as the patient can conveniently do so; the object being, not to fatigue the muscles, but by degrees to strengthen them; at the same time, that the ribs and spinal column are brought into a position the reverse to that into which the curvature has a disposition to throw them."—P. 107, 8, and 9.

We have thus given our readers some data for judging of the merits of Mr Lonsdale's work. Of new plans for the treatment of curvatures of the spine, the profession is justly jealous; and we are sorry to say there has been too much room for jealousy; for, not to

speak of the quackery which has so often lurked under this title, we have to be on our guard against the crotchets which the most sincerely upright cannot help indulging in, when they chance to take up a subject of this kind, and at the same time have the misfortune to be of a mechanical turn. We have, however, found no reason to suspect Mr Lonsdale of falling within this description, and we trust that the experience of the profession will confirm not a few of the inferences which he has made in this very important department of practice.

Monographie über den Einfluss der Gase auf die form der Blatkörperchen von Rana Temporaria, &c.

Monography on the Influence of Gases upon the form of the Blood-Corpuscles of the Common Frog; with two Copperplates. By Dr EMIL HARLESS. Erlangen: 1846.

THE subject of which our author here treats has not hitherto lain uncultivated. The most cursory insight into the labours of the last few years furnishes ample proof that, among microscopical and chemical inquiries, the importance of this particular kind of investigation has not been undervalued. The ordinary fault hitherto chargeable against experiments in this department, lies in the manner of the investigation. "In most of the trials before made," as our author justly observes, "the blood was agitated with one of the respective gases, and a drop of it covered after it had been spread with pains over a slip of glass. But when the blood is treated in this manner, the atmospheric air is not at all excluded from exercising its power of altering and modifying the shape of the blood corpuscles." It was to obviate this objection that Harless was led to undertake a series of trials, in which he succeeded in constructing an apparatus every way fitted to the object in view. For a description of the apparatus in question we must refer to the work itself, contenting ourselves with a statement of his results, as alone suited to the practical character of our pages.

In the first place, then, among the gases, the agency of which our author has investigated, there are some by which the blood-corpuscles are entirely destroyed, and others by which changes only of such a kind are effected as are removable by the aid of other gases, the absolute destruction by the second kind taking place but under limited circumstances. To the first head belong Chlorine, Sulphureted Hydrogen (S H), Phosphoreted Hydrogen (P H^2), Cyanogen and Sub-oxide of Nitrogen ($\text{N}^2 \text{O}$); to the latter, Oxygen, Carbonic Acid Gas, Hydrogen, and Oxide of Nitrogen (N O). Secondly, the individual blood-corpuscles of the same blood do not all undergo alteration at the same moment. In all probability, those of more recent formation undergo alteration more rapidly than those which are older. Thirdly, the changes

effected by the gases differ considerably according to the succession in which the blood is subjected to them. A striking instance of such a result is obtained in the alternative action of oxygen and carbonic acid gas. Every one is aware of the effect produced upon the blood-corpuscles by these gases respectively when each acts separately, and in such a case the current of each may be in contact with the blood-corpuscles for half an hour without occasioning their destruction. The contrary, however, is observed when the current of each of these two gases is interrupted every fourth minute by the other; for in such a case the oxygen at its sixth, or at most at its ninth application, is no longer able to produce its former effect, the envelopes of the blood-corpuscles being already completely dissolved by the carbonic acid.

“The question, then, as to the manner in which the blood-corpuscles of the living body give way, hitherto quite unanswered, thus obtains a very decided solution, namely, in the alternating agency of the carbonic acid and oxygen. The more rapid the alternation of these gases (the more rapid the circulation), the more rapid must also be the dissolution of the blood-corpuscles; and it must, therefore, be more rapid in the living organism of warm-blooded animals than in the stagnant blood, or in that of cold-blooded animals. It must, however, be remarked as regards the living body, that these gases are never in it applied to the blood in the same state of purity in which they were employed in the experiments, and hence that their action must be less rapid and energetic. Yet these experiments afford an explanation why any retardation or acceleration of the circulation, or a less than normal supply of oxygen, or of carbonic acid, must be detrimental to the animal economy.”

4thly, As regards the manner in which the size of the blood-corpuscles, is changed by the action of oxygen, or of carbonic acid, Harless disputes the existence of vital contractility in their envelopes, because these are affected neither by cold nor heat. Nor can the difference in size be dependent on thickening induced by coagulation and the like, or by attenuation of the envelopes, as but few gases give rise to coagulation, viz. chlorine, sulphurous acid, and phosphoreted hydrogen; and, moreover, the coagulation once produced, cannot be again resolved. Hence, and from repeated observation, Harless feels confident that the variation of size following the application of oxygen, carbonic acid, and hydrogen, results from the formation of folds. “The supposition is easy, that vesicles with somewhat elastic walls filled with and surrounded by fluids which maintain an endosmotic or exosmotic relation to each other should show contractions in their walls at some points of their surface as soon as they have become in any degree emptied. The more the inner fluid escapes, the more numerous will be the folds, which running across each other, and more and more pressed together by the elasticity of the envelopes at last fail

to be recognised, the many little folds occasioning the appearance of turbidness in the envelopes. When on the other hand, the vesicle becomes gradually filled, the closer packed and imperceptible folds expand by little and little, and these being thus obliterated, the envelope becomes smooth, having re-acquired its former dimensions. The variation of size is not the immediate effect of the action of the gas on the blood-corpuscles, but of the altered endosmotic process. Further, neither albumen nor hæmatin undergoes any such rapid change by the chemical action of oxygen or carbonic acid, as to give rise to the altered condition of the fluids within and without the vesicles, necessary for the just mentioned modification of the endosmotic process. For an explanation of the necessary change, therefore, we must resort to the supposition of a difference having arisen in the state of the gases within and without the blood-corpuscles."

Part Third.

PERISCOPE.

PHYSIOLOGY.

REMARKS ON THE MOVEMENT OF THE IRIS. By SCHUR.

EVER since the law of reflex motion was discovered, the motor innervation of the iris has been considered as falling chiefly, if not exclusively, under that law. This assumption derived probability from the correspondence between the amount of the stimulus of light and the extent of motion in the iris. The contraction of the iris at the moment of its being directed inwards and upwards, a fact first observed by Müller, was looked upon as a slight exception to the law, an evidence of the iris being not entirely excluded from the law of associated motion. Again, the dilatation and contraction of the pupil, while the intensity of light remains the same, according as near or distant objects are looked at, the pupillary movement partaking in the accommodating power of the eye, the contraction of the pupil subsequent to irritation of the conjunctiva, as also in neuralgic affections of different facial nerves and other facts in favour of the associated character of the pupillary movement, were entirely neglected. Lastly, the statement of older authorities regarding the continuance at times of the motion of the pupil in amaurotic eyes, was explained away by the supposition that the retina in such cases might still retain enough of sensibility for the excitation of reflex movement, or that the pupil of a diseased eye might obey the stimulus of light falling on the retina of a sound eye. A case bearing on these points has recently occurred in the practice of Schur; and to obviate all difficulties, he has given an exact account of it. The following are its chief particulars:—

The patient, forty years of age, was attacked for the first time in 1836 by general convulsions, the intervals between the attacks being gradually diminished till the autumn of 1841, when there hardly passed a day without a fit. The fit consisted in tonic contractions of the flexor muscles of the fore-

arm, hand, and fingers, of the muscles of the right side of the throat and neck, in tetanic stiffness and transitory convulsion of the right leg, in quivering movements of the facial muscles, in spastic closure of the rima glottidis with threatening of suffocation, and lastly, as happened thrice in the course of 1842, in a state of unconsciousness, with stertor, apparently the forerunner of apoplectic death. Before and during the ordinary attack, the patient experienced a very intense aching, not increased by pressure, at the right side of the head below the parietal and temporal bones. In April 1841 the patient first observed an impairment of vision, the progress of the affection being so rapid as to pass into perfect blindness before the end of six months. At the time of examination the upper eyelid hung relaxed over the eye, covering the upper edge of the pupil, while it could hardly be elevated. The eye itself was fixed. The movements of the eyeballs, slow and difficult, were still in correspondence with one another, with the exception of their convergent motion, which was altogether lost. The pupils themselves appeared considerably dilated, of cloudy aspect, and showed no sign of motion whatever under direct or transmitted sunlight, or during the movements of the eyelids and eyeballs, consequent on the irritation of the conjunctiva, by means of a pen or otherwise. Nor had the attack any effect on the pupils or the eye-lids, the eyeballs alone being frequently rolled to the right side. As to the rest, the patient had incoherency of thought, his spirits impaired, his face had lost all expression of intelligence; the movements of mastication were slow and ungainly, the tongue was put out with difficulty, the articulation of words indistinct, the motion of the right extremities less energetic than those of the left side.

As regarded the seat of this disease, the periodical convulsion of the right side, without any cerebral symptoms whatever during the intermissions, with the exception of the paralysis of both optic nerves, suggested that it was in the motoreal cerebral apparatus of the left side, especially in the neighbourhood of the thalamus opticus or chiasma. The nature of the disease next engaged attention; for though the existence of an adventitious formation would forbid any hope from treatment, the presence of a congestive, hemorrhagic, or inflammatory process, would render the prognosis less desperate. Schur inclined to the latter alternative on the following grounds:—The first convulsive attack had occurred after powerful exertions continued for a few days, and the patient being of a plethoric constitution, was disposed to cerebral congestion; and as the fits were followed by perfect intermissions, there was no ground for assuming the existence of a deep lesion of structure; lastly, hyperemic and hemorrhagic conditions are of far more frequent occurrence in the cerebrum than adventitious formations. He therefore prescribed venesection to ten ounces, cupping between the shoulders every fortnight for three months, a plaster of tartar emetic to the neck, and a course of purgatives, beginning with more active cathartics, and passing from these to a mixture of rhubarb and flowers of arnica. After a few months of this treatment, the convulsive attacks, and the paralytic affections of the tongue, face, and extremities, disappeared, the eyes alone remaining in the former deplorable condition.

About three years after this improvement, Schur was struck when examining the patient with the facility and rapidity of movement now exhibited in the upper eye-lids, while even the pupils appeared of far less magnitude, and again normal in their mobility. Notwithstanding this amelioration, the patient could not distinguish light, nor yet were the motions of the pupils in any way influenced by its stimulus, or by irritation of the conjunctiva, the *membrana Schneideriana*, or the lachrymal points. It was when the patient moved the eye-balls or eye-lids that motion of the pupils instantly took place, with this difference, that the movement of the eye-balls was commonly followed by uncertain and less lively action of the iris,—the motion consisting sometimes in feeble dilatation, sometimes in feeble contraction; whereas the opening of the upper eye-lids was accompanied constantly with a very considerable and vivacious contraction of the pupils. Whether the closing of the eye-lids was attended with dilatation of the pupils, Schur

did not succeed in discovering, though it is rendered probable by the circumstance that, on the commencement of their opening, the pupils were always seen to pass from dilatation to contraction.

Remarks.—The observation of Müller that the contraction of the rectus internus and rectus superior is associated with the contraction of the pupil, has been already referred to. Now the case before us shows a greater extent of association in the motions of the pupils, namely, the motion of the previously motionless pupils, in association with the action of the muscles, raising the eye-lid, as well as in that of certain muscles of the eye-ball. Thus the following conclusions result: 1st, Contemporaneously with the innervation of the levator palpebræ superioris there takes place a motorial excitation of the circular fibres of the margo pupillaris: 2d, Contemporaneously with the innervation of the orbicularis palpebrarum there takes place a motorial excitation of the longitudinal fibres of the iris, which arising from the margo ciliaris of the iris, run to its pupillar border: 3d, The contraction of the recti and oblique muscles of the eye-ball gives rise to a more feeble and indefinite action of the pupillary fibres, the contraction being excited sometimes in the circular, sometimes in the longitudinal fibres of the pupillary curtain. Schur considers the two first conclusions as demonstrated facts, corroborating their truth by observing that the protection afforded by the contraction of the pupil against the incident light is of far more use at the first opening of the eye than after the light has become diminished in its stimulus by being freely absorbed. Of the third conclusion the evidence falls much short of demonstration; for on the one hand the action of the muscles of the eye-ball could not be considered as normal, the patient being unable to exercise these motions with exactness or to direct the eye-balls inwards; on the other hand, the rolling of the eye-balls being almost constantly accompanied with a feeble movement of the upper eye-lids, it could not be ascertained whether the excitation of the pupillary fibres was in association with the action of the eye-ball or of the eye-lids. These remarks, however, must not be mistaken for a denial of the reflectory motions excited by light in the healthy pupil, they go no further than to a restriction of the views hitherto prevalent. According to Schur the associated and reflectory movements are severally exerted as follows:—On opening the eye to moderate daylight, we observe in the first instant a quick and considerable contraction of the pupil, this is followed in the next instant by a slight wavering of the diameter or an alternate contraction and dilatation of the pupil; in the last instant when the iris is come to repose, the diameter of the pupil is found to be greater than during the first instant, but less than during the dilatation in the second instant. In the first instant the movement is an associated one, while in the second it is of a reflectory character affected by and corresponding with the degree of the stimulus of light. It is therefore by this combination of mechanism that the eye is protected against excessive light, and endowed with the power of accommodating itself to the varying intensity of that stimulus.—*Archiv. f. Physiol. Heilkunde*, 1847. H. i., p. 37–48.

We fear these observations of Schur leave something still unexplained on this highly interesting point of Ophthalmic Physiology: what is here added to our knowledge is a case of complete immobility of both pupils in which a lesion of power in the third pair plainly accompanied the loss of sensibility in the optic nerves, the recovery of the third pair after a time being followed by a restoration in part of the mobility of the pupils. Are we to conclude, then, that, for complete immobility of the pupils in amaurosis, a direct affection of the third pair is necessary, as well as insensibility of the optic nerves? Since it has long been known, as mentioned in Schur's statement (and some of our readers may remember that it was known to Whytt¹ a hundred years ago),

¹ Whytt on Vital Motions, &c. 1750.

that the motion of the pupil depends on some other conditions besides the admission of light to the retina, the proper theoretical inference should all along have been, in amaurosis with complete immobility of the pupils, that the motor nerves of the iris have suffered a loss of direct power as well as of that which they exert when light falls on the retina. But to reconcile us to this inference we have still to discover why the common case in amaurosis should be that of complete immobility, while the less common case should be of partial mobility—that is, why in the common case there should be a direct affection of the third pair along with that of the optic nerves, while in the rarer case there should be a simple loss of power in the optic nerves. There is, however, this alternative. The liveliness of the motions of the pupil, as described by Schur in the above case, at a time when the eyes were quite insensible to light, does not well accord with the belief of the principal source of its motions in health being the excitation of the retina by light,—but it is not improbable that a closer attention to those motions of the pupils which are independent of the stimulus of light, or which are not reflex, would show a greater variety as regards extent in different individuals than has hitherto been suspected, and it is not unreasonable to suppose that Schur's patient being of a very excitable habit was one in whom the motions of the iris, independently of light, had a maximum range. The accounts by practical writers of the state of the iris in amaurosis are often vague from the want of attention to the exact particulars in each case, and it cannot be doubted that where in complete blindness of both eyes the pupil has been thought to act on the admission of light, the direct motions, in excitable persons, have been mistaken for the reflex. If this view be well founded, the direct motions of the pupil in the complete amaurosis of ordinary temperaments pass unnoticed, while it is only in the very excitable that they become obvious enough for observation.

CHEMISTRY.

ON THE PRESENCE OF SUGAR OF MILK IN THE MILK OF THE CARNIVORA. By Dr
A. BENSCH.

DUMAS inferred from his experiments¹ on the milk of the bitch, that though sugar was present in considerable proportion when the diet was vegetable, yet that the sugar disappeared entirely when the food consisted wholly of flesh. Dr Bensch finds that this conclusion is erroneous: affirming after careful experiments, that on long continued exclusively animal diet the sugar does not disappear from the milk, and hence that the organism must possess the power of forming sugar of milk from the fatty or nitrogenous matters. He thinks that the difficulty of detecting sugar in the milk of dogs fed exclusively on an animal diet, arises from the tendency of the phosphoric acid then present to convert the sugar of milk into uncrystallizable grape sugar, which remaining with the extracts in a syrupy form, escapes observation.—*Liebig's Annalen*, Bd. lxi. Heft. 2; and *Chemical Gazette*, June 1847.

OCCURRENCE OF ARSENIC IN VINEGAR. By DESCHAMPS and CHEVALIER.

DESCHAMPS found on preparing pure acetic acid from wood vinegar, that the latter contained arsenic, which he ascribes to arseniferous sulphuric acid having been used in the manufacture of the pyroligneous acid. As wine-vinegar is frequently strengthened with pyroligneous acid, Chevalier was induced to examine several samples of ordinary vinegar, and found some of them to contain very perceptible quantities of arsenic.—*Journ. de Chem. and Méd.* II., p. 334; and *Chemical Gazette*, June 1847.

ON CAROTINE, THE PECULIAR CRYSTALLIZABLE PRINCIPLE OF DAUCUS CAROTA.
By ZEISE.

ZEISE gives a process by which Carotine, the peculiar crystalline principle of the carot, hitherto known imperfectly, can be obtained in a state of purity. Carotine has a faint rather pleasant odour; is heavier than water, and entirely insoluble in water; it is nearly insoluble in alcohol, and very slightly soluble in sulphuric ether. It is extremely soluble in the sulphuret of carbon. Between 300 and 400° of it melts into a perfectly transparent, dark red liquid, and solidifies on cooling, into a vitreous mass. When heated in air it burns with flame, and leaves no trace of the residue. Carotine is a combination of carbon and hydrogen.—*Ann. de Chem. et de Phys.* May 1847; and *Chemical Gazette*, June 1847.

SURGERY.

TWO CASES OF STRANGULATED HERNIA OF THE CÆCUM. By LANDSBERG, of Breslau.

THE first case offers no remarkable interest. It was a case of crural hernia of long standing, in a female of 50, which becoming strangulated, was operated on and cured. In the second case the disease proved fatal, and the dissection is given. The patient was a female, 41 years of age. She had discovered six months before a small tumour somewhat painful in the right groin, which soon disappeared. After an unusual exertion, in the beginning of November 1841, the tumour re-appeared, with pain, constipation, and vomiting. The medical attendant thought he had reduced the hernia as the patient obtained some relief from the taxis, and had subsequently several liquid stools; nevertheless the swelling and pain continued, and were ascribed to an unreduced omental hernia, particularly as the patient had several liquid stools, and the vomiting had ceased. On the eighth day after the attack, Landsberg found the patient in the following state: perspiration copious, breathing painful and hurried, thirst, heat of surface pungent, face and extremities sometimes hot, sometimes cold; pulse thready and above 200; the hernial tumour seated on the outside of the crural vessels, was three inches long, three-lobed, elastic at the base, not tympanitic; the skin was sound; the attempts at reduction painful. These symptoms became more and more alarming, and the patient sunk about midnight. A crucial incision being made over the seat of the disease, a portion of intestine was found displaced which was soon recognised as the cæcal appendix; it was surrounded neither by hernial sac nor serosity; it projected across the internal crural aperture above the crural vessels; towards its free extremity it had a contraction giving an appearance as if the appendix were divided into two parts; it was thickened, black, gorged with blood, and was more than two inches in diameter. A large quantity of feculent matter issued by the crural aperture coming from the cæcum, the posterior surface of which was gangrened. The aperture of the cæcal appendix admitted the barrel of a quill, the extremity of which passed out by a preternatural passage beyond the contraction; neither liquid nor gas issued from the cavity of the appendix.

On this last case there is an editorial remark in the *Gazette Médicale* to the effect that it is probable the hernia was formed at first by the cæcum, with its appendix, and that by taxis the reduction was effected sufficiently to permit the passage of the fæces, and to diminish the pain and swelling, but without putting an end to the inflammation which had arisen, whence resulted gangrene, perforation, engorgement, peritonitis, and death.—*Allgemeine Medicinische Central Zeitung*, December 1846; and *Gazette Médicale de Paris*, 5 Juin 1847.

Cæcal herniæ, though not of very common occurrence, have met with less attention from practical writers than might have been expected, when we con-

sider the peculiar difficulties which attend them in practice. Cæcal hernia has been chiefly described and probably has most frequently occurred under the form of an oblique inguinal hernia of the right side; it has been known also as an inguinal hernia of the left side, and congenitally as a hernia of the tunica vaginalis. In the form of crural hernia, like the two cases above noticed, cæcal hernia has been described less frequently. There are no sufficient means of recognising hernia of the cæcum previous to operation—their uniformly slow descent cannot always be satisfactorily ascertained, and though constant with them, is not wholly peculiar to them; and the depression in the right iliac fossa sometimes discoverable owing to the descent of this portion of the intestine from its usual place, though a sign not to be neglected, can hardly be relied on. The great anatomical peculiarity of this kind of hernia is, the absence of a hernial sac; and hence it appears to have happened that surgeons failing to recognise the cæcal structure have opened the gut in the expectation of finding a sac. There is a source of confusion in the descriptions of authors, owing to their admitting that the cæcal hernia has in certain circumstances a partial sac. The portion of peritoneum here termed a partial sac does not correspond to the hernial sac of ordinary hernia, but to the peritoneal covering of the intestines in general. When the cæcum descends into the scrotum, the inferior part of the hernia corresponding to the original posterior portion of the cæcum, is free from peritoneum; while the peritoneum, which is seen to cover the upper part of the hernia, represents the original partial peritoneal coat which lies on the front and lateral parts of the cæcum in situ. It corresponds in no respect to a hernial sac, except that this partial peritoneal coat of the herniated cæcum is not the identical piece of peritoneum which lay on the cæcum while in the iliac fossa, but a portion corresponding in size derived from the peritoneum in the region of the inguinal canal, its cellular connexions alone being elongated. In short, the cæcum in hernia descends, like the testicle in the foetus, slowly along the cellular aspect of the peritoneum from a higher to a lower point, but presenting during its descent, and at the lowest point of its descent, much the same attachment to the peritoneum as it had at first. When there is a true hernial sac in ruptures, the herniated part, covered by its proper peritoneal coat, is contained within a pouch of peritoneum; but the herniated cæcum, when covered at all by peritoneum, is behind it, as the intestines at large are behind their peritoneal covering. We quote the following passage from one of the authorities whose language on this point we have been criticising, for the double purpose of exhibiting another form of cæcal hernia besides those already adverted to, and of showing how strictly true it is that the herniated cæcum cannot fall within a hernial sac, as the words in italics must satisfy our readers. “As the cæcum descends, the neighbouring portions of the ilium and colon are drawn down, and also a larger peritoneal pouch, which may allow portions of the floating viscera to descend into it. This new protrusion, by progressively enlarging the sac, may cause it ultimately to descend lower than the cæcum itself. If in this advanced state of cæcal hernia the sac be opened, the floating viscera are seen within it; after the removal of which the appendix vermiformis comes into view, and *the posterior wall of the sac appears pushed forward by the cæcum situated behind it.*”¹ As regards the connexions of the peritoneum, what is true of inguinal hernia of the cæcum must be true also of the crural hernia of that portion of intestine.

ON THE SUBPERIOSTEAL EXTRACTION OF BONES. By LARGHI, Principal Surgeon of the Hospital of Verceil.

THE object of this paper is to point out the many advantages of removing portions of bone, so as to leave the periosteum and the muscles attached to it as entire as possible. A few extracts from the memoir will enable our readers to understand our author's plan. He insists that there is not in general any

¹ A Practical Treatise on Abdominal Hernia, by T. P. Teale. 1846, p. 23.

necessity for removing the periosteum along with the bone on account of its participating in the same disease, and that such cases are readily discerned, while he affirms that hypertrophy or thickening of the periosteum, usually observed in the resection of bones, is not a morbid process, but an effort of nature in the way of remedy. "How, then, ought we to proceed? The end in view being to preserve the periosteum, in order to the production of a new bone, it should be our aim in extracting the solid portion of the bone to injure the membrane as little as possible. For this purpose it should be divided no farther than is necessary for the removal of the bone. The portion of bone to be removed is either short or it is long. If it is short, it is sufficient to make a longitudinal incision parallel to the length of the portion of bone to be extracted, and reaching to its surface—through a muscular interspace, for the sake of preserving the muscles. This incision being made, the two borders of the divided periosteum are to be separated, which is the more easy, as it is already in part separated from the bone by the gelatinous fluid, secreted for this very end, and the membrane is thus to be detached all round the bone. In this manner a passage is made for a band, which, by means of a flexible needle, is brought to encircle the bone. While successive tractions are made on the bone by means of this band, the muscles inserted into the periosteum are thrown into contraction, and act in an opposite direction over the several points of the periosteum, whence a complete separation is effected. If there be any difficulty, the injection of warm water assists." Our author then proceeds to show that a large piece of bone may be extracted by performing the above process at each extremity, and that it may sometimes be done without any intermediate cutting of the soft parts. On this plan he says he successfully extracted, in 1845, the osseous part of the eighth and ninth ribs in one patient, almost the whole of the right humerus in another, a part of the right ilium in a third, the lower part of the ulna in another, and several times the first phalangeal bone of the toe.—*Gazette Médicale de Paris*, 5 Juin 1847.

SOME COMPLICATED CASES OF LITHOTOMY. By VONDERFOUR.

The following cases are reported by Vonderfour:—

1. *Recto-vesical operation after the lateral operation.*—A Polish nobleman, fifty-two years of age, had suffered from symptoms of urinary calculus for about four years. In January 1844, the lateral operation was performed, and several small stones removed. The operator, however, was obliged to leave a larger calculus in the bladder, which he was unable to extract. The patient recovered from the operation; but two months afterwards, was suffering more severely from his urinary symptoms than formerly, having obtained relief only as long as the wound in the perineum remained open.

On introducing a sound, it was now arrested about six inches from the extremity of the urethra. An incision was made into the bulb of the urethra, and three small fragments removed. Copious hemorrhage ensued, which was arrested by applying ligatures to two arteries and the subsequent application of ice-cold clothes. Three days after this operation, the sound was again introduced, and it was found that a large stone lay, evidently encysted, behind the prostate. To ascertain definitely whether this calculus was encysted, a pair of polypus forceps were introduced through the wound in the perineum. On seizing the calculus and drawing it downwards (the right fore finger being introduced at the same time into the rectum), it was found to be firmly encysted, and its surface apparently in contact with the mucous membrane of the rectum. The patient was too much exhausted at that time to undergo any further operation, but at the end of a few weeks, the wound in the perineum being healed, the recto-vesical operation was performed on the 24th of April. A staff being introduced, and resting upon the stone, an incision was made about an inch long in the raphe of the perineum terminating in the anus. A straight scalpel was now introduced into the rectum on the fore-

finger, and its point made to enter the bladder behind the prostate. In withdrawing the knife, part of the rectum, the sphincter ani and the deeper parts of the perineum were divided. The edge of the scalpel was now turned downwards, the point was made to enter the membranous portion of the urethra, and pushing it forwards, the rest of the urethra, the prostate and rectum were cut in the mesial line. The indurated walls of the sac still remained to be divided, which was done with a *lithotome caché*, and the calculus with some difficulty was removed in fragments.

The patient made a good recovery, but six months afterwards a recto-vesical fistula remained behind the sphincter, which, however, gave him so little inconvenience, that he would not submit to anything being done for its cure.

2. A pensioner, sixty years of age, suffered from a perineal fistula, and at the same time from symptoms of urinary calculus. On introducing a sound into the bladder, that organ seemed to be filled with calculi. As the fistula occupied the right side of the perineum, the lateral incision was made on that side, and twenty-four calculi of different sizes extracted from the bladder and urethra. The patient made a good recovery, and the fistula became closed. On this occasion the surgeon, apparently for no particular reason, made use of a silver catheter instead of a staff, and during the operation, it had become bent at a right angle, so that the operation had to be performed on a staff introduced through the wound in the perineum, and the utmost difficulty was found in withdrawing the catheter.

3. The lateral operation was performed on a boy of three years of age, and a calculus, weighing a scruple, extracted. Everything went on well till the sixth day, when the contents of the rectum passed through the wound. The rectum had been uninjured during the operation, and the surgeon referred the giving way of the rectum to ulceration, the knife having passed very close to the walls of the rectum. The fistula, however, closed of itself, without any interference, before the external wound had cicatrized.

4. A strong mason, twenty-two years of age, suffered severely from symptoms of urinary calculus, and, on sounding, a large stone was felt. The lateral operation was performed, but on attempting to extract the stone through the arch of the pelvis, the patient screamed and struggled so violently, that the operator was obliged to desist from his attempts. On introducing the finger into the bladder, the calculus was found to be a very large mulberry one. Another attempt at extraction threw the patient into tetanic convulsions. It was now determined that the high operation should be performed, but the patient would not submit, and died three days afterwards from acute peritonitis. The calculus measured an inch and two-thirds in diameter.

ON THE PREVENTION OF INFECTION FROM WOUNDS IN DISSECTION. By PROFESSOR HARGRAVE, of Dublin.

Professor Hargrave offers some suggestions for the prevention of infection from wounds in dissection. The following is the most valuable part of his communication :—

“For some years I have been in the habit of treating myself in the following manner, when suffering from such injuries, either by my own inadvertence or the awkwardness of others. I have always recommended it to my pupils whenever I have seen them so injured.

“The treatment applies to the fingers and the thumb (the parts most frequently liable to be wounded). Wash them well for a few minutes in cold water, then suck them; immediately after apply a ligature above the cardiac side of the wound with such tightness as will induce decided congestion, which will be indicated by the colour of the parts; some blood will also flow from the injured surface, and a certain degree of numbness will follow its application. The ligature is then to be firmly tied and knotted, and allowed to remain on for at least twelve hours; I have kept it on for double that period, and still pursued my professional engagements.

"The physiology of such treatment is explained by the ligature causing a permanent stasis in the fluids of the parts injured on its distal side, and producing a well-marked plethora in them, the greater the amount of it the greater will be the impediment to absorption, admitted by all; yet one pathological observation seems to militate against this statement. The constriction caused by the ligature will also oppose a barrier to the return of the venous and lymphatic fluids into the system, consequently to their being circulated through it, so that the poison is prevented entering into the constitution and destroying it, which will then be eliminated locally from the parts where it was first applied; thus suffering and pain will be obviated, and life valuable to all will be preserved.

"I have no hesitation in directing the attention of the profession to this simple, and, in my experience, efficacious practice. I would also suggest to those engaged in post-mortem investigations, whether anatomical or pathological, a plan worthy of their adoption preparatory to commencing them,—which is to smear or rub into the hands any bland oil; this supplies an additional coating to the epidermal one, and will act as a means of closing any *very minute* and superficial abrasion which might exist, though the individual is not aware of it. Will it also limit the absorbing powers of the skin?"—*Dublin Med. Press*, p. 358, 9th June 1847.

ON ECTROPION AND A NEW WAY OF OPERATING FOR IT.

Dr Blasberg (*Oest. Med. Woch.*, No. 36) ascribes the formation of ectropion not to those various causes which are generally considered as apt to produce it, but rather to a peculiar condition of the orbicular muscle. He is inclined to think that the proximate cause of ectropion consists in a prevailing contractile power in those fibres of the orbicularis which surround those that form the ciliary portion of the muscles. In a man *æt.* 50, with sarcomatous ectropion, Blasberg found a furrow below the ciliary margin of the lower lid, which became deeper every time the patient closed his eyes forcibly. He made a vertical incision in the lid, and finding some of the fibres of the orbicularis much contracted, he divided them, and cured the ectropion. In another case, he made a horizontal incision, about three quarters of an inch long, and having exposed the orbicularis, he divided at three different places some of its fibres which were most developed, and which were situated at about a line below the ciliary margin. In a short time the ectropion disappeared.—*Gaz. Med. di Milano*, Feb. 27, 1847.

THE TIBIO-TARSAL AMPUTATION IN DUBLIN AND PARIS.

The tibio-tarsal amputation has at length been performed in Dublin, and the operator Mr Bellingham, like many other persons who have tried it, is immediately convinced of its utility. He observes that there is little doubt that Mr Syme's modification of amputation at the ankle-joint will eventually supersede amputation of the leg below the knee in many cases of diseased tarsal bones. He says further, "The only objections which it appears to me can be fairly urged against this operation are, that it is by far a more painful one to the patient than amputation as ordinarily practised, while it is a much more tedious and troublesome one to the operator. Disagreeable an operation as amputation is, under any circumstances, the tedious dissection which is necessary here, in order to form the lower flap, renders this much more so; indeed, for this reason, it is an operation which I would not willingly perform, unless the patient could be brought under the influence of ether."—*Dublin Medical Press*, June 2, 1847.

M. Banders, of the Val de Grace Hospital in Paris has just performed the operation for the fourth time, by making lateral flaps and preserving the malleoli. We are informed by M. Rognetta, editor of the *Journal* from which we quote, that in his opinion the modification of M. B. is preferable to the

original operation of Mr Syme, because the flap is formed by the tissues of the heel, which were hard, slightly vascular, not fleshy, subject to become gangrenous, and difficult to dissect. The operation has now been performed seven times in France, and in all with success.—*Annales de Thérapeutique*, Juin 1847.

The objections made by Mr Bellingham and M. Rognetta are easily disposed of. Those accustomed to perform the operation do not find the dissection difficult, and none who have seen Mr Syme practice it, have ever considered it so. It is difficult to perform some operations from written instructions alone. Many surgeons who, like Mr Bellingham, found it tedious and difficult, have considered it neither one or the other after visiting Edinburgh; and many who have been wholly prejudiced against it, have adopted it with enthusiasm on seeing the evidence which this city contains of its advantages. But, supposing it were tedious, this can form no valid objection to a proceeding which has for its object the preservation of the leg, and we are at a loss to understand on what principle the hospital surgeons of London and elsewhere continue to sacrifice the limbs of their patients to a most injurious routine practice. The fear of gangrene need never be entertained, as experience has demonstrated that when the flap is not too long it never arises.

PATHOLOGY AND PRACTICE OF PHYSIC.

DEATH OF A NEW-BORN CHILD FROM HYPERTROPHY OF THE THYROID GLAND.

A ROBUST and full-grown infant born in a state of asphyxia, continued to respire in an abnormal manner even after the removal of the asphyxia. A dose of squill juice occasioned vomiting, after which the child fell asleep. On awakening the suffocative attack was renewed, the voice being hoarse and crowing. Deglutition was also impeded, the food being swallowed with difficulty and with perceptible noise. There was manifestly some permanent obstacle of a kind at once to narrow the wind-pipe and the gullet, in confirmation of which a perceptible swelling occupied the neck. On the fourth night a suffocative attack put an end to life. The dissection gave the following light on the case:—The thyroid gland engorged with blood and composed of a very compact parenchymatous substance, was so much enlarged as to occupy the whole anterior region of the neck—its two horns extended above the lateral parts of the hyoid bone; its lateral borders reached beyond the anterior margins of the sterno-mastoid muscles; the upper concave edge covered the larynx; its lower concave edge the upper part of the sternal manubrium. Behind the thyroid gland there were seen two thick muscles surrounding the windpipe and throat throughout. The most considerable stricture of the windpipe was at its lower part, while the rima glottidis was perfectly normal. At the bifurcation of the trachea a great quantity of dirty milk-like mucus was met with. As to the remaining organs, the thymus was less developed, both the lungs of normal appearance, with the exception of the top, which exhibited several more compact, somewhat liver-like, dark-red spots. The heart was deep-red, and as well as the pericardium was engorged with venous blood. In the pericardium there was about a spoonful of yellow serum. The valve of the foramen ovale was not yet quite closed. The main vascular trunks were of healthy structure. We learn from these post-mortem appearances that in this case the pulmonary circulation had not yet been fully developed, in consequence of which the brain became gorged with blood, and at last apoplectic.

There are two diseases which might have been confounded with that just described, asthma stridulum and croup. These differ, however, in many points; the former by its more tardy appearance, the non-permanency of the

dyspnœa, the tetanic spasm, and complete interruption of the respiratory process at the close of life; the latter by the normal deglutition and in common with the former by its more tardy appearance.—*Caspar's Wochenschrift*.

CASE OF ISCHURIA NEONATORUM. By ALOIS BEDNAR.

A CHILD who died on the 13th day after birth, during the last five days while he was under the care of the reporter, passed no urine. It does not appear whether or not he had passed any on the previous days. The dissection showed the following very interesting state of the parts. The caput gallinaginis instead of passing as in the normal structure into a prominence, ending in two branches which again divide into a great number of little folds, was here branched at its anterior extremity, into two mucous valves, running along the walls of the urethra, from behind downwards and forwards, towards the middle space where they united together. These two crescentic valves with their concavity looking towards the bladder, included between their combined terminations, a very narrow fissure running necessarily from behind forwards. From the structure it is evident that whenever the bladder contracted on the urine, the cavity of the valves became filled with it, and the fissure between them being completed by their distension, not a drop could issue beyond. It was easy to pass a thick probe from the anterior part of the urethra into the bladder, but when the probe was passed from the bladder it was impossible to reach the urethra without the assistance of the eye, the probe being otherwise continually thrown back by the valves. As to the surrounding parts, the rest of the urethra and genitals were quite healthy; the urinary bladder was hypertrophied, its usual thickness being trebled, the thickening being principally in the muscular coat. On the inner surface of the bladder there was seen an incipient tubercular tissue, moreover some little pervious dilatations, and a greater sac with a sphincter-like opening at the termination of the left ureter. The remaining portion of the ureter was dilated and thickened, the renal substance was atrophied. As it cannot be assumed that the high degree of hypertrophy and dilatation of the urinary organs had been developed during the short continuance of life, it is very probable that as soon as the secretion of urine commences, there is a necessity for its evacuation by the urethra. And hence, also, it is to be inferred that an empty urinary bladder is not to be regarded in Forensic medicine as one of the evidences of an infant having lived after birth.—*Zeitschr. d. Gesellph. d. Ärzte Zu Wien*, Febr. 1847, p. 279.

CLINICAL NOTES ON HOSPITAL PRACTICE.

SULPHATE OF QUININE IN ANEURISM OF THE AORTA AND OTHER INTERNAL ANEURISMS.—It appears that sulphate of quinine has been employed with much success in some Italian hospitals for the relief of aneurism of the aorta and other internal aneurisms. It belongs, in this use of it, to what are termed hyposthenics (subduing action) and is to be carried as far as the system will bear it. It has, say its Italian supporters, the immense advantage of bringing down the pulse without disturbing its rhythm, of making the buffy coat of the blood disappear, that is, of dissipating the organic condition, namely, arteritis, on which it depends, and thus of retarding the progress of the aneurismal tumour. The other hyposthenics adapted to the same end according to the same authorities, as by alternation with the sulphate of quinine, are the vegetable and mineral acids, the sulphate of iron, the ergot of rye, the cold ferruginous waters, the arsenious acid, the acetate of lead, and the iodide of potassium.

ELATERIUM IN BRIGHT'S DISEASE OF THE KIDNEY.—Rostan in the Hospital "La Charité," has begun to follow the recommendation of Bright in favour of elaterium in granular disease of the kidney of the chronic form. He has found it to be of some advantage, though hardly coming up to the expectations he had formed of it. This he ascribes to the inefficiency of the elaterium to be met with at Paris, as compared with that sold in London—and he has taken steps

to procure a supply of the more efficient article from London. The editor of the *Annales de Thérapeutique*, considers elaterium to be so little known in France that he has transcribed in his April number the principal part of Dr Christison's account of this substance, from his treatise on Poisons, as the only carefully written article on the subject which he has met with.

IODIDE OF POTASSIUM IN HEMIPLEGIA.—The patient was a female of 44, of previous good health—there was complete loss of feeling over the whole of one side, and to a considerable extent loss of motion—the attack had been preceded for a few days by vertigo and other signs of cerebral congestion, and by convulsive movements of the face. The disease was now of nearly two years standing—and from time to time there were attacks of epileptiform convulsions. She had been treated first at home and then at the Hospital “Cochin,” without any amendment, and, finally, entered “La Charité, under Briquet. He came to the conclusion that the disease must be dependent on a syphilitic tumour (exostosis or periostosis) at the base of the cranium, and put the patient under full doses of iodide of potassium. This treatment has been persevered in already for two months, in doses increased from 10 to 18 grains a day, with the most striking success. There is now hardly any remains of the disease except that the integuments over the great pectoral and in the claviculæ region of the left side are still insensible, while the tongue still turns a little to the right when put out. The same treatment is still continued, and there is a progressive improvement. It should be mentioned that the patient denies that she had ever been affected with syphilis.

HYPOTHENICS IN TUBERCULAR PHTHISIS.—It appears that the inclemency of the first months of this year has filled some of the Parisian hospitals with cases of phthisis, most of them in the last stage of exhaustion. We find that out of forty beds in Andral's wards of “La Charité,” twelve were occupied a few weeks since by patients labouring under this ruthless malady. What, asks the reporter, does M. Andral do to arrest the progress of this scourge of humanity? Barley-water, Seltzer-water and opium, exhaust his prescriptions, “à titre sans doute de medication morale.” On this despair of arresting the progress of phthisis at least in its early stage, M. Rognetta, so well known as a staunch disciple of the Italian school, remarks, “It is forgotten that what kills is not strictly the tubercle, but in fact the visceral reactions which its mechanical effect excites.” . . . “It is certain that if we wait before combating the reactions in question, till phthisis has reached that stage when the lung is in a great measure consumed by caverns, congestions, hepatizations, till the heart and great vessels have undergone sensible alterations, it would be chimerical to hope for any favourable result from any means in the power of medicine. But let these reactions be taken at their onset, when they are still but functional, and let them be attacked then not by detraction of blood, for this remedy is insufficient and rarely applicable on other accounts, but at once with hyposthenic cardiaco-vascular remedies, and there will be found in these resources an efficient power of which those can form no conception who obstinately reject all exact notions on the dynamical power of remedies.” He goes on to say that if M. Bouillaud, M. Andral, and M. Chomel were asked if they had ever seen phthisis treated by the appropriate administration of sulphate of iron and quinine, tartar emetic, ergot of rye, cantharides and compounds of iodine, they would tell you that all these are incendiary drugs.

BILIARY CALCULI AND POLYSARCIA.—Lallemand, surgeon at “La Salpêtrière,” had remarked in his dissections as well as in hospital practice that biliary calculi are most frequently met with in persons who are very fat. Rostan, when he was physician at La Salpêtrière, continued these researches, and he found very often biliary concretions in the gall-bladder of very corpulent old women. M. Rostan considered what relation could exist between polysarcia and concretions of this sort. It is not easy perhaps to answer this question exactly, but there is here probably some direct relation of connexion between the

qualities of the bile or of the blood of the vena portæ, sent to the liver for purification, and the work of alimentation in the digestive passages by which the blood is produced.—*Annales de Thérapeutique*, April and May, 1847.

MIDWIFERY, AND THE DISEASES OF WOMEN AND CHILDREN.

INFLUENCE OF ETHERIAL INHALATIONS ON THE UTERINE CONTRACTIONS.

NOTWITHSTANDING the remarkable results obtained by MM. Paul Dubois, and Simpson, one important point with regard to the exhibition of etherial inhalation in obstetrics has not yet been set at rest, viz:—has ether the effect as supposed by M. Velpeau, of suspending or diminishing the power of the uterine contractions so far as to facilitate certain obstetrical operations, such as the version of the fœtus. The following fact will further confirm the experience which we have already acquired, viz. that etherization neither suspends nor diminishes the resistance offered by the uterus to the introduction of the hand, and therefore does not facilitate version.

M. Stoltz, of Strasbourg, was called to a primipara, æt. 24, of strong constitution, and in the sixth month of pregnancy. She received a fall on the first of March, and after suffering somewhat from pain in the abdomen, and sacral region, she was taken in labour on the 5th of the same month. On examination, M. S. found the right foot and right arm at the uterine orifice; from the shape of the uterus, it was concluded that the fœtal head was to the left and inferior portion of the uterus, and the pelvic extremity to the right and upper side. The absence of a battement showed the fœtus to be dead. M. S. determined to deliver by turning, the patient was placed in the required position, and ether exhibited with the usual results. Seeing that sensibility had almost entirely ceased, M. S. attempted the introduction of his hand into the vagina, when the woman immediately awakened up, and resisted him with loud cries; he then desisted for two or three minutes, until insensibility was complete. From that moment he was enabled to introduce his hand into the vagina, and accomplish the necessary operation without the patient at all resisting him, or complaining, although the passage of his hand through the vulva was not effected with greater ease than is usual in primiparæ. Having seized the foot which presented at the os uteri, M. S. attempted to extract the fœtus, but so powerful were the uterine contractions, that the thighs could not be disengaged. Having applied a noose upon the right foot, M. S. then introduced his hand again in order to seize the other foot still in the uterus: he had at first great difficulty in passing his hand through the os uteri, and even after having succeeded in this, he was unable, in consequence of the violence of the uterine contractions, to penetrate far enough into the uterus to reach the left foot; he found himself compelled to desist from this attempt, and at length with great difficulty succeeded in extracting the body of the fœtus from the uterus, but the os uteri still contracted so powerfully round the neck of the fœtus, that he could not proceed with the extraction. During the whole of this time the etherial inhalation had been continued, and the patient had remained in a state of complete and uninterrupted repose and insensibility. The patient at length showed an inclination to vomit, and without much effort ejected a large quantity of frothy mucus; about a minute thereafter she woke up, and said she had dreamed that some one had attempted to deliver her, and had thereby caused her pain; she gradually recovered, and complained only of a slight uneasiness in the head and throat. The head of the fœtus remained all this time encircled by the os uteri; in about an hour uterine contractions were renewed, and a few slight extracting efforts sufficed to complete the birth of the fœtus.—*Gazette Médicale de Strasbourg*, Mars, 1847.

MONSTROUS BIRTH, REQUIRING EMBRYOTOMY. By M. HURON.

M. H. was called to a patient in labour for the sixth time. On making a

vaginal examination he found that the foetus presented its pelvic extremity, and presently four feet protruded from the vagina. M. H. at first imagined that the case was one of twins; but on making a further examination he found that either pair of legs proceeded from a separate pelvis, but that higher up the foetuses were intimately connected; by and bye the thighs and the lower parts of the trunks were disengaged, and lastly, one umbilical cord inserted below the point of junction of the foetus; the uterine contractions continued until the lower part of the common thorax was extended, and then there remained no doubt of the nature of the monstrosity. It was probable, but it could not be accurately ascertained, whether or not there were two heads—if this were the case a serious obstacle to delivery was to be apprehended, for the probability was, that the bulk of the two heads together would be too large to admit of their entering the pelvic cavity. This probability was confirmed by the fact that from the moment that the lower part of the thorax was born, the delivery did not advance in the slightest degree. M. H. could introduce his hand only a short way into the pelvis towards the sacrum; there it met with a bulky resisting mass, formed no doubt by the two necks and heads. The foetuses showed by their movements that they were alive when the legs were protruded, but they were now dead, in consequence of the pressure upon the cord. M. H. had therefore only the interest of the mother to look to. He first tried traction upon the four limbs without any result; he therefore set about trying to push back one of the foetuses and extract the other, to tilt the one head over the other so as to cause their successive and not simultaneous entry into the pelvic cavity; this plan also was found unavailing. Forceps for obvious reasons could not be applied—it was therefore resolved to cut away one of the bodies as far as possible; and as it was obvious that the head next the sacrum could be more easily drawn down than the head next the anterior wall of the pelvis, for the latter would probably hitch upon the symphysis pubis, the anterior foetus was for this reason cut away, and a bistoury was passed through its body between the eighth and ninth dorsal vertebra. This body being removed, the operator was enabled to exert traction upon the posterior foetus, much more in the direction of the pubes. Consequent upon this measure the posterior head, closely followed by the other, gradually passed through the pelvis. The case is accompanied by an anatomical description of the monster, but it is not stated if the mother recovered.”—*Archives Generales de Médecine*, Mai 1847.

Part Fourth.

MEDICAL NEWS.

EDINBURGH OBSTETRICAL SOCIETY.

SESSION VI.

FOURTH MEETING.—*April 13th*, 1847. DR SIMPSON in the Chair.

Dr Young, Dr Richard Young, Dr Balfour, Dr Menzies, and Dr Burns, were admitted as resident members.

SPONTANEOUS EVOLUTION.—When this subject was brought before the Society at last meeting, Dr Keiller remarked that a striking instance had occurred to him when practising in Dundee. He had seen the patient early in the labour, but was obliged to leave her in the hands of a midwife, who, being a friend of the patient, had volunteered her attendance as a sick-nurse.

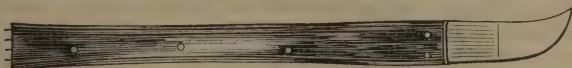
Dr K. was not again summoned until the case had assumed the following aspect:—The liquor amnii for several hours discharged, a large swollen arm occupying the passages, the os uteri firmly grasping the obtruded shoulder of

the child,* and the uterine contractions unusually violent and continuous, a state of matters that had, in all probability, been encouraged, if not induced, by large doses of the ergot of rye, which the "*midwife*" confessed had been administered for the purpose of bringing the case to a speedy termination.

Dr K. failed in his repeated attempts to insinuate his hand into the uterus, in order to complete the delivery by the operation of turning; notwithstanding the free use of opium, the uterine contractions continued for some time excessively severe, when, during a violent pain, the arm was suddenly withdrawn from the passages, and the feet and body were almost simultaneously expelled, a rapid and complete evolution having spontaneously occurred. The child was of course dead. The mother made a good recovery.

Dr Keiller farther stated in connexion with this case, that he had attended a *twin sister* of the patient in three of her confinements, in two of which the *placenta* presented, demanding turning, and in her third accouchement, the breech was the form of presentation.

DR WEIR'S VACCINATOR.—*Dr Weir* showed to the Society a new instrument which he and others had used for the last two years. It consists of a small handle of ivory, with four needle-points projecting from one extremity, and a small curved knife for collecting and separating the vaccine matter at the other (as shown in the cut). The skin is opened by a crucial scratch with the needle points, which are held vertically, and are lightly applied, so as merely to remove the cuticle.



The advantages of this instrument over the lancet are, that the operation is done more speedily, and that it opposes a larger surface for the absorption of the lymph, which is less liable to be washed away by too great an effusion of blood.

Dr Weir also adverted to the different modes of preserving and carrying vaccine lymph. Of the ordinary methods, viz. by means of the ordinary vaccine bottle—between plates of glass—and in a glass tube, or in a bulb and tube hermetically sealed; the last was certainly the best method to follow in sending vaccine matter to a distance, as to India, for example. The tubes, however, are very liable to be broken. *Dr W.* had lately attempted to preserve it by soaking a piece of sugar with the lymph, pounding the sugar when dry, and keeping it in a well closed bottle. He applied this powder by sprinkling it on the exposed surface with a hair-pencil. In all the cases in which he had used this powder, the vaccination succeeded quite well. In two cases he made two scratches, in one of which he inserted the lymph in the usual way; in the other he sprinkled the powder. The result was much the same in all. He had attempted some time ago to preserve the lymph in naphtha, but the result was not satisfactory.

Dr Thomson stated that he almost never failed in vaccinating after the ordinary method. He always took off the matter from the pock on the seventh day. It was less in quantity than on the eighth day, but he believed it to be much more active.

Dr Weir had used matter which was twenty years old, and had been carried to India and back again four times. It was preserved partly in glass-tubes, hermetically sealed, and partly between plates of glass, covered with bladder. With the former he was invariably successful; with the latter only in one instance.

A discussion followed as to the quantity of matter that should be taken from a vesicle. Some maintained that the only active lymph is what is actually contained in the vesicles at the moment, while others were of opinion that the serous fluid which continues to exude for some time after the pock is opened, will communicate the vaccine virus by inoculation equally well as what escapes

at first. It was also asked if the effect might not possibly be obtained by inoculating with the serum or the tears of a person in whom the vaccine pock has come to maturity, in the same way as recently children have been said to have been affected with measles after inoculation with the serum or tears of others labouring under measles at the time. Dr Mackenzie, Dr Weir, and others, agreed to make experiments on the subject, and to report to the Society.

CASES OF FATAL HEMORRHAGE FROM THE UMBILICAL VESSELS IN INFANTS.—*Dr Simpson* read a detailed account of two fatal cases of hemorrhage after separation of the cord. They occurred in India, in the practice of the late Dr Christie, and were communicated to Dr Simpson by Dr Christie of Dundee.—Both infants were of the same parents, who were quite healthy. The hemorrhage did not come on till some days after the cord had separated. The ordinary means for arresting hemorrhage by pressure, and various styptic substances were used, but only with very temporary benefit. Both children died on the eleventh day. In both instances there was a degree of jaundice. In the first case, the state of the vessels was not examined, but in the second the umbilical vessels were found to have their walls much thickened and indurated as far up as the liver.

Other cases were mentioned, and the transfixion of the bleeding part with a needle and including ligature (as in hare-lip), was described as the best plan of treatment.

FATAL HEMORRHAGE FROM THE UMBILICAL CORD THREE DAYS AFTER BIRTH.—*Dr Keiller* stated that he had been called upon to examine the subject of the following case of *fatal umbilical hemorrhage*, which recently occurred in the practice of a gentleman in town.

About $\frac{1}{2}$ past one p.m., on Tuesday the 3d instant, Mrs —, aged 29, was delivered of her third, a fine plump male child. The infant continued quite well until 5 o'clock, when it began to vomit a quantity of green bilious matter. On Thursday morning, however, the child was apparently well, being able to suck greedily, but again vomited towards the afternoon. About two a.m., on Friday morning, the mother first discovered that the binder, &c., of the child were "soaked with blood from the navel." (She stated that she had changed the linens of the infant about 12 p.m., and did not then notice the existence of any bleeding). The practitioner who delivered her was immediately sent for, but could not visit at the time; he however told the parties to apply a ligature below the one that was already around the cord, but they did not *deem it their duty to interfere*, "but preferred waiting until the gentleman could find it convenient to come and tie the cord himself," which was not until between 4 and 5 o'clock, when he visited and examined the bleeding point which was at the root of the cord, and applied to it the nitrate of silver, which seemed at the time to be sufficient to prevent the farther continuance of the hemorrhage. Before half an hour had elapsed, however, he was again summoned, when he tied a ligature around the umbilicus, embracing a portion of the skin which formed its circumference. This treatment however proved too late; the child died in a very few minutes after the application of the ligature.

FUNGUS OR FUNGATING EXCRESCENCES OF THE UMBILICUS IN INFANTS.—*Dr Simpson* stated, that in infants, after the umbilical cord has dropped off, instead of the raw surface contracting and cicatrizing, he had several times seen large granulations appear, and a red, elevated, fungus-like excrescence form at the umbilicus, resembling the fungus testis of surgeons. These umbilical excrescences in general shrink and slough after a time, or do so on being touched with alum or other astringents, or with nitrate of silver. In one case which he had lately seen with Dr Finlay of Newhaven, this simple treatment had little or no effect. The excrescence enlarged to the size of a cherry, which it likewise resembled in colour. It was apparently insensible to touch, but blood oozed from its red surface under slight handling. It was cauterized several times with nitrate of silver; but this did not cause it to shrink. At last, after several weeks, a ligature was passed round its base, and in a few days it had dropped off. It had not offered in any degree to return.

Dr Weir stated that he had met with the same appearance in a family of four or five children. In all of them a similar, but smaller excrescence, formed on the umbilicus. They soon shrunk on being treated with powdered alum.

TREATMENT OF INFLAMMATORY INDURATION OF THE CERVIX UTERI BY DEEP CAUTERIZATION WITH POTASSA FUSA.—*Dr Simpson* stated that his own observations fully confirmed the recorded opinions of *Dr Bennet* and others regarding the general dependence of leucorrhœa upon inflammatory ulceration and induration of the cervix uteri. He had found inflammatory enlargement and induration of the tissues of the cervix very frequent in practice, and existing, in fact, in most cases of very chronic and aggravated leucorrhœa. In practice he had seen it mistaken for the induration and ulceration of carcinoma, &c. Formerly, in the treatment of these common cases, *Dr S.* had employed the frequent local application of leeches, and counter-irritation to the sacrum, &c., with the use of pessaries of mercurial and iodine ointment, keeping the indurated tissues imbedded in these applications, &c. The cure in this way is tedious, and months are often required before the indurated parts became reduced. Various local escharotics, partly to destroy the indurated tissues by direct decomposition, and partly to soften down the remainder by new inflammatory action, had been in modern times employed for the same purpose, and with much more certain and expeditious effect. *Dr S.* had in this way employed in a number of cases nitrate of silver often applied, Vienna paste (*potassa cum calce*), and nitric acid. He stated the particulars of a case which he had treated successfully four years ago with nitric acid, and he had lately seen the patient in perfect health; it was at the time supposed that she had cancer uteri. Latterly, *Dr S.* had abandoned these and other escharotics, and now always used the common *potassa fusa*. He had found it far more manageable, speedy, and certain than any other method. He used it of course through the speculum, applying a stick of it freely with a proper caustic holder to the ulcerated and indurated tissues. It required to be rubbed or held *strongly* for a time against the part which was to be destroyed. In general a piece three-quarters of an inch, or an inch long, was melted down. The decomposition produced by it often caused a hissing sound. If the induration is extensive, and the whole cannot be removed at once, increased action and absorption are set up in what remains, and the parts adjacent become softened and diminished in size. Absorption in this way was truly one of the results or consequences of inflammation, though still an undescribed *termination*. In some aggravated cases two or more applications of the caustic are required, at intervals of eight or ten days. *Dr S.* has never seen pelvic cellulitis, or any other bad result follow. The appearance after the operation is as if a portion had been clean cut out with the knife. A large quantity of vinegar and water is immediately thrown up through the speculum to neutralize the potassa, and prevent it from injuring the sound parts. A copious purulent discharge usually follows for several days, requiring the use of astringent washes, or zinc ointment pessaries. When the whole of the induration is once removed, the remaining ulcer heals rapidly and permanently. An ulcer over an indurated part may be cicatrized, but it is almost certain to break out again and again till the induration itself is reduced.

EPIDEMIC FEVER AND SCURVY.—The number of cases of fever seeking admission into the Edinburgh Royal Infirmary continues undiminished. The managers are indefatigable in their exertions to provide accommodation. Sheds, tents and attics have been in active requisition to receive the numerous applicants. Eight physicians have their hands full, and two more are about to be appointed. The cases of fever alone amount at present to about 600. The Epidemic is a short fever, with an almost invariable relapse about the 14th day. Along with this there are not a few cases of genuine typhus, and still some cases of dothenenteritis. The epidemic is not very fatal, except in those beyond middle life. Its contagious character is beyond doubt—many of the nurses have

been attacked as well as the assistant medical officers. Among these we have already to lament the death of two young medical men of uncommon promise, Mr Holmes Ivory and Mr W. S. Dugale, and others are unhappily still in danger.

Glasgow, as regards Scurvy, has been in much the same state as Edinburgh; and as regards fever, it is, like Liverpool, worse off. Scurvy is prevailing north and south of us, and is even heard of in a severe form from Exeter, where it has been well described by Dr Shapter. Even in London, so remarkable for its ordinary immunity from fever in relation to its population, fever is much more severe than usual—the Fever Hospital being already full, while the mortality per week a month ago, was already 50 within the precincts of London—more than 20 above the weekly average from fever at this season. It is not likely that any large town in the kingdom will escape the prevalent epidemic.

PROGRESS OF ETHERIZATION.—Dr Mackenzie of Glasgow says, that after witnessing the sedative effects of sulphuric ether in preventing the pain of surgical operations, he was led to try whether the same application might not prove useful in quelling painful diseases. He has since used it in a variety of eye-diseases with very satisfactory results—the diseases mentioned are scrofulous ophthalmia, corneitis, sympathetic ophthalmitis, neuralgia affecting branches of the fifth nerve, and asthenopia—and in all these some benefit was obtained.—*London Medical Gazette*, June 18.

BOOKS RECEIVED.

1. A Treatise on the Structure, Diseases and Injuries of the Blood-vessels, &c. By Edward Crisp, M.R.C.S. London. 8vo. 1847.
2. Hydropathy and Homœopathy impartially appreciated, &c. By Edwin Lee, Esq., &c. London. Small 8vo. 1847.
3. The Retrospective Address on Medicine for 1846. By W. H. Ranking, M.D., Cantab. Worcester. 8vo. 1847.
4. Observations on Aneurism and its Treatment by Compression. By O'Brien Bellingham, M.D., Edin., &c. London. 12mo. 1847.
5. Remarks on Medical Organization and Reform (Foreign and English), with a Supplement upon the Medical Reform Question. By Edwin Lee, Esq., &c. London. 8vo. 1847.
6. Hassall's Microscopic Anatomy. Part ix.
7. Chelius' Surgery. By South. Part xvi.
8. Observations on the Treatment of Lateral Curvature of the Spine, &c. By Edward F. Lonsdale, F.R.C.S., &c. London. 8vo. 1847.
9. Practical Observations on the Pathology and Treatment of certain Diseases of the Skin generally pronounced intractable, &c. By Thomas Hunt, M.R.C.S. London. 8vo. 1847.
10. Surgical Operations after the use of the Vapour of Sulphuric Ether. By T. W. T. Johnstone, M.D. (From the Madras Spectator, April 1847.)
11. A Treatise on Diet and Regimen. By William Henry Robertson, M.D., &c. Part ii. London. Small 8vo. 1847.
12. Introductory Discourse on Pathology and the Practice of Medicine. By Robert Spittal, M.D., F.R.S.E., &c. Edinburgh. 12mo. 1847.
13. Observations on the Principle of Vital Affinity. Part ii. By W. P. Alison, M.D., F.R.S.E., &c. (From the Trans. of the Royal Soc. of Edinburgh.) 4to. 1847.
14. On the Pathology and Treatment of Dysentery. By William Baly, M.D., &c. 8vo. (From the London Medical Gazette.)
15. On Dyspepsia, &c. By John Burdett Steward, M.D. London. 12mo. 1847.
16. De Symptomatologia Meningitidis Cerebralis acutæ Dissertatio, &c. Auctore Moritz Frier. Hannæ. 12mo. 1847.

ERRATUM IN LAST NUMBER.

P. 939.—For Dr Smith Tyler, read Dr Alexander Tyler.

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AUGUST 1847.

No. 14. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Use of Ether in the Performance of Surgical Operations.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh, and Honorary Member of the University of Cracow.

TOWARDS the close of December last, I received from Dr Alison a letter addressed to him by his friend Dr Boott of London, announcing that he had introduced from America the inhalation of Ether as an antidote to the pain attending surgical operations. I lost no time in trying this proposal, at first by extemporaneously constructed, and afterwards by more perfect, apparatus. My colleagues in the hospital also performed various operations under the influence of ether, so that before long a considerable amount of observations was accumulated.

The results thus far obtained were to my mind not at all satisfactory. In some cases it had been found impossible to produce the state of insensibility desired. In others, violent convulsive movements had been induced. In others, prolonged nausea and discomfort. While in several instances death had occurred under circumstances that seemed to leave no room for doubt as to its having proceeded from the inhalation. In common with many members of the profession, who had experienced similar discouragement, I therefore resolved to abandon the use of ether; at all events, until more certainty could be reposed on its administration.

In London, where the practice commenced in this country, and where some of its earliest supporters had nearly, or entirely, desisted

from employing ether, a fresh impulse was given to its use by the skilful manipulation and care of a gentleman (Dr Snow), who devoted his attention to the subject, and in whose hands the inconveniences previously experienced were no longer encountered. I therefore felt desirous of making another series of trials, and with this view employed a very simple and efficient apparatus, which was said to have been extensively used in obstetric practice by the Professor of Midwifery, without producing bad effects. This was merely a flask to hold the ether, poured on some bibulous material, such as a piece of flannel, with a mouth piece, and an aperture in the side to admit air. The wide openings, and absence of valves, render respiration by this apparatus perfectly free; while there is no risk of the ethereal vapour being allowed to enter the lungs without a safe admixture of atmospheric air.

I found the results of this second trial very different from those of the first. The patients, instead of requiring a tedious and laborious respiration, frequently interrupted by cough, yielded almost immediately to the overwhelming and not unpleasing influence of the full ethereal stream. They consequently inhaled on the whole a smaller quantity, and suffered in a proportionate degree less of the disagreeable effects. The following operations were all performed, nearly at the same time, within a few days of each other, and may be mentioned in illustration of the impunity from bad consequences. The first occurred in private, and the others in the hospital.

1. Large fibrous tumour of the breast. From the commencement of inhalation to the conclusion of the operation four minutes elapsed. Complete union by the first intention. Patient able to leave her bed-room before the end of a week.

2. Amputation at the shoulder joint for osteo-sarcoma of the humerus. Union by the first intention. Patient up and dressed by the end of a week.

3. Excision of the shoulder joint, including the head of the humerus and glenoid cavity, removed by dividing the neck of the scapula. Union by the first intention through the greater extent of incision. Recovery delayed by slight erythema, but now advancing rapidly to a satisfactory conclusion.

4. Amputation of the foot by Chopart's operation through the tarsus. Union by the first intention. Slight suppuration under the integuments over the end of the fibula, requiring an outlet for the matter. Recovery now nearly completed.

5. Large tumour of the scrotum, weighing two pounds. Not the slightest unpleasant symptom. Wound nearly healed.

6. Tumour of the jaw, requiring removal of four teeth and a portion of the alveolar process. No unpleasant symptom. Patient dismissed a few days after the operation.

While discharging what I conceive to be my duty in offering this testimony as to the safety and efficiency with which ether may

be administered, I am very far from desiring to sanction its indiscriminate employment, or attaching to it the importance that has been so much insisted upon by some popular writers. In an inflated article of "the North British Review," obviously written more with a view to make an impression upon the non-medical part of the public, than to convey useful information—the author, with greater credit to his plausibility than sincerity, if, indeed, he really possessed any surgical knowledge or experience, has not scrupled to magnify the value of an influence causing insensibility, or forgetfulness of suffering, by greatly exaggerating the pain of operations, and misrepresenting the effect of its suspension upon their performance. Physical pain is undoubtedly an evil, but certainly one of very secondary importance when compared with any want of perfection in attaining the object for which an operation is performed. It may be added, that the pain of trivial operations, such as the extraction of a tooth, or the evulsion of a nail, would form a very erroneous standard for estimating the amount of what is felt on the occasion of more extensive mutilations. I have frequently, in operating at the hospital, called attention to the perfect quiet and composure of patients under the greatest liberties ever taken with the human frame, such as the removal of large tumours, amputations, and lithotomy. On many of these occasions not one cry or groan, or even any alteration of the breathing, not a movement of the body, or contortion of the countenance could be noticed; and the patients, when asked to describe their sensations, have represented them as no wise unbearable.

But however severe the pain really were, it would still be of little consequence when compared with the effects of the operation; and the patient, upon awakening from his ethereal trance, would be sorry to learn that immunity from suffering had been obtained at the expense of unnecessary injury to his body. It has been said, indeed, by the Reviewer, that the ether, so far from impeding the proper performance of operations, will greatly promote it, by freeing the surgeon from a powerful inducement to hurry through their steps, instead of taking them with care and deliberation. But surely, though this argument might suffice for "*vulgus captandum*" purposes, it could not for an instant impose upon any member of the surgical profession. Operations performed quickly are in general performed well, not *because* of the short time they occupy, but in consequence of nothing more being done by them than what is actually required; while slow operations are in general ill-performed, not by reason of their slowness, but from the unnecessary groping, squeezing, cutting, and tearing required for their completion. If a surgeon can perform an operation properly, he will not render his work more perfect by protracting its execution; and if he cannot duly discharge his duty on a patient who retains sensibility, he will find himself no less unable to do so in operating upon one stupified by ether.

In many operations it is of the utmost importance that the patient should retain a voluntary control over his movements, not only for assisting the operator by executing those that he may desire, but by abstaining from those which would be obstructive of the object in view. Thus I have known the little operation for fistula in ano not only impeded but prevented by the convulsive efforts induced through the use of ether. In all careful dissections, as those for hernia, and the removal of tumours from intricate connections of importance, I would therefore advise against the inhalation. I lately disarticulated a clavicle from the sternum, for osteo-sarcoma, and dissected out some large deep-seated tumours of the neck, with results which, I believe, might not have proved so satisfactory if ether had been used. In operations affecting the nose and mouth, also, I should think it inexpedient to render the patient insensible; lest from the want of voluntary effort, suffocation, or an approach to it, might arise from the entrance of blood into the air passages. Finally, I beg to warn against administering the ether to persons in a state of great weakness or exhaustion. About a week ago I performed amputation of the thigh on a young man brought into the hospital from the neighbourhood of Kirkcaldy, where, seven hours before the time of his admission, his leg had been completely shattered by a railway carriage. There was no sign of re-action, or promise of any from delay, and I therefore proceeded to remove the limb. Upon the incisions being made, relaxation of the sphincters took place, the contents of the rectum and bladder were voided, and an effort at vomiting seemed the prelude of immediate dissolution. Before tying the arteries, I waited to ascertain whether the condition of the patient depended upon syncope or death. My colleague, Dr Duncan, by causing alternate pressure and relaxation of the chest, effected artificial respiration for some time without any sign of returning life. But by and by the actions of the system were gradually restored, and maintained through the use of stimulants. The wound has healed by the first intention, and the patient now seems to be in a fair way of recovery, which he most certainly would not have been if the vital powers had been in the slightest degree lowered or impeded in their operations by the use of ether.

ARTICLE II.—*Contributions to the Pathology and Treatment of the Scorbutus, which is at present prevalent in various parts of Scotland.*
By CHARLES RITCHIE, M.D., one of the Physicians to the Royal Infirmary of Glasgow.

(Continued from page 49).

SINCE the former part of this paper went to press, I have had an opportunity, through the kindness of Dr Easton of the Fever Hospital, of examining the body of a woman who died while labouring under the usual form of scorbutus; and as it supplies a deficiency

in the pathological history given of the disease in my former communication, I make no apology for inserting the dissection here.

Eliza Forbes, aged 33, 8th June 1847.—Six days ago was seized, without known cause, with rigors, headache, and general soreness, which latter continue. Mind is confused; countenance depressed and typhoid; tongue brown and encrusted in centre, dry and white at edges; skin hot and dry, but without eruption. No chest symptom. Abdomen tender on pressure, and there is diarrhoea with dark brown dejections. Urine natural but high coloured. Catamenia regular.

Both legs are of a light yellow tinge, and on the inside of each there is a dark purple coloured patch of about two inches in length. Similar ecchymoses are situated immediately above the popliteal spaces. Gums are slightly spongy. Died on the morning of the 11th.

Inspection.—Twenty-eight hours after death. Body large, plump, and rounded. A few purple patches on face. Entire system of subcutaneous veins filled with blood, causing a striking appearance of claret coloured arborescence over general surface. Blood was being discharged from vagina, which latter was livid. Cuticle over most dependent part of swelling in right ham was elevated by bloody serum. On making an incision here and over remaining portion of the scorbutic swelling on the front of the leg, there was a copious escape of somewhat reddish transparent fluid. The areolar tissue was of a faint red colour, and loaded with fat and serum, to which latter it owed its colour. Above the ham it exceeded an inch in depth, and, at other parts, was about half this thickness. Various ecchymoses were observed, having respectively for their centres the veins in the superficial fascia of the leg, from which they stretched outward, through the areolar and adipose matter, to the discolorations in the skin.

Trunk.—The heart was a little larger than natural and flaccid. Nearly four ounces of blood were obtained from different sources from the body. It was mixed with a few soft clots and fibrinous shreds. The inner surface of the heart was of a purple colour; and the serous lining of the aorta throughout its course, as that also of the femorals, was of a crimson red, marbled by deeper lines of colouring at some points than at others. The internal membrane was soft, and separated with ease from the other layers. Vena cava, iliac and femoral veins were dyed of a pale port wine colour. Spleen enlarged and diffuent. Liver of increased bulk, pale, and softened. Bile tarry. Kidneys large, flabby, and of a deep purple colour.

The *causes* of the epidemic remain to be spoken of.

It is familiar to every one that the failure of the potato crop for two successive seasons, did, with the stunted growth of our pastures and other crops, lead to a rise of between 30 and 40 per cent. on all kinds of provisions during last winter. From the same cause the value of money rose in the same period little short of a half; and, in consequence, the workmen in every branch of home trade were thrown more or less out of employment. The winter, again, was early, continuous, protracted, and severe. The second summer, which we have usually enjoyed these few years past from the end of November till January, was replaced this season by an unmitigated winter, which, excepting a little warm weather in April, continued in much of its coldness down to May. One effect of this was to render all kinds of fresh succulent vegetables unattainable by nearly every class; and another was, to raise the price of milk and other dairy produce beyond the constant reach of the poor. A third consequence was, that exposed persons who had much phy-

sical exertion, required for the maintenance of their heat an increase of nutriment from which they were precluded by the other circumstances of their position ; and concurrent effects of the same cause were, that while the ordinary trades-people got chilled and wetted, their feet and legs particularly, in passing to and from their workshops, the railway labourers, who are hired by the piece and cannot work during rain, sustained by this means a serious reduction of wages, and were at the same time kept often exposed to the weather in expectation of work.

Accordingly, of the infirmity patients in this epidemic, about 95 per cent. had suffered a total deprivation of fresh succulent vegetables for a period of more than six months, and the allowance of the remaining number had been insufficient. Above 50 per cent. had been cut off besides from fresh animal food for an equal period, and another section had had this hebdomadally only, or on rare occasions. Weavers and shoemakers, none of whom earned more than seven shillings per week, and most of the women, had sustained, in addition to the other substractions stated, an under supply of the farinaceous food to which they were limited ; and, finally, every patient nearly in every class, had been shut up throughout a series of more than six months, to running the changes on two, three, or at most, a few articles of diet.

In a large number of the cases the patients had resided in damp houses, or were exposed, when at work, to cold and wet. About one-third of them were above 40 years of age, and others had been weakened by parturition, lactation, a mercurial course, previous attacks of acute illness, or the presence of the scrofulous diathesis, secondary syphilis, or other cachectic diseases.

Reviewing, then, these facts, abundant and obvious cause of the disease presents itself. A want of proportion between the elements in the food of the patients, and those demanded by their organism as omnivorous animals, is, I apprehend, demonstrated. An habitual and protracted diminution in the chyle of many, of nutritive constituents of every sort ; the absence from that produced in many more, of the nitrogenous principles of fresh animal food, and the extinction in it, in all, of the albumen, acids, and salts supplied by succulent vegetables, are all established. The coetaneous influences—positive and negative, of other depressing causes, as great physical labour, severe exposure to the weather, previous debility, &c., are equally shown ; and under such a disposition of the circumstances of the investigation, I would esteem it to be doing violence to every principle of sound sense still to profess dissatisfaction, as some do, and turning away from what is known and tangible, to seek the causes of the epidemic in the unknown and impalpable obscurities of an ærial constitution. I believe that the errors in diet which have been stated, and the want, in particular, of proper vegetable food, were the true exciting causes ; and that cold and other debilitating agents operated often as the predisposing causes of the disease.

Proximate Nature.—The essential principle in the proximate nature of this malady appeared to be diminished vital power; the immediate cause of which was probably the deterioration, as its earliest general effects were the irregular distribution, and, also, the transudation of the blood, or of either its serum or its globules. The external phenomena, again, to which these gave rise were general tegumentary anæmia, œdema, sometimes anasarca, and local congestions; while the character of these latter varied according to different accidental circumstances. Thus parts, the vitality of which is relatively low, as the adjacency of old wounds, and the skin on the inferior extremities; or which are exposed from position or function to pressure or other similar influence, as the soft parts of the extremities, the gums, the lungs, or bowels, were the first to manifest the disposition to hemorrhage, and also, on the exposure of the patient to cold, to assume a cachectic type of inflammation. This inflammation, again, though in its generic nature unhealthy, exhibited various specialities according to the tissue in which it was situated. Thus, when it affected serous and fibrous structures, a low kind of rheumatic inflammation was set up—fibrine and serum were effused, and, according to the locality involved, we had a pleuritic effusion, a synovitis, deep-seated neuralgic pains in the bones, or distended and impacted fasciæ of some of the limbs. When the superficial cellular texture, again, was its site, the inflammation assumed a low form of the suppurative, and gave rise to furunculi; while, when it laid hold on the mucous tissues, an unhealthy erysipelatous action was induced, by which sometimes the epithelium or cuticle might be seen elevated by bloody serum, into blebs, or the pilous or mucous follicles be converted into papules sometimes scarlet, at others, purple-coloured, and often, if on the cutis, passing into desquamation, or, when situated in the mouth and throat, or in the intestines, running into ulceration. There were two chemical analysis only made of the blood, and neither were very complete, but both of them, so far as they went, and the microscopical examination also of the same fluid, as well as the general features of the affection, indicated a generic affinity with those numerous species of cachexia, originating in various manners, the character of which consists, as a rule, in a diminution of the red corpuscles, and of the fibrine, with augmentation of the water, and often of the colouring matter of the serum; but which in exceptional cases, exhibit a relative, or even, when complicated with local inflammations, an *absolute increase* in the quantity of the fibrine of the blood.

On reviewing the epidemic in the aggregate, the occurrence of the disease in distinct groups cannot be mistaken. One variety distinguished by anæmia, emaciation, diarrhœa, dejections of fluid blood, dropsy, the circulation little affected, and the more distinctive symptoms of scorbutus wanting. A second by anæmia, often by diarrhœa, rapidity of the pulse, epigastric pain or oppression,

great general distress, an urticated crimson efflorescence on the skin, petechiæ and hemorrhages. A third by pains, most commonly along the course of the nerves, but at others, situated in a bed of muscle as the gluteal; the cases having a close resemblance sometimes to general rheumatism, and, at others, to ischias nervosa, morbus coxarius, or disease of the knee joint; their true nature being manifested by the sponginess of the gums, a perhaps slight ecchymosis only, the inefficiency of ordinary treatment, and the good effects of a full diet. And, lastly, the more ordinary form, in which affected gums and legs were the prominent symptoms. These subdivisions of the complaint had a conspicuous generic community with one another in their history, exciting cause, general aspects, and indications of treatment; but they appear to have arisen, each of them, under some distinctive combination of circumstances from which they had derived equally remarkable specialities.

Another fact which will not fail to attract notice on such a retrospect is, that cases of each variety of the disease appeared having the more peculiar and characteristic symptoms of some other of the forms associated with those of their own. Thus there are instances recorded in the journals of our wards in which the extreme cachexia of the first form, the hemorrhages and disturbed circulation of the second, or the pains of the third, were united in different degrees with more or less of the massive crural swelling, and muscular agglutination of the fourth.

The evidence on this head has seemed to me to be conclusive in regard to the identity of proximate nature in all the forms. Spanæmia,—thinness or poverty of blood, is their universal, and, I think, essential character, and although a measure of inflammatory action is often seen in the rheumatic and more purely scorbutic species, and sometimes also in the hemorrhagic, sufficient to cause them to tolerate bloodletting, if not even on rare occasions to require it at least locally, this is nothing more than what happens often in other forms of spanæmia, such as the complications of chronic albumenuria, and of typhus and other adynamic fevers.

Purpura hæmorrhagica, for example, is thus essentially a spanæmia, —sometimes, even, it is also an anæmia; yet in insulated cases the congestion or inflammation of some organ, as of the liver, does give a character to the constitution of the blood, and to the symptoms and appropriate treatment, which is at variance with the generic nature of the affection. In the necropsies of some such cases, the aorta has been found filled with a fibrinous cord; but the same fact holds good in multiplied instances of typhus. A cause of confusion on this subject, seems to be a supposition that purpura hæmorrhagica is always the same disease. It appears to me more correct to regard it as a symptom, or congeries of symptoms, rather than as itself a disease; and it is certain that it is developed in such different circumstances, and from such opposite causes, as do materially

affect the import and significance of its appearance. Thus, in one it is intra-uterine and congenital; in another, it does not appear till early childhood, but is mixed up with circumstances which characterise it as hereditary; and in both such there may be, for any thing which is at present known, disorder both of the solids and of the blood. In other instances the sanguineous effusions are more clearly the external sign only of the impoverished state of the blood, which has proceeded from a grave and protracted dietetical error, aided in some individual examples by the parallel influences of pregnancy and lactation; and, in another class, they are effects of the depressing agency on the vital powers of habitual exposure to a vitiated atmosphere, or to this, united with cold and damp. The gradual alterations set up in the constitution of the blood, by frequent blood-letting, or by hemorrhoidal or uterine discharges,—or yet again in albumenuria, or even in phthisis pulmonalis, or in the latter stages of different fevers,—do all terminate in spanæmia, and have each, either individually or conjointly, produced the development of purpura. The hemorrhagic effusions occurring in some as necessary and inevitable effects of the extreme rarity of the blood, produced by the more or less lengthened application of one or more of the exciting causes mentioned; and in others, as the sudden consequence of the accidental addition, in a spanæmic constitution, of an intercurrent cause, such as a blood-letting, a paroxysm of anger, or the setting up of constitutional excitement, by the scarlatinous, variolous, or other animal poisons, or even by such lesions, sometimes, as excite inflammation.

I apprehend that this distinction between the generic or simple constitution of purpura hæmorrhagica, and the specific complications of individual examples of it, is the real point of interest in the consideration of its pathology and treatment. In many cases, there is nothing present but a sense of lassitude, with circumscribed extravasations of fluid blood into the dermoid and areolar tissues; and in such the pathological condition is one simply of spanæmia, which will disappear with the removal of its exciting cause. In another series, nausea, bilious dejections from the stomach and bowels, acute epigastric pain, fever, tegumentary anæmia, and intestinal or other hemorrhages, are superadded; and in such cases the liver, sometimes the spleen and kidneys, and at others, the mucous surface of the intestines, or all these organs together, are found in some red and congested merely; but in others, more or less pulpy, and, when there have been profuse discharges of blood, also anæmic. These are examples of the congestive or collapsing form of the affection, which, like their analogues in scarlatina, typhus, and erysipelas, prove so often fatal, but which ought, nevertheless, to be met by the same general principles of treatment which are suited for those.

I shall conclude these observations on the proximate nature of
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the epidemical disease of which they treat, by the relation of another fatal example of one of its varieties, which has fallen under my observation since last month.

Mary J. Rogers, aged 24 years, a cotton-mill girl, June 2, 1847. Admitted in a state of great prostration at five o'clock p.m. Is of middle size and full habit. Is reported to have been of sluggish disposition, and says that for eight months, down to a week ago, she had not menstruated, but that she was then seized with sudden and profuse menorrhagia, which continues. Three days since her body became covered with an urticated purple rash, which is still observed of a dusky hue, and her lips, face, hands, and feet are livid. Pulse about 90. Pupils dilated. Gums and tongue pale, but sprinkled with livid spots. The food for the previous winter and spring had been porridge and butter-milk for breakfast and supper, with some tea after former meal. To dinner got sowins, or bread and butter-milk; sometimes tea, at others broth, and once a week had ham, and twice a week a halfpenny worth of sweet milk. She was ordered to be surrounded with jars of hot water, to have an ounce of wine and three grains of sugar of lead every hour, with a drachm of turpentine three times daily, and the tampon to the vagina. There was some appearance of reaction in a few hours, but it proved insufficient, and she died at nine o'clock next morning.

Inspection.—Twenty-eight hours after death. General aspect of body stout. An exaggerated form of cutis anserina over skin, especially on trunk. On abdomen some of the elevated points resemble papules, and when viewed with a magnifier, they appear vesicular. Few can be discovered on face, neck, or arms; but on the feet they are copious, and some of them umbilicated. They are pale, and seem to be cutaneous follicles. Trunk is the seat of a crimson efflorescence, and the arms and legs of scattered petechiæ, and some sugillations. Vulva covered by bloody incrustation, and orifice occupied by a clot, which reaches into vagina.

Head.—The scalp much thickened and cedematous, a quantity of slightly red serum escaping from it when cut. Both temporal, and the occipital bones, covered by large ecchymoses. Inner surface of skull pale, as were also the membranes. Cerebral veins filled with dark liquid blood, which readily escaped on removing the heart, leaving the general surface of the brain pale. A considerable ecchymosis at the edge of both hemispheres adjacent to the longitudinal sinus. The vessels of the brain, including those of the membrana choroidea, empty, and its substance universally soft.

Trunk.—A layer of fat, of about one-third of an inch in thickness, covered the abdomen. The muscles were very florid and soft, but without any extravasation; the external surface of the liver and intestines was pale.

Thorax.—The lungs weighed $36\frac{1}{2}$ oz.; they were without particular congestion, and although not putrified, they were friable in their texture. The inner surface of the bronchi pale. The heart weighed 8 oz. It was empty, very flaccid, and its substance pale and soft. The right ventricle was covered with fat. The right auriculo ventricular opening could transmit the whole four fingers, and that of the left the tips of three. The inner surface of the left cavities in particular was of a dark red colour. Left ventricle dilated into a pendulous bag with thin friable walls, capable of containing about 8 oz. of fluid. Aorta stained of a deep red throughout its coats.

Abdomen.—Liver weighed $44\frac{1}{2}$ oz. It was very pale and soft. Its granular or lobular structure was not discernible in any place, and in some parts it was reduced to a pulp. Gall bladder contained about 2 oz. of fluid, treacle-coloured bile. Spleen $4\frac{1}{2}$ oz. in weight, of a deep purple colour, and good consistence. The right kidney weighed 5 oz., and the left kidney 6 oz., and both were in a state of great granular degeneration. A few sparse ecchymoses in submucous coat of intestines. The inner surface of the femoral arteries, for more than a line in depth, was of a vivid florid colour, remarkable fibrous aspect,

and brittle texture; and this portion seemed to be made up by the serous, and an internal layer of the muscular, coats. Inner surface of veins also red. About 6 oz. of blood were extracted with difficulty from the body, and, subjected to analysis by Dr R. D. Thomson of the College, it was found possessed of the following composition:—

Water,	}	992·074
Globules,		
Albumen,		
Fibrine,	-	1·106
Salts,	-	6·820

I would now close this paper with a very few remarks on the treatment.

The indications of treatment which I pursued were, first, to restore the general vigour and health consistently with the particular circumstances; and, second, to attempt the removal of the local complications, such as the hemorrhagies and inflammations, on general principles, modified, of course, in every case by the character of the general health.

In the first form of the epidemic, the successful treatment of the cases was very hopeless. They were the product of long deprivation of adequate food and clothing, and not only resisted curative means, but were often injured by them. The most suitable diet was one of milk, with the addition of some light farinaceous substance, of one or two eggs daily, or a small quantity of wine. After a day or two's use of some such food, it was not uncommon to find fever kindled up; and, should a more stimulating diet have been employed, severe diarrhœa. The former effect was attempted to be removed by restricting the patients to weak tea and toast, or panada; and the latter by varied remedies. When the diarrhœa was more simple, a couple of ounces of the dried root of the *tormentilla erecta* boiled slowly in two pints of milk and one of *aqua calcis* to a pint, was used daily. In similar cases the *infusum hæmatoxyli* alone, or having the tormentilla added, was employed; and along with these as much even as three grains of the sulphate of iron every four hours, was given, with some aromatic, in a few instances with marked benefit; and in some other cases five or more grains of the sulphate of zinc three times daily, effected at least a diminution of the purging. In many patients with or without bloody stools, the sugar of lead, to the extent of about a scruple daily, was given for sometimes a couple of weeks continuously, without any bad effect or manifestation on the gums; and, in general, also with only limited good effect. The various preparations of catechu, kino, and opium, were also used, but the latter sparingly, as stupor was induced by even small doses. In all the cases, the tendency to congestion of the organs was sought to be obviated by the use of flannel coverings; warm fomentations, hot mustard pediluvia, and on a few occasions by the short application of blisters to the abdomen. When acute hemicrania, threatening as was believed, some effusion on the brain, presented, blisters were used; when dullness on percussion.

thoracic pain, or dyspnœa seemed to indicate congestion in the lungs, dry, and in rare cases, moist cupping, and sinapisms were employed; and, when the most prominent symptom was dropsy without any bowel affection, very striking amendment ensued on recourse being had to stimulating diuretics, as gin and squill, with a nutritious diet, and bandages to the limbs and abdomen.

In regard to the treatment of the second form of the epidemic, or the more purely hemorrhagic febrile variety, I can say little. I had three cases only under my own treatment which could be held to belong to this category. One was the congestive case of the girl Roger just related; and in her I sought to relieve the internal organs by determining to the surface, and by supporting the action of the heart; while I attempted the arrest of the hemorrhage by lead, turpentine, and the tampon. Another is still in the infirmary, but I trust out of danger. She was brought in about four weeks since in a state of extreme prostration of the strength, great rapidity of the circulation, acute epigastric and articular pains, constipation, spongy gums, with numerous petechiæ on legs, and œdema. She had warm fomentations, four ounces of wine daily, a couple of ounces of lemon-juice every eight hours, and a milk diet. The third is a boy, also still under treatment, although nearly fit for dismissal, who came in about three weeks since with sunken dejected countenance, inability to remain erect, a pulse of about ninety, bleeding gums, pains in limbs and some petechiæ and ecchymoses. His recovery has gone on steadily with no other treatment than rest in bed, and the usual diet of the scorbutic patients.

In reference to the curative means employed in the third or rheumatic form of the scorbutus, they were also sometimes restricted successfully to mere regimen and rest. These were experimental cases, but in others, warm fomentations, and, when the pains were not acute, warm frictions of camphorated oil were applied. Frequently the detraction of blood from the parts, and the use of repeated blisters were necessary; and, often, these were conjoined with quinine, cod-liver oil, anodynes, and a full vegetable and animal diet.

The remedial management of the fourth and most frequent species, or that with debility, contracted, swollen, and painful limbs, ecchymoses, and hemorrhages, has been more varied than that of any of the former.

Many of these patients were allowed a diet consisting of 8 ounces of oatmeal made into porridge, and taken with 14 ounces of unskimmed milk at twice, as breakfast and supper, or a corresponding quantity of sowins; and for dinner, 12 ounces of animal broth, made with vegetables, 4 ounces of fresh meat, and 2 ounces of turnips or carrots. Besides these edibles, each patient had an orange, and a pint of porter daily, and either two scruples of crystallized citric acid, or two ounces of lemon-juice, and half drachm of nitre every eight hours. To this general treatment was added constant fomentation

of the affected parts with thick pieces of wet hot flannel, covered over with thin wax cloth; and, so soon as the parts would bear it, the deep tissues of the limb were shampooed for half an hour, or more, twice daily in hot water, or with warm camphorated oil, and the contracted parts attempted to be methodically stretched and exercised. Sometimes the simple fomentation was exchanged for one of a solution of *urias ammoniac*; and frequently it became necessary to apply leeches for the removal of pain. Under this plan, some recovered in two weeks, more in three, and all (except one with pulmonary complications, and another with great contraction of the tendo Achillis, who is likely to take a couple of weeks longer), within the month.

Another group had a diet consisting of 48 ounces of unskimmed milk, 8 ounces of oatmeal made into porridge, and 24 ounces of wheaten bread made into panada, and one or sometimes two eggs daily. Such as were feeble or losing blood, had four ounces of wine, and all, except one or two, were allowed citric acid or lemon juice, and also local treatment similar to what has been described above. The dismissals here took place from the twenty-first to the thirtieth day. The patients all experienced an agreeable change in their feelings by the end of the first week, but about the third or fourth week they became stationary, and often required a fuller diet from the end of the first fortnight.

A third proportion of the patients had food of a different kind. It occurred to me that could a dietary be constructed, which would at once be so economical, that the worst paid of the working classes could reach it, and so constituted, that scurvy patients would recover under it, it might reasonably be expected that by its general adoption in future seasons, when neither fresh vegetables nor milk could be had, that the production of the disease might be prevented. Accordingly, a scale of diet was arranged of 4 ounces of oatmeal made into porridge, 4 ounces of peasemeal made into brose, and taken along with 4 ounces of wheat bread, 4 ounces of skimmed milk cheese, and 12 ounces of new milk, or, for three days in the week, 1 ounce of butter, or 1 ounce of molasses for breakfast and supper; while dinner was made to consist of four ounces of toasted cheese, alternately the half of the week, with 6 or 8 ounces of fresh haddocks, and a daily allowance of 16 ounces of wheat bread, one pound to one pound and a half of boiled cabbage, two ounces of vinegar, half an ounce of syrup, and a little salt. Some had three ounces of fresh butcher meat twice a week as an addition. Under this treatment some patients were dismissed well in fourteen days; others, in three weeks, and none have yet exceeded the month; and, presuming that *sour-croute*, having an aromatic as ginger added to it, and eaten warm, would prove an efficient substitute for the fresh vegetables here employed; and, also, that the condition of actual disease from which our patients were recovered, is a fair equivalent for the waste of alimentary matter occasioned

by the exertion of persons in health, it seems to me that some such dietary might in a future emergency be recommended to the working classes with every prospect of advantage.

ARTICLE III.—*Contributions to Pathology and Rational Medicine.*

By JOHN HUGHES BENNETT, M.D., Lecturer on Pathology and the Practice of Physic, Director of the Polyclinic at the Royal Dispensary, Edinburgh, &c.

No. VIII.—*On Exudation.* PART II.—*Its Development—continued.*

VASCULAR GROWTHS.

VASCULAR growths are formed by an increase in the dimensions or number of the arterial, capillary, or venous vessels. Several growths already described, as well as such as are of a cancerous nature, are very vascular;—indeed, so much so, that in some cases the slightest touch causes alarming hemorrhage, as in the case of so-called uterine polypi, and fungus hæmatodes. No doubt there is considerable increase of vascular growth in such tumours, but their basis is formed of other material,—they are not wholly vascular. This term is more properly applied to those diseases which have hitherto been denominated *aneurism*, *erectile tumours*, and *varix*.

Aneurism is an arterial swelling, which may vary in size from the slightest possible dilatation of the calibre of the vessel, either wholly or partially, to the formation of enormous tumours, larger than the human head. In all such cases, we find the tumour to consist externally of the dilated and hypertrophied structures of the vessel itself, or the tissues in its immediate neighbourhood, whilst its substance is composed of blood, more or less coagulated. The morbid anatomy and pathology of these growths is so well known, as to render a description of their several varieties, in this place, unnecessary.

Nævi, or *aneurisms by anastomosis*, are blue or claret-coloured stains, often raised above the surface so as to form tumours. They are generally soft; for the most part situated in the subcutaneous tissue, the skin covering them being of unusual delicacy. When compressed, they may be gradually emptied of blood, which returns like water into a sponge on removing the pressure. For the most part they are congenital. When the arteries are numerous in them, they have a brownish or reddish colour, and pulsate during life. When the veins abound, they are of a blue or purple colour. Their texture consists of numerous capillaries, more or less distended, mixed with arteries and veins, the interstices of which are filled up by areolar tissue. A section presents a cruciform appearance, owing to the open mouths of the vessels divided. In one case of aneurism by anastomosis in the brain, we found the intervacular tissue loaded with earthy salts.

Varix is a permanently enlarged vein. Swellings from this

cause may exist in various parts of the body, but are most frequent in the saphena veins of the inferior extremities, the spermatic veins (*varicocoele*), and hemorrhoidal veins (*hemorrhoids*). In all these cases the veins gradually enlarge, and then become distended, tortuous, and coiled up. Several of these, accumulated together, may produce knotty swellings in the legs, cause the testicle to assume an unusual size, or produce tumours which, during stool, are protruded beyond the margin of the anus. Such growths may ulcerate, and cause death by hemorrhage, or they may be spontaneously obliterated by the formation of clots within them.

Vascular growths, for the most part, consist of hypertrophy by dilatation, no new structures are produced, with the occasional exception of such as arise in the clot of blood within them, which may be converted into fibrous tissue, or into a calcareous mass. Sometimes the vessel becomes obliterated, and assumes the structure, density, and appearance of ligament.

New vessels constitute one of the most common pathological formations. They exactly resemble the old ones in structure, but how or in what manner they originate, is not yet positively determined. The observations which have been made in connexion with this subject, have led to three theories. 1st, That new vessels are of independent origin, and that they, as well as the blood they contain, spring up in a blastema according to the general laws of cell formation. 2d, That the globules of the blood escaping from the vessels, channel a way, through the surrounding exudation, and then form new vessels. 3d, That the walls of the vessels themselves at particular places present bulgings and irregularities, which become pushed out more and more by the *vis a tergo*, and so form new channels. An inquiry into this subject is surrounded with difficulties. No certainty however can be arrived at as to the mode or modes in which new vessels are formed, without more extended series of exact observations than we as yet possess.

CARTILAGINOUS GROWTHS.

Cartilaginous growths were first described by Müller, under the name of *Enchondroma*. When formed in the soft parts, they are surrounded by an envelope of cellular tissue, when in the bones by a bony capsule. In the first case they occur, although very rarely, in the glands as in the parotid or mamma. In the second case they are most common in the bones of the extremities. When formed in the substance of long bones, they present rounded, smooth tumours, when in the periosteum or flat bones, their surface is rough and nodulated. They are rarely met with.

In structure, enchondroma presents all the characters of cartilage. That is nucleated cells varying in size, isolated or in groups, situated in a hyaline substance. A network of filamentous tissue runs through the substance of the tumour forming areolæ, in which blood-vessels ramify. Within the areolæ so formed, the cartilage

is found. These two elements vary as regards amount in various tumours. Sometimes the cartilage is in excess when it resembles that in young animals, or that in the fœtus. At others the fibrous element abounds when the whole is similar in structure to fibro-cartilage. Between these two extremes there are infinite varieties, and sometimes it presents all the characters of articular cartilage. We have seen all these varieties in one tumour.

Not unfrequently a bony nucleus may be observed in a nodule of enchondroma, and sometimes they may be observed in all stages of transformation into perfect bone. Some of the exostoses, to be spoken of immediately, are probably owing originally to an excess of cartilaginous growth.

Enchondromatous tumours are continually mistaken for cancerous growths, as was pointed out by Müller. They are usually denominated *osteo-sarcoma*. Not unfrequently they soften, and under such circumstances, present all the external characters of what is called soft cancer. This softened portion, even when examined microscopically, may lead to error, as the cartilage cells which then float loose, mixed with granules and the debris of the tumour, very much resemble those in cancerous growths. They may be distinguished, however, by the action of acetic acid, which renders the whole corpuscle more faint, instead of producing as will be hereafter shown to be the case in cancer, a marked difference between the external cell wall, and the nucleus.



Structure of an Enchondromatous Tumour of the Humerus, for which Mr Syme amputated the arm at the shoulder joint. Fig. 1. Section of a firm cartilaginous nodule. The right of the figure exhibits above mineral deposit in and around the cells, and below some isolated cartilage corpuscles. Fig. 2. Exhibits the effect of Acetic Acid, rendering the whole, and especially the nucleus, more transparent. Fig. 3. Shows the cartilage cells, and fibrous tissue, isolated, broken up, and mixed with numerous molecules, in a nodule of soft and almost diffuent consistence.

OSSEOUS GROWTHS.

We have seen that in many of the cartilaginous growths deposit of bone may take place to a greater or less extent. In such cases, the new cartilaginous tissue undergoes the true bony transformation in the same manner as normal cartilage becomes ossified in passing from the foetal state through the periods of youth, manhood,

and old age. This we must separate from the numerous forms of calcareous concretions so frequently met with. True bone may be at once recognised by its osseous corpuscles and canaliculi. Earthy concretions only consist of an amorphous mass of mineral material.

Osseous growths may affect the external surface, the substance, or the internal surface of bone. In the first case they are denominated *exostoses*. They form prominences on the surface of the bone varying in size from a small point to that of a cocoa nut. There is no part of the osseous frame free from them, but they are very common in the bones of the extremities. They may arise as the result of direct local injury, as from a blow or fall, or they may be connected with peculiar constitutional diseases as syphilis or rheumatism. In syphilitic constitutions, exostoses more especially arise on the shafts of the long bones; in rheumatic persons, they surround the joints.

Bony growths may more especially affect the substance of bones, and this in two ways. An exudation may be poured into the cancelli of the osseous texture, which is gradually transformed into perfect bone. From this cause its substance becomes much indurated and of great density, and the cancelli and medullary cavity are more or less obliterated. We frequently observe this in the long bones of the inferior extremity as well as in the flat bones of the cranium. Some of the latter have thus become upwards of an inch in thickness, and on section presented the close texture and density, although not the structure of ivory. Sometimes, however, the bones, instead of being condensed and thickened, become spongy, the cancelli enlarge, and the whole assumes unusual lightness. In this case, the exudation poured into the cancelli is transformed into pus, and acts as a distending power, and sometimes collects in the central cavity, causing at the same time expansion and hypertrophy of the surrounding osseous tissue. This constitutes the *spina ventosa* of authors. In such cases, the external wall of the bony tumour may be thin or unusually thick. In the Musée Dupuytren at Paris is the preparation of a tibia, the head of which is the size of a child's cranium, six months old. In some parts of its circumference, it is very thin and perforated by openings for the escape of matter. In the Museum of the Royal College of Surgeons, Edinburgh, the head of the tibia is expanded nearly to the same size. The bony walls, however, are upwards of an inch thick, surrounding a central cavity, in which pus collected, and was evacuated from time to time through an opening an inch in diameter.

Bony growths are sometimes thrown out on the internal surface of bones. This occurs in a peculiar disease first described by Rokitsansky in puerperal women. We saw this production frequently in Berlin, on the internal surface of the cranial bones, in the numerous dissections which occurred in the Maternity Hospital of that city during an epidemic puerperal fever which raged there in 1840. Unfortunately, they were not examined microscopically.

The internal table of the skull in all these cases was so soft, that the knife could readily penetrate it. These deposits, when dry, assume a granular laminated aspect, more or less curled up and separated from the internal lamina of the cranial bones. We have examined very fine specimens of this lesion in the pathological museums of Prague and Vienna.

The growth of new bone after fractures or injuries, takes place in the following manner:—An exudation is poured out from the vessels in the neighbourhood, which at first unites the lacerated edges of ruptured periosteum, muscle, and cellular tissue, so as to form a capsule around the whole of the denuded and injured bone. This exudation, at first granular, is partly transformed into fibrous tissue, and partly into compound granular corpuscles, which may be observed to form an internal coating to the capsule just alluded to. The blood extravasated is rapidly absorbed, and a gelatinous exudation, which is poured out from the neighbouring capillaries, collects between the capsule and denuded bone. This, at first yellowish, becomes gradually lactescent and white, and assumes all the characters of cartilage. This cartilage, in its turn, is transformed into bone, by exactly the same process as the one structure passes into the other in the normal state. As solidification takes place, the soft parts are absorbed and contracted, whilst the bony deposit insinuates itself between and around the fractured bones, producing complete union.

Certain textures have been occasionally transformed into true bone. We examined the preparation of an eye at Munich, in the possession of Professor Förg, which contained an osseous mass, attached internally to the choroid and fibrous structure of the sclerotic, and encroaching considerably on the space usually occupied by the vitreous humour. A thin section of it exhibited numerous bony corpuscles. We have seen true bone formed in the substance of the dura mater, where it has been exposed by removal of a portion of the cranium by the trepan. The osseous laminae, sometimes found on the surface of the spinal arachnoid, also possess the true bony structure. Ligaments have occasionally been transformed into osseous texture (Henle); and Dr Wilkinson of Manchester lately communicated to the Pathological Society of that city an instance where numerous muscles of the body had undergone a like transformation.

In all these cases the osseous-looking deposit was proved, by a microscopic examination, to be formed of true bone. They are analogous in structure to the bony masses occasionally found in fibrous or enchondromatous tumours, and must be separated from earthy concretions, which frequently resemble them to the naked eye. Of these latter, whether amorphous, or assuming a regular form from accidental circumstances, we shall speak more at length at a future period under the head of Concretions.

On Subluxation of the Humerus, Forwards and Inwards. By CHARLES HENRY HALLETT, Demonstrator of Anatomy in the University of Edinburgh.

It is my intention to detail, in this notice, the different circumstances connected with partial dislocation of the humerus forwards and inwards, two examples of which have been examined in the dissecting-room of the University. I am led to do so because the morbid appearances and general characters of this subluxation are unnoticed in the works especially devoted to the consideration of such injuries, probably on account of its not having yet attracted the attention of the surgeon, or of the surgical pathologist. A well-marked instance of it was met with, about three months since, in the right upper extremity of a man, who had been evidently possessed of great muscular strength during his life-time. On examining it, I found the lesions in the parts about the shoulder-joint to be curious and important, and to differ greatly from any I had previously read of. It was immediately remembered that a precisely similar state of matters had been previously observed in the dissecting-room, and that a preparation of the parts concerned in the lesion had been placed in the Anatomical Museum. This preparation was examined, and found to present the same appearances as that more recently made. A cast of the bust of the subject had been made before the dissection of the extremity was commenced, and thus I was enabled to observe more particularly the nature and extent of the deformity caused by this form of subluxation, to compare it with that resulting from other forms of luxation of the shoulder-joint, and to discover that the lesion was attended with signs so characteristic and defined as to render its diagnosis at once both easy and certain.

On these accounts, I shall enter into the consideration of all the circumstances attendant on the displacement, and detail the observations I have made concerning it; beginning, for reasons which will be evident as I proceed, with the morbid appearances.

The articulating surface of the head of the humerus was displaced entirely from the glenoid cavity of the scapula, was thrown forwards by extreme rotation outwards of the arm, and was driven inwards so as to overhang and project into the subscapular fossa, and to cause the protrusion forwards of the anterior wall of the axilla. The head of the humerus was prevented from gliding in the glenoid cavity during the execution of any of the movements of the limb, in consequence of striking changes in its form, which I shall attempt to describe. A large segment of bone had been removed from the external and posterior part of the head of the humerus, and from that portion of the anatomical neck which intervenes between the head and the greater tuberosity. More than half an inch had been ground down or absorbed to the level

of the shaft; and this had been so regularly and evenly performed, that it appeared rather to have been effected by mechanical means, as by a saw applied at first longitudinally, and then transversely through the whole depth of the head of the bone, so as to remove a prismatic portion of it, than by the increased action of the absorbents induced by constant attrition of the anterior lip of the glenoid cavity against the anatomical neck and head of the humerus. The whole of the depressed surface was covered with cartilage, and presented here and there a number of grooves of a brownish colour, where the cartilage was apparently deficient. These grooves indicated so many places in which the absorbing action was exerting its influence in removing the bone immediately before the individual expired.

The loss of this portion of bone had caused a material alteration in the configuration of the head. It no longer possessed its rounded hemispherical form, but had become irregularly ovoidal. The head of the bone appeared at first sight to have been considerably elongated, and this elongation seemed to have caused the alteration in its shape; but, on closer examination, the appearances were found to be deceptive. A large depression, therefore, existed on the inner and posterior part of the humerus: this depression presenting two surfaces, one longitudinal, which impinged against the anterior smooth surface of the neck of the scapula; the other transverse, which rested on the anterior half of the glenoid cavity, the corresponding lip of which was received into the angle formed by the meeting of these two surfaces. The glenoid cavity had not escaped from the effects of attrition, for that portion of the anterior lip which projects somewhat into the axilla, and is the strongest part of the cavity, had been absorbed, and thus reduced to the level of the surface of the neck. I might briefly state that the glenoid cavity in a perfect scapula is somewhat pyriform, that its inferior two-thirds represent a segment of a circle, whilst the superior third is a segment of an ellipse. Now, in both the cases I have examined, the projecting border of the circular portion had been removed and brought on a level with the ellipsoid portion, so that the glenoid cavity and the neck of the scapula had an elliptical form, which appeared like the change in the form of the head of the humerus, to have arisen from an increase of their longitudinal diameter; but I found from admeasurements, that this diameter was not increased—that it bore its usual relation to the size of the scapula. Besides these, some other changes require to be mentioned. The greater tuberosity of the humerus was situated in the posterior half of the glenoid cavity. It had its form altered in such a manner as to permit it to perform the movements which the joint, in its existing state, would admit of. The three surfaces into which the supraspinatus, infraspinatus, and teres minor muscles are inserted, were all merged into one smooth surface covered by the tendons of these muscles. The lower part of

this surface, which projected somewhat from the upper, chiefly occupied the glenoid cavity.

All the muscles inserted into the superior extremity of the humerus were involved in the displacement, and were all more or less elongated or shortened. The supraspinatus, infraspinatus, and teres minor muscles were all shortened to an appreciable extent. The pectoralis major and the subscapularis were elongated and on the stretch. The former set of muscles had their tendons of insertion flattened out and confounded with one another. The tendon of the subscapularis was probably thicker than usual at its lower part. The long tendon of the biceps, instead of passing over the centre of the head of the humerus, coursed over the superior border of the greater tuberosity and entered the bicipital groove, which, with the tendon of the biceps, was nearly in a line parallel with the posterior border of the glenoid cavity. The capsular ligament of the joint, and the tendon of the pectoralis major, assisted by a slight increase in the depth in the bicipital groove, the consequence of the absorption of a part of the greater tuberosity, prevented displacement of the long tendon of the biceps, a circumstance very likely to have happened, considering the relation of the parts, had not nature made some efficient provision against it.

There was no effusion of blood in or around the shoulder joint; no abnormal development of ligamentous tissue, nor any other change but such as I have mentioned, external to the joint. The lower portion of the capsular ligament of the joint, which had not the slightest appearance of having been ruptured, was united to the apparently thickened tendon of the subscapularis muscle, and with it, supported the head of the humerus as in a sling, and bound the greater tuberosity firmly down to the glenoid cavity.

Such are the morbid appearances and alterations seen in and about the two joints I have carefully examined. The older specimen differed from the more recent one, in one trivial and unimportant circumstance, namely, that a very small portion of the greater tuberosity of the humerus had been detached from it, and was found embedded in the capsular ligament. It was also taken from the right superior extremity of a male subject.

The articulating surfaces of the shoulder joint having undergone such extensive changes, we must expect the motions, usually performed by it, modified in a corresponding degree. Rotation, circumduction, and abduction were all in abeyance; adduction was imperfectly performed; and flexion and extension were the only movements that could be executed by the limb in any thing approaching the normal way. The position and changes in the muscles, and the manner in which the glenoid cavity and the head of the humerus were locked together, united to cause this paucity of motion. Thus, the arm could not be rotated inwards, because the longitudinal surface of the depression on the head of the

humerus abutted immediately against the neck of the scapula, and controlled all motion in that direction; the attempt to rotate outwards was equally ineffectual, for the subscapularis and pectoralis major muscles were already too much stretched to permit the external rotatory muscles to act efficiently; circumduction, depending on the power of moving the limb freely in every direction, could not be performed for the same reasons; every attempt at abduction was successfully resisted by the subscapularis, pectoralis major, and by the longitudinal surface of the depression on the head of the humerus. The other movements, not being restricted or controlled in any way, took place for the most part in the usual manner. It is to the power of flexing and extending the limb, whilst the other movements were imperfect, that we must refer the production of the depression on the head of the humerus.

The deformity occasioned by this form of sublucation greatly resembles that induced by complete dislocation forwards of the humerus. The acromion process was prominent and angular; and the deltoid muscle was flattened, whilst the anterior wall of the axilla was rendered prominent by the head of the humerus pushing it forwards—characters also peculiar to the dislocation forwards. Indeed on comparing casts of these two dislocations, it was found that the external characters about the shoulder were so similar and corresponded so closely that it would be impossible to distinguish them at first sight. The surgeon, however, would have no difficulty in recognising this sublucation, since, on placing his fingers in the axilla, he would discover the head of the humerus in the immediate vicinity of the glenoid cavity, the borders of which he would be unable to feel; moreover he would find the limb everted to a great extent; the hand and arm being turned from the body, and looking outwards, whilst the elbow was placed against the hip; and would be unable to rotate it inwards, provided the displacement had been of long standing. These characters, which were repeatedly noticed by myself and by Mr George Tate, the gentleman who was dissecting the limb, were absent, in every other form of dislocation, complete or incomplete, of the shoulder joint, and will hence serve as a ready and certain means of diagnosis. It is necessary for me to mention, that the deformity also resembles that occasioned by partial dislocation forwards of the humerus, but the circumstances of the limb being everted, and of flexion being perfectly performed, in the sublucation forwards and inwards, will point out the means of distinguishing them. I may also state, that these two forms of sublucation of the humerus differ from each other most materially in the changes which occur in and around the joint after the displacement has occurred; what these differences are need not be pointed out; suffice it to say, that if we are to be guided by the recorded descriptions of the dissections of joints in which sublucation forwards has been detected, there is not the least similarity between them, in any one respect.

Observations such as these, made in the dissecting-room, are necessarily and unavoidably imperfect. The practical anatomist is seldom able to trace the previous history of the individual on whose body the observations are made, and if, by chance, he can trace it, it is generally but for a brief period, and scarcely ever reaches that at which the lesion occurred. Both individuals from whom the foregoing facts were obtained, were unknown, so that we are left entirely in the dark respecting the causes of the displacement. I cannot, however, refrain from attempting to deduce them from the data afforded by the circumstances with which we are already acquainted. A blow or a fall insufficient to cause complete luxation of the head of the humerus, might still be accompanied with sufficient force to drive it into the abnormal position it was found to occupy in these two cases. I have no doubt that the displacement may be caused by direct injury, but I am also led to believe that it might originate in another manner. There is a trial of strength performed in Scotland, and, I believe, in Scotland only, which those who practise it, designate by the unmeaning phrase of "putting you down." It consists in two individuals seating themselves opposite each other, joining their right hands together in a peculiar manner, closely approximating and fixing their elbows on a table, or any other stationary object, and then attempting to twist each others arms down to the object on which their elbows rest; the individual who can twist outwards his antagonists arm in this amicable manner, into the required position, being declared the victor. Now, I conceive that this trial of strength may induce subluxation of the humerus forwards and inwards. During its performance all the muscles of the arm and shoulder are violently and continuously contracted. If the arm should then be suddenly and violently rotated outwards and extended, we might expect that the muscles so suddenly stretched would suffer considerable injury. I believe that the subscapularis and pectoralis major muscles might be rendered inert by this violent over extension, and that these muscles being inactive and the force being continued, the head of the humerus might be readily displaced. The head of the humerus can certainly be thrown into the abnormal position before described by rotating the arm suddenly outwards after the pectoralis major, and the subscapularis muscles have been detached from their humeral attachments. I have tried the experiment several times in the dissecting-room, and have never failed to displace the head of the humerus from the glenoid cavity, and to twist the greater tuberosity into it, without rupture of the capsular ligament. We have only to presume that this can be done, under certain circumstances, in the living body, and we have all the conditions necessary to induce the deformity, and the changes observed in the articulating surfaces of the shoulder joint. The pectoralis major, and subscapularis being unable to act from over extension, and the

external rotator muscles being shortened to a considerable extent, and having no resistance to overcome, the head of the bone would be retained in the abnormal position into which it had been forced, whilst the flexor and extensor muscles, not having been involved in the injury, and still performing their functions, would cause those movements, by which the thread of the bone would be bound down or absorbed, in consequence of the border of the glenoid cavity, rubbing against the anatomical neck of the humerus which is unprotected by cartilage. The work of destruction once commenced, the muscles, although they might have recovered their tone, would be unable to re-instate the head of the humerus in its proper position. This is merely conjectural on my part, but unless we assume the muscles to be altered in the manner I have indicated, it would be impossible to account for the production of the alterations in the bones. They are observed to be altered in that manner, after the alterations in the bones have occurred; whether they are primarily in that state cannot be ascertained. Eversion of the arm could alone induce such changes, and hence I am led to believe there is some connexion between the very evident eversion of the limb seen in the cases I have detailed, and the violent and forcible twisting outwards of the arm, practised in the trial of strength to which I have referred. Professor Syme, I understand, has found it a prolific source of dislocation of the radius, may we not, therefore, be justified in supposing that the injury is occasionally inflicted on the shoulder instead of the elbow joint, the condition being similar?

Little can be said about the treatment to be adopted by the surgeon, for the restoration of the perfect use of the limb, if he should chance to meet with a case of this subluxation in a living individual. The luxation may be reduced, by lifting the head of the humerus out of its abnormal position, by the aid of a towel, the arm being rotated inwards, and carried across the trunk; and when reduced, might be treated according to the common principles of surgery, although, it is more than probable, that the displacement would remain permanent, in consequence of the changes in the joint, unless it came under the notice of the surgeon at an early period.

Before concluding this imperfect notice, I would remark, that whilst the opinion I have advanced respecting the cause of this subluxation is purely hypothetical, the changes induced by it, and the characters by which it may be recognized, are taken from direct observation made on two well marked cases. My only intention in recording these observations, is to bring under the notice of the surgeon, the existence of and the character by which he may distinguish this subluxation of the humerus, and under the notice of the surgical pathologist, the occasional existence of lesions in the articulating surfaces of the bones entering into the formation of the shoulder joint, which have been hitherto undescribed.

ARTICLE V.—*Remarks on Scurvy as observed in Cumberland, and the Southern parts of Scotland.* By HENRY LONSDALE, M.D., F.R.C.P. Ed., Physician to the Cumberland Infirmary, &c., &c.

THE history of scurvy, interesting to the physician and no less gratifying to the philanthropist, is often cited to show the advantage of well-directed hygiene, and the superiority of modern medicine over that of less favoured times. Though very few practitioners of the present day have seen scurvy, all seem pretty nearly agreed that the disease has its origin in the absence of fresh vegetables, and that a restoration to these, or the free use of fruits of the genus *citrus* of the *aurantiaceæ* will speedily establish health in scorbutic patients. As medicine is a progressive art, and men are now taught the advantage of viewing things in a broader light, it need not surprise us to see dogmas set aside, and principles, apparently well established, called in question by a wider and more searching philosophy. Much praise is due those who set themselves the noble task of observing disease *de novo*; even when, as in scurvy, history to all appearance had completed its task. I am led to these remarks from a perusal of the opinions published in the last number of this Journal by my esteemed friend and preceptor, Professor Christison, who, not content with the views of approved authors on the subject of scurvy, has attempted a further generalization, or at least introduced some novel and curious facts bearing significantly on any discussion which may hereafter be entered upon in relation to this disease.

During the past winter, and more especially about the beginning of the present year, I frequently observed amongst the out-patients of the Cumberland Infirmary, and occasionally in those of better circumstances in respect to food and clothing, a deteriorated condition of the system with no very specific, or localizable disease. The parties presented an unhealthy complexion, and complained of great languor, listlessness, and debility, irregular dyspeptic symptoms, diarrhoea, and more frequently dysentery. The legs were sometimes swollen, œdematous and painful, and the most trifling sores were slow to heal. Rheumatic pains were common, and in women of less robust habit menorrhagia was not unfrequent. These and other ailments became associated in my mind with what the older authors would have termed a “cachectic condition.”¹

¹ It may be well to mention here that disease has been much more common than usual amongst cattle in the neighbourhood of Carlisle, and, as in the human subject, much less amenable to treatment. Veterinarians have not been able to specify the nature of the epidemic, beyond mentioning the great frequency of *Pleuropneumonia*. As far as I can learn the “bleeding plan” which ought to obviate pneumonic disease is losing credit, and not a few of the sensible class of farmers seeing the fallability of the art, eschew physic entirely, and commit

Depletives of all kinds could not be borne, tonic medicines on the other hand were in great request, and fulfilled the best indications. On conversing with Mr Thomas, our active house surgeon, on the prevalence of disease and its peculiarities, during the month of March, he expressed his doubts as to the character of a few cases where the parties complained of contusions, sprained ankles, &c., and gave me reason to apprehend that scurvy had shown itself amongst the applicants for *surgical* relief. At that time I had not seen scurvy, nor did any come under my notice till the 20th of April, when a girl of 18 years of age applied as an out-patient. Three days subsequently, two railway labourers were brought to the infirmary from the "Summit Huts" on the Caledonian Railway, a distance of nearly 50 miles. One of these men, aged 43 years, rapidly fell a victim to the disease. I abbreviate the house surgeon's report of the case.

His face is swollen—lips livid—ecchymosis about the right eye. Such was his difficulty of breathing, that when carried from the conveyance into the Infirmary, he could not walk above two or three steps. The sounds of the heart were feeble, and a short bruit accompanied the diastole: respiratory sounds, healthy on the anterior and lateral parts of chest, but inaudible behind. This man who gave but an imperfect account of himself, said he had been able to work until about a fortnight ago, when his illness commenced with difficult breathing and swelled legs. After being placed in bed his breathing became more and more embarrassed, and he died six hours after admission.

A *post-mortem* examination of the body, eighteen hours after death, shewed effused blood in the muscles of the abdomen, and along the course of the veins at the lower part of neck: there were also patches of ecchymoses over the convexity of the transverse colon; but these and incipient granular degeneration of the kidneys were the only morbid appearances. The state of the gums and the knee-joints, and a more detailed history from his companion proved his disease to have been scurvy. The long journey and great exertion were more than his debilitated system could bear up against,—hence the suddenly fatal termination.

My attention was now fully awakened to the subject, and the continual accession of cases from the Caledonian Railway, and outdoor applicants in the persons of labourers, shoemakers, weavers, factory-girls, &c., afforded ample scope for investigating the disease in all its relations. I was fortunate also to secure the co-operation of medical friends in the neighbouring towns and rural districts, only two of whom had seen scurvy previously, and some

their animals to the more tender mercies of nature. It seems pretty certain, however, that the cause of disease amongst the brute creation is not as yet traced to any error in food, whilst the prevailing opinion is in favour of atmospheric influence aiding, if not originating morbid action.

of them naturally had their doubts as to the supervention of a disease which ranked rather amongst "the things that were," in the eyes of landsmen.

So much has been written on the history of scurvy, that I do not feel myself justified in doing more than state that the symptoms presented the true features of Scorbutus, or Sea-Scurvy, such as have been already given in the June number of this journal, p. 878. As worthy of notice I may mention, that hemorrhage was common from the gums. Epistaxis was less frequent. In two cases blood was passed *per anum*, and in one case, but only in trifling quantity, from the bladder. A tendency to menorrhagia was also observed. The stiffness of the joints completely disabled a large number from walking, whilst pains in the joints were at times unusually severe. The embarrassed breathing on the slightest exertion does not seem to have been so particularly noticed by writers on the present epidemic. It was well marked in the severe cases which came under my observation, and to show how readily it was induced, a patient in bed attempting to lay hold of an object rather beyond his reach, had a fit of difficult breathing, which lasted an hour and a half. There was nothing abnormal in the respiratory sounds in any of the cases. The urine occasionally showed uric acid. It was difficult to say whether the gums or limbs were first affected.

These remarks are meant to embrace a general survey of what has been observed of scurvy, in a district presenting very varied circumstances in reference to the locality and dwellings, no less than the occupation and living of the individuals affected. What can present a greater contrast in the artizanship of life than the handloom weaver of pallid complexion and slender frame, worried with a large family, ill-fed and worse clothed, following his trade in low confined apartments, often in close proximity to noxious animal effluvia, and the railway-labourer, active, athletic, iron-framed, generally without a wedded charge, and the "little responsibilities," well fed, and warmly clothed, and with his avocations inhaling the "incense breathing morn?" These men may be almost viewed in the light of antipodes to each other in this life of labour; yet they appear to suffer equally from any infringement of that law which points out variety of food as essential to the maintenance of healthy action in the human economy.

To make these remarks as concise as possible, it seems advisable to arrange in different groups the persons affected, treating of the prominent facts in each group, *e. g.* the habits and food previous to the attack, the duration of the disease, and the result of medicinal agents employed in the treatment.

It is by no means an easy matter to arrive at correct data regarding the exact quantity of food used by any class of people, whilst those who depend upon manual labour for their subsistence, will naturally in periods of scarcity be obliged to make the kind and quality subservient to their means.

In Carlisle and its immediate vicinity, the persons chiefly afflicted were weavers, and their wives and daughters working in the factories, shoemakers, and comparatively few of any other class of artizans. They lived in inferior dwellings, complained of the severity of the winter, want of clothing, and short allowances of food. Bread, oatmeal, treacle in very small quantities, tea and coffee with an occasional herring formed their entire food. None had tasted potatoes after the harvest of 1846, or for a period of seven or more months, excepting one who remembered Christmas day, from the fact of potatoes forming his principal meal on that festival of the Catholic Church.¹ The bread was variable in quantity, but never exceeded the demands of the appetite,—in short their food was deficient in quantity, miserably so in variety, and altogether incompatible with the maintenance of vigorous health and strength. Few had made use of milk to any extent, even in the most favourable circumstances for obtaining it, whilst the majority said they had no liking for milk, or that it did not suit them. I was particularly careful in my inquiries on this subject, and from my knowledge of the kind of diet generally adopted by this class of people, the information obtained did not appear remarkable. The lower classes here do not sufficiently appreciate the value of milk, viewing it rather in the light of a drink or vehicle for solid food, and therefore too expensive, except in the rearing of children. On being closely questioned as to the cause of their ailments, not a few adduced a “different living,” the want of animal food, but the most striking circumstance in their diet was the want of potatoes, which had been the staple food at dinner, and often at supper. The deficient supply of this esculent had been manifested for two years previously, but never entirely failed till the last season. It therefore became less a matter of surprise that the more intelligent of our patients attributed their unhealthy state to a deficiency of the potato crop. Before offering any decided opinion on this subject, let me remark, that women debilitated with suckling, or of a menorrhagic tendency,—people of both sexes suffering from sores or burns, or who had taken two or three doses of mercury, or laboured under disease which enfeebled the constitution, seemed peculiarly liable to the inroads of scorbutus. Whatever lessened the vital powers, developed or hastened on the disease.

This cannot be said of the railway excavators, who in general lived on beef or mutton, salt bacon, suet puddings, bread and butter, oatmeal porridge and treacle, tea and coffee, and occasional potations of ale or beer. The exact quantities devoured by

¹ St Jerome and the early Fathers might have anathematized the person guilty of such gastronomical remembrance of a holy festivity; in our days, however, the “dignitaries of the Church” appreciate too highly the

“Fair round belly with good capon lined,”

to interfere with the *stomachic* privileges of her majesty's lieges.

these muscular fellows could not well be ascertained, but that there was no deficiency of the food used is pretty evident from their repeated statements. A personal inspection¹ of their breakfast and dinner tables enabled me to verify these statements. They assured me that they often consumed a pound of beef at a meal.

The barren mountainous country through which the Caledonian Railway passes for a long distance, precludes the purchase of milk during any part of the year, whilst in the more cultivated districts from Lockerbie southwards, the supply is so scanty that it can hardly be viewed as forming part of the dietary of the excavators.

On questioning these men as to their fondness for, or use of milk during periods antecedent to their being engaged on this line, I learned that none had been in the habit of using it for the last five years, or so rarely as not to be noticed in any inquiry *quoad* their food. A majority of these excavators were Englishmen belonging to the northern counties. As they constituted a large body of men foreign to the district in which they carried on their labours, the farmers did not consider it politic to increase their stock of cows for a temporary purpose, moreover the rural population were generally intimidated by the outrageous behaviour of these workmen, and did not solicit their favours. Taking these circumstances into consideration, it is reasonable to infer that the quantity of milk consumed by these people was of very small amount. The replies elicited by my repeated questioning confirmed the above view; and the report of Dr Smith and Mr Cockburn, of Moffat, is to the same effect. Mr C. writes, "few of those affected have been in the habit of using milk."

In the most southern part of Dumfriesshire, the Gretna district, scurvy has been very prevalent since January; indeed, I believe that here it showed itself sooner than in any part of the wide locality referred to in these remarks. Here the farms are large, and agriculture is conducted with great skill. The land is not much above the level of the high-water mark of the Solway Firth, as it

¹ I visited the numerous huts from Moffat to the "summit" on Sunday the 2d May, along with Messrs Thomas and Cockburn. In a great number of the huts we saw the men breakfasting of beefsteaks or mutton chops and bread. The dinner comprised bread, boiled beef, or bacon, pea soup or broth, and suet puddings containing currants. The animal food was taken in large quantities, though the men had not had the benefit of labour or exercise (owing to the day and the heavy rains), to improve the appetite. I occasionally found the bread stale, and the butter rancid; the "tommy shops" were heavily complained of. The uncooked beef and mutton seemed good, perhaps less fat than usual. The sleeping apartments were much too small, ceilings low, and ventilation imperfect.

The site of the huts was not well chosen, either they were too near the "Avon stream," or on mossy wet soil. In the last mentioned sites, fever made greatest ravages.

runs up the "Kirtle" and "Sark" rivers. The villages of Gretna, Springfield, and Rigg of Gretna, are close to the English borders, and consist of a few straggling houses, inhabited mainly by weavers and field labourers.

In this, as in all rural districts, the farm-servants and cottars have milk twice a day at least, but the other division of the labouring population, the artisans and families, are not favoured with milk beyond the summer and autumnal months, the rest of the year they use treacle-beer to their porridge. In this district, through the kindness of Mr Carruthers, surgeon, I had the opportunity of observing well-marked cases in both sexes, in cottars who were in the habit of taking milk daily, to the extent of from 10 to 32 ounces; and in artisans, who were obliged to use treacle-beer in lieu of milk for eight months in the year. Mr Carruthers informed me that potatoes have always formed a staple article of food in this district, comparatively little animal food being used, fresh fish and salt bacon occasionally. Oatmeal in various forms is daily consumed. During three winters the potato crop has been failing, but the supply was never entirely cut off till last autumn and winter. In the case of milk there had been no manifest deficiency. Here then, in the Gretna district, a number of people, pretty nearly 100 in all, who, in respect to locality, dwellings, and labour, were the same in 1846-7 that they had been for years previously; but being deprived of that which had long occupied a prominent position in their diet, namely the potatoes, could no longer withstand that breaking down of the constitution which terminated in scurvy. Had the artisan classes been alone affected they would have afforded a strong confirmation of the opinion so ably advocated by Christison; but the fact of many of the agricultural class, who were daily in the habit of using milk, being equally diseased, makes the cause, if an error of diet (and there is no other apparent cause), to rest with the potatoes. The following cases may be cited as additional instances to those recorded above. A gentleman of middle age, highly distinguished for his agricultural attainments, who had not used milk for more than 20 years, became afraid, in September last, of eating potatoes, of which esculent he was formerly fond, lest it should induce disease, and suddenly attached himself to cocoa, arrow-root, and other farinaceous food,—the quantity in all being deficient for the healthy standard. In April 1847 scorbutic symptoms appeared, and I prescribed for him oranges, beef tea, and a more liberal allowance of food generally. He was restored to health in a fortnight. A clergyman in the same vicinity was also afraid of using the potatoes last autumn. His living in other respects was the same as in other years. He became scorbutic about the latter end of April last.

A sailor in the light-ship off Maryport, on the west coast, placed himself under my care in May. He had not tasted fresh vegetables for more than eight months. In other respects his food was the

same last year as it had been for four years previously, during which period he had been a strong healthy person.

An agricultural labourer who abandoned potatoes entirely in August 1846, continuing his small quantities of milk, and the saccharo-farinaceous food so often alluded to, became so severely affected with scurvy, that he could not be raised to be shaved without fainting; his death was daily expected. Can anything be more striking than these cases, exhibiting the cause of the disease?

Dr John Graham, of Brampton, nine miles east from Carlisle, reports of his cases of scurvy, "the most general fact respecting the diet is the total want of potatoes and fresh vegetables—then comes the total absence of fresh animal food—salt herrings and bacon being the only substitute for it." Again he says, "It appears that the patients, all of whom were paupers, had in former years had potatoes, and though not accustomed to much milk, the quantity not exceeding three or four ounces daily, had generally more than during last season." In a third of Dr G.'s cases no milk had been taken for two or more years.

Dr George Mein describes the food of the scorbutics in Cannobie, a rural district near the English borders, as consisting of tea, coffee, porridge, bread and butter, no meat and no vegetables. The frost had spoiled the turnips and cabbages. Milk he reports to be "scarce every winter in Cannobie." Dr Weir, an old practitioner in the district, does not think the supply of milk differed at all from ordinary years, and attributes the cause of scurvy to the want of the potato and other fresh vegetables.

Dr Bogie of Annan, Dumfriesshire, has seen between 90 and 100 cases amongst the pauper class, and railway excavators on the Nithsdale Railway. He had previously seen the disease on board of ships. In reference to the present epidemic, he informs me that he "could trace no connexion between milk and the disease." A number, but the proportion he could not specify, made use of milk in the summer months. The food appears to have been very similar to that recorded above, namely, saccharo-farinaceous; the absence of potatoes and fresh vegetables being very prominent. Dr Bogie describes the pulse as very languid, and "never emptying itself," and that mercury with chalk improved the pulse by relieving the hepatic circulation and producing bilious stools. Contrary to the experience of writers, he found the physiological action of the mercurial necessary, in some cases, where nutritious food, and his favourite remedy,—salicin (10 or 12 grains daily), had failed in their ordinary beneficial effects in this disease.

The average duration of the disease under salicin and nutritious diet was rather more than a month.

Dr Walker of Annan had treated a dozen cases. He attributes the disease to the want of potatoes and fresh vegetables.

My highly esteemed friend, Dr Dickinson, who has had more experience of scurvy than any one in Cumberland, reports that the

disease had not shown itself in Workington (a seaport town of 7000 inhabitants on the west coast), and assigns as a reason, "that vegetable food was more abundant there than in many situations, particularly turnips, of which large quantities were used."

A few days ago, my talented friend, Dr Browne of the Crichton Institution, Dumfries, told me in conversation, that he had scorbutic cases generally every spring, and that during the present season the disease had been much more common, in all probability, from the want of fresh vegetables, as the parties were not without a fair supply of milk. Dr Grieve, who joined us in conversation, could not trace any connexion between milk and the disease in his abundant experience of scurvy as now prevailing in Dumfries. Both gentlemen agree as to the absence of potatoes being the chief cause, whilst they were inclined to attribute something to atmospheric or other influences which a more extended experience may unravel.

Though the diet of the scorbutics under my care have been fully considered, it is right to state, in reference to Carlisle and its vicinity, that milk was not quite so abundant amongst the poor in the early part of winter, owing not so much to the diminution of supply occasioned by diseased cattle, or lack of fodder, as the fact of provisions generally being almost double the price of ordinary years, thus compelling the adoption of weak tea and coffee. In former years, for instance 1842-3, &c., the "murrain" was common, and milk was then more scarce, and abandoned by many lest it should induce the same disease in the human system,¹ yet there was no approach to scurvy during that scarcity of milk. It is, indeed, a striking fact, as an intelligent medical friend writes, "that of all the years under the sun, that marked by a dearth of potatoes should be chosen for the advent of scurvy," and, I may add, amongst a class hitherto considered exempt from its attacks. Whilst this clearly points out something else than the want of milk as the *origo mali* of the present epidemic, I am in no wise doubtful, but a copious supply of this bland nourishment would be, in many cases, an useful anti-scorbutic, owing to its possessing *per se* those proximate principles proved by Prout, and others, so essential to nutrition.² And I can

¹ It deserves remark, that the poor are always more prejudiced against doubtful articles of diet than those in more easy circumstances. The cry against "murrain milk" was strong during the period. During the present spring it required great and persevering efforts to persuade the starving hundreds to adopt the use of rice, hominy and other substitutes for potatoes.

² There must be *something* in the potatoes; they enable Irishmen to do a great amount of work. Paddy pays us an annual visit in the harvest, and prefers a "potful o' praties" to the best roast beef which we can set before him. Butter-milk is very well in its way, but we don't churn butter every day of the week. Don't boil the potato to affect the centre much, and Pat will tell you that the "hard mate" and a pickle of salt, "barring the milk," will do "rite well."

Let chemists look to this. The "heart of a potato" may make an English Liebig of some enterprising fellow.

fully comprehend its great services in turning the tide in favour of health, when a previous diet had been faulty, either in variety, as seen at Perth, or quantity, as too often met with during last season.

TREATMENT.—I have already spoken of the severity of the cases under my care in the Infirmary, and may further add that not one of them was able to chew animal food, or even soft bread, in addition to their being incapacitated from walking from one room to another. Their appetites were good. They were ordered a pint of beef-tea, a pint of porter, 12 oz. of bread and the same quantity of milk *per diem*, also ʒi or ʒij of citric acid and lemon-juice with 4 oz. of infusion of gentian and water as an acid drink. They soaked the bread in the beef-tea, and relished the bitter infusion and acid drink amazingly. The worst cases had two oranges daily. Nothing in medicine has delighted me so much as witnessing the happy effects of this plan of treatment. Four excavators were placed in a ward by themselves, labouring under scurvy in the most severe form. On the third day after admission, these fellows began to speak in a cheerful tone. Hemorrhage from the gums had ceased, the joints were less painful and less swollen, and no fresh ecchymoses had shown themselves. On the sixth day, the gums enabled them to chew, and other ameliorations had gone on *pari passu* with the gums. On the ninth day they were all sitting up, and enabled to walk along the room; on the 12th they became useful in cleaning the walks; and on the 14th day, presented themselves to the committee of the institution, and returned thanks for what one of them, the spokesman of the party, chose to call “a miracle effected on them.” The weavers did not make such great progress under the same treatment:—from 20 to 26 days being required to effect a cure in them. The excavator, who accompanied the fatal case, remained in the house 40 days. His diet had been no better than the poorest of our scorbutics, and he was not of the most healthy character.

The average duration of the out-patients to whom an abundant supply of the citric acid and gentian mixture was given, and who dieted as they best could of fresh vegetables, was about 25 days. The smallest alteration of diet had a wonderful influence in checking the disease, so that I am not surprised at the accounts given by Lind of a “few pickled cabbages now and then” affording immunity from the disease. Those who obtained oranges advanced most rapidly. So much was I convinced of the value of oranges, that on being called to an excavator who had lost more than three pints of blood by epistaxis in less than three hours, and was reduced to extreme weakness both in body and mind, I prescribed oranges *ad libitum*. He was badly nursed, and got weak broth, and little farinaceous food, but his recovery was complete within the twenty days. A woman less severely affected than the majority

was cured with a shilling's worth, about 15 oranges! The lightship sailor who came to me on crutches got well on oranges in 14 days. The agricultural labourer, who could not have his head raised without fainting, was cured with oranges and porter, and citric acid, small quantities of each, in about 15 days. He was "the miracle" of the district in which he lived, as his friends had given him up for lost.

In the list of treatment adopted by others, I see the mineral and vegetable acids, bark, wine, beef-tea, milk, occasional purgatives and diaphoretics, oranges, potatoes, &c. It is often noted by my obliging friends that the scorbutics only had a few potatoes, or fresh vegetables, or milk, and soon rallied; even a good supply of nettle broth or beer cured slight cases; again and again showing that the slightest change of diet or drink was beneficial, and that in proportion to the nutritious quality of the food along with variety was the rapidity of the cure.

As vegetables became plentiful, scurvy disappeared from amongst us. I do not hear of fresh cases anywhere in Cumberland at this date (July 9th.)

Conclusion.—On weighing well the circumstances related above, I am led to make the following inferences. 1. That as the vegetable world became more or less blighted, man in common with the higher class of animals suffered, from causes not well understood, but apparently of an epidemic nature, which have deteriorated his condition, and made him the more ready victim to scurvy, fever, &c. 2. That scurvy originates from an error of diet as generally believed,—the occupation, dwellings, &c., sometimes viewed as collateral causes having little or no influence. 3. That a deficiency of potatoes constitutes the chief error of diet, and is the main cause of the present epidemic, whilst the absence of variety and deficient quantity of food hastened the development of scurvy. 4. That the use of milk, as might be anticipated from its proximate principles, lessened the liability to scurvy, but did not prevent its occurrence:—its powers in correcting a monotonous diet have been acknowledged in the list of remedial agents.

P.S.—The present epidemic of scurvy contains a warning, which concerns the government—the profession, and the public. Let us hope that it will not be lost sight of.

Carlisle, July 9, 1847.

Part Second.

REVIEWS.

1. *Report on the Fever at Boa Vista*, by Dr M'WILLIAM. Presented to the House of Commons, in pursuance of their Address of the 16th March 1847. Folio, pp. 112.
2. *Letter addressed by Sir WILLIAM PYM to the Lords of the Council, relative to a Report on the Fever at Boa Vista*, by Dr M'WILLIAM. Presented to the House of Commons, in pursuance of their Address of May 14, 1847.

THE eventful story of the *Eclair* steamer must still be fresh in the recollection of our readers. It will be remembered that she reached Portsmouth in the end of September of 1845, last from *Boa Vista*, one of the Cape de Verd Islands, after having lost by fever, within a short period, sixty-five out of a crew of 146 officers and men; that there were still twenty-three sick on board; that she had lost her captain, her surgeon, and assistant-surgeon, besides two surgeons who had subsequently volunteered aboard; and that of the entire ship's company only forty-one had escaped being attacked. It will be remembered, also, that alarming fever first appeared on board this ship during her voyage from Sierra Leone to Gambia and *Boa Vista*, and that she was permitted by the Portuguese authorities to land her entire crew at the latter place; but that, as the fever still continued to spread among the crew on shore, it was deemed advisable that the crew should be re-embarked, and that she should proceed to England.

The two parliamentary documents before us refer to an inquiry subsequently committed to Dr M'William by the Board of Admiralty, apparently in consequence of information that a fever, resembling that which prevailed in the *Eclair*, had broken out among the inhabitants of *Boa Vista* soon after the steamer had sailed from that port.

Dr M'William was sent out to *Boa Vista* in February twelve-month, with instructions to inquire into all the particulars of the fever which had affected the island, and in an especial manner, whether that fever had any connexion with the visit of the *Eclair*. The document drawn up in consequence by Dr M'William, consists of several official communications, of the evidence of about 120 persons residing on the island, Europeans and Africans,—officials, merchants, soldiers, labourers, washer-women, and the like,—relative to the kind of intercourse between the crew of the *Eclair* and the inhabitants, and to the subsequent origin and progress of

fever on the island,—of general observations by Dr M'William on the information obtained—on the climate of Boa Vista—on African fever—and on the fever of the *Eclair*. The following passage is so important, that we must give it in Dr M'William's own words.

"The main conclusions, from a review of the whole of the circumstances that were brought under my notice relative to the '*Eclair*' at Boa Vista seem to be,—

"First, That the fever on board the '*Eclair*' was primarily the remittent of the African coast, which is not a contagious disorder, but that the disease acquired contagious qualities in virtue of a series of causes.

"Second, That although there exists on the Island of Boa Vista a physical cause capable of producing remittent fever, yet it does not appear that that cause was in action when fever broke out in September 1845, and that the island was quite healthy when the '*Eclair*' arrived there.

"Third, That the disease of which the Portuguese soldiers died at the Fort (Duke of Braganza), on the small island, was that which afterwards ravaged Boa Vista, and the same as that which prevailed among the crew of the '*Eclair*.'

"Fourth, That the fever was propagated throughout the island almost exclusively by direct intercourse with the sick, there being only two cases in which there appears any probability of persons being infected in any other way.

"Fifth, That although those who had passed through the fever were much less liable to the disease than those who had not, yet it would appear that a person having had one attack, possesses no absolute protection against a second attack.

"Sixth, That connecting the whole of the circumstances attending the arrival and stay of the '*Eclair*' at Boa Vista with those under which the disease appeared on the small island, and afterwards on Boa Vista itself, leaves no doubt of its having been introduced by the '*Eclair*.'

"Seventh, That in all probability the mortality from fever on the island was much increased by the want of proper nourishment for the people, as well as by the total absence of medical assistance for some months.

"Eighth, That the disease had in no case spread to any of the other islands of the Cape de Verd Archipelago."—Pp. 111, 112.

We should add, that Sir William Burnett, in a letter to the Secretary of the Admiralty, contained in the document before us, says—

"Dr M'William appears to have taken considerable pains to gain information; but after a careful perusal of the papers he has sent I am compelled to say that I cannot conscientiously arrive at the conclusion the Doctor has done, viz., 'That the fever was occasioned by the intercourse with the *Eclair*, which is, however, the chief point on which we differ; agreeing with all my late reports to their Lordships on the same subject; and also in conformity with a former report of mine to their Lordships respecting the case of his Majesty's ship *Bann*, in the year 1824."

On the other hand, Sir William Pym, in the second document on our list, to use his own words, considers "Dr M'William's Report to be a most valuable document; and that the variety of uncontrovertible evidence brought forward by him, relative to the disease in question, has finally decided and set at rest a most important and long-contested question relative to the nature and history of yellow fever, more particularly as to its infectious power."

From Sir William Pym's letter we quote also the following passages, to remind our readers of the views he entertains on the fevers incidental to hot climates.

"For many years a controversy has been carried on by medical men relative to the infectious or non-infectious nature of the yellow fever; a controversy originating from an opinion that the marsh or remittent fever, and the disease known as yellow, Bulam, or black-vomit fever, are the same disease; this last, in the opinion of non-contagionists, becoming, from a vast variety of causes and circumstances, a concentrated remittent or malignant yellow fever. This controversy I trust is now finally settled by Dr M'William's report; and that the question ought not to have been upon the subject of contagion or non-contagion, but upon the existence or non-existence of two very different diseases. At page 75, Parliamentary Papers, I have stated that there are two distinct diseases prevalent on the coast of Africa; the one the remittent fever, from which our seamen suffer so severely in consequence of boat service on the rivers. This is the same disease as the well-known Walcheren fever, the malaria of the Levant, and the jungle fever of India, and exists in all warm climates, in moist and uncultivated grounds. Any person who has had one attack of this fever is very liable to a second, and afterwards to attacks of ague. This fever is not infectious.

"The other disease is a very different one; it is in no way connected with malaria or unhealthy situations. It is unknown in the East Indies, in Egypt, or in Turkey, and is a native of and peculiar to the west coast of Africa, as the *Pestis Bubonica* is to Turkey and Egypt.

"First, It is a disease *sui generis* known by the name of African, yellow, or Bulam fever, and is the *Vomito Prieto* of the Spaniards, from its being attended with the peculiar and fatal symptoms of black vomit, a symptom which rarely if ever appears in the marsh or remittent fever.

"Second, It is highly infectious.

"Third, Its infectious powers are increased by heat, and destroyed by a certain degree of cold.

"Fourth, It attacks natives of a warm climate in a comparatively mild form.

"Fifth, It has also a singular and peculiar character, viz., that, like small-pox, it attacks the human frame but once.

"This disease, from the west coast of Africa, has been at various times imported into different islands and countries, viz., to the different West India Islands, the Island of Ascension, and to different ports of Spain and North America, as in the year 1845 into the island of Boa Vista."—Pp. 4, 5.

The few particulars from the documents before us which we have just given, cannot fail, we think, to interest our readers in a high degree. The whole account of the Eclair, and, in particular, the results of the inquiry into the health of Boa Vista, ought to awaken an earnest attention, on the part of all who have the credit of the profession at heart, to the unseemly violence of contention which still too often marks the opinions even of sensible men, with regard to the origin and spread of some of the fevers of hot climates.

The point which most urgently presents itself for consideration is, why it should happen that diversities of opinion marked by such violence, we had almost said animosity, prevail on what in all strictness is a part of sober medical science. It is true that in ruder times the learned were accustomed to indulge in the utmost license of recrimination, even when their differences ran on what concerned the exact sciences. But it is humiliating to think that any remains of this leaven of barbarism should still attach to debates relative to the conditions under which grave facts like diseases arise. And yet, perhaps, all surprise should cease when it

is remembered how numerous the medical body is, and how many belonging to it are impelled to write on the spur of the moment by the force of unusual circumstances, without any previous preparation by reflection on what constitutes medical probability, logical inference, or certainty of fact, their sole qualification being too often a dauntless spirit and a determination to place the subject of their observation on what seems to them a satisfactory footing. But how few points in medical science are to be carried by a *coup de main*? The natural course of improvement is at the best zig-zag, and so if a subject be pressed forward in a straight line, the longer will the retrograde direction be, which at last must be entered on. As regards the extent to which fevers are contagious, it is clear to all the world that the acrimonious debates of the last half century (to go no further back) have left the question in pretty much the same state as at their commencement. The enthusiasm of Chisholm, the ingenuity of Bancroft, the self-sufficiency of M'Lean, the zeal and industry of Jackson, Fellows, Pym, Burnett, and a host of others, have had no other effectual bearing on the question than to convince all sensible people of the necessity of some new mode of penetrating such mysteries than disputatious wrangling and sharp encounters with the weapons of sarcasm. Medical facts are seldom of easy proof, and none are less so than those which relate to the rise and spread of epidemic diseases. But when to the inherent difficulties of such points the distortions incident to human passion are added, how is it possible that any progress can be made towards an insight into truth? And besides the distortive vehemence which professional disputations have introduced into such inquiries, there is the more secret influence of the zeal on one side and on the other, of those interested in a pecuniary point of view respectively in the maintenance and in the abolition of quarantine. The quarantine question hardly deserves to be regarded as a medical investigation. It has plainly been hitherto a long struggle between the mercantile interest, whose property is heavily taxed for quarantine dues, in addition to the evils of detention, and the quarantine officers who profit by the number, the extent, and the rigour of the quarantines declared at their respective ports. How must medical facts fare under such an ordeal!

While we confess that we have hitherto numbered ourselves with those who have regarded contagion as unknown among the fevers at least of tropical climates, we cannot but feel our former belief much shaken by the results of Dr M'Williams' inquiry. We feel the necessity of abandoning all decided opinion on the subject, and, if we could, we would willingly persuade the rest of the medical world to do the like, and to regard Dr M'William's inquiry as the first of a new series of observations, on which no final judgment should be pronounced till sufficient data shall have been collected to set the question as much at rest as the question in

Europe regarding the contagiousness of continued fevers, or that of the non-contagiousness of periodic fevers now is.

In the view, then, which we are disposed to take, we think the following propositions nearly represent the logical points of the case.

1. That the whole subject of contagion as respects the fevers of hot climates (including the plague) should be regarded as a *tabula rasa*, as a part of etiology, in which the ascertained facts are totally inadequate to warrant a decided opinion.

2. That no advantage can result to medical science from discussions in which the merits and demerits of existing quarantine laws are debated.

3. That since small-pox, syphilis, and other diseases, are freely communicable in hot climates, there is no *a priori* ground for the belief that fevers cannot be contagious in such climates.

4. That there is no sufficient evidence of the opinion that all contagions are specific, as maintained by Bancroft, that is, that all contagious diseases never arise or are propagated otherwise than by contagion.

5. That though the contagious fevers of Europe lose this property when persons labouring under them are conveyed to hot climates, it does not follow in obedience to any acknowledged principle, that there should be no contagions capable of propagating fevers in such climates.

6. That it does not follow because the ordinary fevers of hot climates are not communicable by contagion, that there are no fevers in those climates which are neither contagious nor capable under unknown circumstances of becoming contagious.

7. That if the contagious fevers of hot climates be of much rarer occurrence than the non-contagious fevers of analogous type, the difficulty of establishing the existence of contagion must be great, as is proved by the example of those parts of Europe where contagious fevers have been less frequent during considerable periods of time, thus giving rise to a temporary disbelief in the existence of febrile contagion.

8. That the more or less perfect protection afforded by an attack of contagious fever against future attacks, is an important element in the investigation of the etiology of fever in all climates.

9. That the effect of the perfect ventilation, due to the openness of the houses in hot climates must be a powerful means of restraining the ordinary operation of contagion in the production of fevers—on the supposition that it really exists.

Beobachtungen und Untersuchungen über den rasch verlaufenden Wasserkopf. Mitgetheilt von Karl Herrich. Regensburg. 1847.

Observations and Researches on Acute Hydrocephalus. By CHARLES HERRICH. Regensburg. 1847, pp. 278. Small quarto.

THE work before us, founded upon eighty-one cases of acute hydrocephalus, observed by the author himself, is valuable alike for descriptive accuracy and for the importance of the inferences deduced. Premising that all those cases are to be ranked under acute hydrocephalus, in which, while unusual cerebral symptoms are met with in the last stage of life, at least one ounce of watery fluid is found in the cerebral cavity after death, Herrich goes on to give an account of his various cases, occurring from the age of three months to the seventy-second year. Thus the reader is enabled to study, as it were, with his own eyes, the various modifications which this Protean malady exhibits in different individuals. In another chapter the author gives a very instructive analysis of his cases on the numerical method, in as far as concerns the age, sex, season of the year, temperament, previous condition of health, symptoms after death, and the relation of the functional symptoms to the organic productions. We have room but for a few of these results.

Acute hydrocephalus is known at every period of life; but its frequency, during the first four years, is at the least equal to that throughout all the other periods of life. It is more frequent in the male than in the female sex, and in many instances is congenital. Functional derangements commonly precede the disease, particularly when the predisposition is congenital—chest-symptoms being most frequent, head-symptoms more rare, and abdominal symptoms still less frequent. The period of these antecedent symptoms is generally for one or two weeks only, and seldom extends to three weeks. The commencement of the disease itself is characterized in the majority of instances by headache and vomiting, sometimes by convulsions and chills. The most common symptom is stupor, though in most cases incomplete; among the less common are sleeplessness, restlessness, convulsions from time to time, and these last occur particularly in childhood and in the last stage of the disease. Anormal fluidity of the blood contained in the heart after death appears to stand to the convulsions in the relation of cause. Convulsions seem also to have a connexion with anormal size of the mammæ. Tubercles of the brain on the other hand seem but rarely to be connected with convulsions. The most common *post-mortem* appearances come in their order of frequency as follows; accumulation of water in the cavities of the brain, tubercular deposits and organizable exudations on the membranes, softening of the walls of the cerebral cavities, firm adhesion of the pia mater to the surface of the brain, hyperemia of the cere-

bral membrane and substance. Plastic exudations of the serous membranes occur most frequently on the arachnoid; less frequently on the pleura, still less frequently on the peritoneum; and these in the first mentioned are oftener of recent origin, in the other two membranes of older date. As to the connexion of functional symptoms with organic alterations, paralysis and convulsions were found oftener on the right side; the same being true of tubercles in the chest, while morbid alterations of the cerebral cavities prevailed more on the left side. Thus the head symptoms, paralysis and convulsions, were opposite to the cerebral lesions. The same holds good with respect to the paralysis of the eye-lid and lower extremities.

The whole work is undoubtedly a valuable addition to the literature of hydrocephalus, or rather to that of serous effusion within the cerebral cavities, though we must doubt if the number of cases examined, great as it is, be sufficient to warrant the reception of the results, just enumerated, as general truths.

On Dyspepsia, with Remarks submitted in support of the opinion that the Proximate Cause of this and all other Diseases affecting the general System is Vitiation of the Blood. By JOHN BURDETT STEWARD, M.D., Fellow of the Royal College of Physicians, of London. 12mo. pp. 106. London: 1847.

THIS little book is not as its title might suggest, a dry argument in behalf of the humoral pathology. We must indeed say, that the chapter devoted to the proximate causes of diseases in general, is but a brief summary of the *prima facie* evidence in favour of the participation of the blood in the diseases of the general system—a proposition which we have no intention to controvert. But the author has been himself a dyspeptic,—and his book professes to give his experience in the treatment of himself as well as of others. In so far the observations are valuable, though we are far from thinking that dyspepsia can always be cured by one kind of regimen or treatment, and hence are naturally jealous of having what is true as regards one individual applied to all the rest of mankind. If our author can do for dyspeptics in general what he says he has done for himself, we shall account him a most valuable member of society. “He can eat, drink, and sleep like other persons, and no longer fears the moderate enjoyment of those social comforts, which, whilst they renovate the strength and fit us for exertion, stimulate the sullen to gaiety,—make the lively more cheerful—dissipate the cold formality which often enshrines talent; and, in the refinement which attends it, enables us to forget the animal necessity.

“The cynic may curl his lip, the wise may moralize, but, as little will the latter be able to dissuade, as the former to deter, the great mass of mankind, from believing, that good eating and drinking form, at least, part of the recreations of life, and that the inability to participate in them is no less a privation.”

Part Third.

PERISCOPE.

ANATOMY AND PHYSIOLOGY.

NEW MUSCLE IN THE EYE.

THE *Oest. Med. Woch.*, No. 40, mentions that in the human eye, and also in that of the mammifera, Dr Brüche states that he has discovered a new muscle. This muscle appears in a manner to have the same power as that of the iris. It consists in the grey ring which is found in front of, and in the neighbourhood of the iris on the external surface of the choroid, and which has been usually known under the name of the ciliary circle. Brüche gives to this muscle the name of the expansor muscle of the choroid.—*Gaz. Med. di Milano*, Marzo 6, 1847.

ON THE FREQUENT OCCURRENCE OF ALKALINE URINE IN HEALTH, AND THE ERRORS OF DIAGNOSIS OCCASIONED BY A WANT OF KNOWLEDGE OF THIS FACT. By Dr ADOLPH KRUKENBERG. Brunswick.

THE fact—first promulgated by Wöhler—that the internal use of salts of vegetable acids and fruits containing them, causes the urine to be secreted alkaline, has been too much neglected by succeeding physiologists and pathologists. Our author found that a much smaller quantity of fruit was necessary for the production of this phenomenon than has hitherto been supposed, viz.—2 to 4 oz. of apple pulp, or 12 plums, weighing without the stones scarcely $1\frac{1}{2}$ oz., sufficed to make the urine alkaline and hazy from phosphates, or if clear on excretion, heat caused their deposition; the addition of a little hydrochloric acid caused an effervescence like champagne; too much liquid, a bladder already filled with acid urine, or a disproportionate allowance of flesh, interfered with the success of the experiment. How often are those ill of chronic complaints who use a moderate diet, and with whom fruit is a useful and favourite article, troubled with hazy and alkaline urine, causing anxiety alike to themselves and their physician, which a little physiology does away with. In the simple chronic nephritis of Rayer, the chief symptom is the alkalinity of the urine; in no case was there a *sectio cadaveris*; and some of the cases recovered so quickly, as to justify a doubt as to the correctness of the diagnosis; although he inculcates careful dietetic treatment, it is evident from his work that the semiotic influence of fruit in small quantities was unknown to him. This article is not forbidden at La Charité, and friends of the patients often carry them some. In several of his cases the alkalinity of the urine seemed to depend on purulent admixture, and consequent rapid putrefaction; and in one it seemed to be kept up, if not produced, by the use of an alkaline saline water (Contrexeville). The alkalinity of the urine has also been used by Prout as a diagnostic sign of certain spinal affections. These he divides into two great classes:—1st, Those arising from depressing emotions and weakening influences; and in these he recommends the use of fruit, and fluids containing malic acid, as cider and perry; to these, and not to any disease, our author refers the alkalinity of the urine. 2d, Injuries of the spine; our author states, that neither Rayer nor himself had ever been able to observe the

urine alkaline in cases of injuries of the spine unless there were some existing or consecutive affection of the mucous membrane of the urinary passages, producing purulent admixture, hastening thereby the putrefactive changes in the urine. In the three cases detailed by Prout, two had stricture of the urethra, and the third retraction of the testicle, and a mucous sediment,—all bespeaking the existence of some such affection. A microscopical examination, by showing the existence or absence of pus cells in the urine, would have confirmed the diagnosis, or at once corrected it. How far inattention to diet may have led to error, cannot be specified. Prout also mentions, without explanation, what has been already referred to,—viz. That although alkaline urine, by copious secretion, be clear and bright, yet boiling causes it to deposit a phosphatic sediment, which falls without any such previous process, if the secretion be more sparing; the phosphates separate before the boiling point, and from their great specific gravity fall rapidly, and may thereby, as well as by their solubility in acids, be distinguished from the albumen found in Bright's disease.—*Zeitschrift für Rationnelle Medizin*, III. Bd. 1, ht.

ON THE PERISTALTIC MOVEMENTS OF THE ŒSOPHAGUS. By Dr F. WILD, Capel.

THIS is a long and interesting paper, prefaced by a sketch of the anatomy of the parts, and including a detailed account of a series of experiments, for which we refer to the original. It having been found impossible to produce reflex movements of deglutition, so long as the will remained paramount,—the animals (dogs) were narcotised by injecting an alcoholic solution of opium into the external jugular vein. All voluntary power over the muscles was thus destroyed, while their capability for reflex action remained uninjured. Irritation (by gentle rubbing with the fingers) of the posterior surface of the velum palati, the arches of the palate or the superior and posterior portion of the pharynx, produced, in most cases, contractions, varying in extent; in some cases, merely slight local twitches; more frequently contractions of the directly irritated muscles, combined with constriction of the pharynx, and elevation of it and the tongue; wry distortions, or finally complete peristaltic movement of the Œsophagus. In weak animals, the susceptibility for reflex action was soon exhausted. That this depended on fatigue of the sensitive nerves, rather than on that of those of motion, or of the muscles themselves, was shown by the occurrence of automatic movements of deglutition. A short period of rest sufficed to restore the susceptibility; a short pause frequently intervened between the movements of the pharynx and those of the cervical portion of the Œsophagus,—the transition could often be facilitated by placing a ball of wax at the root of the tongue, or within the constrict. fauc. infer. After complete exhaustion of the usual seats of reflex action, water poured into the mouth excited a series of very lively movements of deglutition; yet, failure of attempts to produce reflex action, after removal of the water, showed that here there was no restoration of the pharyngeal irritability. Irritation of the mucous membrane of the cervical portion of the Œsophagus, produced only local contractions, ceasing on withdrawal of the irritant, except in one solitary case, in which they recurred for some time with intervening pauses. In three cases, simultaneous local irritation of the mucous membrane and muscles, produced peristaltic movements; yet, even in these three animals, each similar attempt was not successful, while each irritation of the pharynx was so. The peristaltic movement consists in a simultaneous shortening and narrowing of the Œsophagus, extending gradually from above downwards, relaxation not occurring until the motion have extended one or two inches beyond. No part is sprung over; each is subjected to this movement, the duration of which is uncertain, and may be prolonged by any opposition to the descent of the bolus. Where this movement is feebly propagated, it frequently, from unknown internal causes, ceases about the middle of the neck, with a few wry distortions. This cessation can be brought about by stopping the descent of the bolus in such a manner as completely to

prevent muscular contraction at the point pressed on, till all motion in the part above has ceased, otherwise the movement of deglutition passes on, although the bolus remain behind. Cutting across the œsophagus, preserving, as far as possible, the nerves and mucous membrane intact, or division of single corresponding nervous twigs on each side, so as to produce paralysis of the smallest portion of the œsophagus, without injury to its structure, also prevents the further propagation of the motion, and this even although the bolus be pushed past the paralysed portion into that still irritable. Longitudinal incisions, even an inch long or square,—portions excised out of the anterior wall,—in no wise hinder the propagation of the motion, so long as no branch of a nerve be injured. Division of the nervus vagus, even as high as the larynx, has no effect on the motion of the cervical, but paralyzes that of the thoracic portion of the œsophagus. From its entrance into the thorax, to the commencement of the mesenteric sac, the œsophagus was found, in many cases, though not in all, capable of being excited to reflex action by a local irritant. Peristaltic motion could now frequently, and more easily, be produced by propagation from the pharynx. That part of the œsophagus contained within the sac of the mesentery, was found much more susceptible of reflex action; the slightest irritation of the mucous membrane produced contractions confined to the periphery of the portion irritated, and lasting somewhat longer than the irritation itself. This local contraction could be changed into a peristaltic movement only by employing a propagative irritation; as a ball, placed immediately beneath the primarily excited portion. Nevertheless, peristaltic movement may take place without any irritation of the mucous membrane, as sometimes the movement passed completely down to the stomach, even when the œsophagus contained not the smallest particle of air. The non-striated fibres of the cardiac sphincter were particularly susceptible, contracting so easily, on any irritation of the mucous membrane covering it, that it was almost troublesome to pass the fingers through it. In some dogs, it was found open, although it contracted on the slightest irritation. No constancy in action can, therefore, be ascribed to it. All attempts to discover an antiperistaltic motion in the œsophagus were vain; it seemed not to exist, and failed to be excited, even in the susceptible thoracic portion, and with the aid of a ball placed above the seat of primary irritation. The vomitory movements, when they occurred, consisted in a passive widening of the œsophagus, caused by the substances forced through it from the stomach, and an elevation of the cervical portion, caused partly by the movements of the larynx, partly by the action of the pharyngeal elevators. Whether the constrictor inferior has a share in these movements is doubtful; sometimes it seemed to act as a constrictor, the ascent of the bolus having been checked by it, and after a time returned to the stomach by a motion beginning from the pharynx. A bolus arrested till the movement of deglutition passes on, is frequently subsequently moved upwards, but only so far as the pressure occurring beneath it can effect. Neither during life, nor after death, has any irritation of the N. vagus ever been productive of peristaltic movement; frequently, however, the contractions of the œsophagus were not simultaneous at all points. This irregularity arose from the irritant—*e. g.* pressure—being insufficient to excite all the fibres of the nerve simultaneously, and disappeared, on this being remedied. The conclusions deduced are, 1st, That the peristaltic motion is brought about by the agency of the central organs of the nervous system: the first act, the contraction of the pharynx, is voluntary, or reflex. Volkmann says that with it the motions of the œsophagus are associated. This, however, appears to our author untenable; because (*a*) it is difficult, under this supposition, to comprehend why so frequently no motion, or one so different of the œsophagus, follows contraction of the pharynx; if we suppose the pharynx to be associated with other central organs in certain circumstances, then we must point out these circumstances: (*b*) were the pharynx and œsophagus, and the separate portions of the latter associated with one another, the slow and gradual propaga-

tion of the movement would be incomprehensible. The velocity with which nervous energy is conveyed, must render the movements of the peripheral parts simultaneous,—as we, indeed, find to be the case in all truly associated parts. The pauses frequently occurring between the movement of the pharynx and that of the œsophagus, gives this objection the greater weight. (*c*) If, however, a gradual propagation of the excitation in the central organs be supposed, the completion of the entire act must be independent of its accomplishment in the peripheral organs. Thus, if the motion ought to pass from *a* to *b* and *c*, *c* must be set in motion, although *b* be prevented. The central organs being uninjured; this postulate is fulfilled by all true associations. On the contrary, the movements commenced in the pharynx, and already propagated to the œsophagus, never reach its inferior extremity, should any obstruction to its contractions occur previously; and if even, contrary to analogy and to our present knowledge, an obstruction in the central organs be supposed to occur simultaneously with the one at the periphery, how the state of excitation in the central organs passed into that of equilibrium, would still remain to be explained.

II. Our author considers the œsophageal motions to be substantially reflex—*a.* the pharyngeal movements may be voluntary, reflex, or automatic, *i. e.*, produced by internal agency without apparent external cause, at a period when the influence of the will was destroyed by the deep coma of narcotism, and the irritability of the centripetal nerves was extinguished. The single acts of pharyngeal motion are most intimately associated. It mattered not to which portion the irritation was applied, provided it was strong enough, the entire act followed simultaneously on both sides; and this even although single portions of the apparatus, as the stylo-glossous and stylo-hyoideus muscles had been removed. If the irritability were weakened frequently, only local contractions of the irritated portion followed; there was consequently only a partial movement of the pharynx, which was, however, never propagated or combined with other movements. *b.* Reflex action must however be looked upon as the cause of combination of œsophageal with pharyngeal action; and also of that of the several parts of the œsophagus with one another; because, *a.*, in spite of Volkmann's doubts, reflex action may be produced in all parts of the apparatus. *β.* A ball placed in the lower portion of the pharynx facilitates the propagation of its motion into the œsophagus. *γ.* The necessity of continuous motion, itself acting as an irritant, is rendered evident by our hypothesis. *δ.* The slowness of the propagation and the duration of each single movement are thereby explained. Nay, it is important for our argument that retention of the ball, consequently prolongation of the irritation, increases the duration of the contraction. *ε.* Since these movements have the reflex character so plainly imprinted on them, facts, at first sight, appearing to oppose this, must be regarded as modifying, *e. g.* to explain why peristaltic movements of the œsophagus, unobtainable by direct irritation, so readily follow contractions of the pharynx, we must consider a particular kind of irritation as necessary for the production of œsophageal reflex action. This will be the more readily sought for in the contraction of the muscular fibres, because the movement frequently passes over the bolus, where it alone by the pressure brought about by the contraction, can be the irritant. This supposition would explain the peculiar intertwining of the fibres of the constrictor inferior with those of the œsophagus; but a doubt is thrown upon it by the fact, that even after local contraction of the œsophagus had been produced by local irritation, no peristaltic movements followed, and this doubt is increased by the fact, that irritation of the mucous membrane of the inferior portion of the pharynx, seems to facilitate the propagation of the motion into the œsophagus. Both of these facts lead to a new hypothesis, *viz.*, that the central organ of the pharynx reacts upon that of the œsophagus, thereby facilitating in it the production of reflex action, the movement having passed into the œsophagus. Its central organ must now, according to this theory, be highly susceptible of

reflex action ; pressure on any part of the tube ought, therefore, to quicken the motion, but it puts a stop to it. There is therefore yet a link wanting in the chain of connexion which, after fruitless attempts to discover it, our author leaves for the ingenuity of others. It would at first sight appear indifferent which direction, motion excited by reflex action, may take. This however is unfounded, as in the thoracic portion of the œsophagus openly and at all times capable of reflex action, our author never succeeded in producing anti-peristaltic motion ; but as no peripheral placing of the nerves can exist by which peristaltic and not anti-peristaltic motion is possible, the cause of its non-occurrence must be sought in some peculiarity of the central organs, by which power is more easily conducted in one direction than in another, as we know to be the case in the columns of the spinal cord. Experiments on rabbits generally confirm those made on dogs ; in birds, the domestic fowl and goose, results varied ; in them the œsophagus is anatomically an intestine composed of longitudinal and transverse nonstriated muscular fibres. Irritation of the muscle produced contractions corresponding in form to the irritant. If it were more extended, contraction recurring after intermediate causes ensued. Finally, contractions propagating themselves downwards, could be produced only when a large bolus had been placed within, and even then not constantly ; but occurred most constantly when proceeding from the pharynx primarily ; they resembled those in the mammalia, but scarce ever extended over the entire length of the œsophagus. When the bolus ceased to descend, the motion at times went on, at others stopped ; and the latter always when pressure was made on the œsophagus, or it was cut across, though peristaltic motion could sometimes be excited a few lines beneath this portion by laying in a bolus ; it could also be excited in the lower extremity of the œsophagus, even when completely dissected out, and left in contact only with the stomach. The bolus stuck in longitudinal incisions, but the motion passed on. Anti-peristaltic movements were never observed even in the frequent almost normal vomiting of the goose. These observations at least suffice to show that those made on one class of animals do not apply to all.

ON THE NATURE OF TETANUS CAUSED BY STRYCHNINE. By PROFESSOR HERMANN MEYER, Zurich.

THE tetanic convulsions, which follow the exhibition of Strychnine, are well known ; as also the fact that these convulsions can be readily excited as reflex movements. The general use of strychnine in experiments on the nervous system is very much based on the knowledge of these facts, and invites to a discussion of the question as to the mode in which the tetanus originates. The following experiments were instituted for this purpose.

Exp. I. The crural nerve of a frog, suffering from convulsions induced by strychnine, was divided ; these immediately ceased in the limb.

Exp. II. No convulsions took place in a limb, in which the nerve had been divided previous to the exhibition of the poison.

From these well known experiments, it follows that the originating cause of the convulsions must be sought for in the central parts of the nervous system. But the question ensues, does the tetanus originate from an affection of an individual portion of the central organ, or in an affection of every individual portion ? The following experiments, also somewhat well known, tend to throw some light on the subject.

Exp. III. The brain was removed from a frog ; tetanus ensued in the trunk.

Exp. IV. Brain and cerebellum removed, result as in the preceding.

Exp. V. Medulla oblongata removed ; same result.

Exp. VI. Medulla oblongata divided ; general tetanus ensued.

Exp. VII. The spinal marrow was removed posterior to the giving off of the nerves to the anterior extremities ; result as in the preceding case.

From these experiments, it follows that the origin of the tetanus is to be

sought for in some cause common to all the central organs of the nervous system. The following experiments show what this cause is.

Exp. VIII. The posterior column of the entire spinal marrow was removed from a frog by means of a fine pair of scissors. A solution of strychnine was then either exhibited by the mouth, or applied directly to the spinal cord. Convulsions occurred only in the jaws and eyes.

Exp. IX. The posterior column of that part of the cord giving off nerves to the posterior extremities was removed. Tetanus occurred only in the head and anterior extremities.

Exp. X. The same portion of cord giving off nerves to the anterior extremities was removed. Tetanus occurred only in the head and posterior extremities.

From these experiments, the author thinks himself entitled to conclude, that the presence of the motor nerves, even though uninjured, does not suffice to produce tetanus; but that there must also be an uninterrupted connexion of the sensory and motor fibres within the central part of the nervous system. The only motions, however, which we recognize as dependant on the sensory fibres, are those which we know to be the cause of reflex movements. The above experiments, therefore, would lead to the conclusion, that tetanus caused by strychnine arises entirely from general reflex movements, and that the action of the strychnine not only excites the motor fibres, but goes to increase the necessary cause from which reflex movements arise. If this be true, tetanus must cease, even when the spinal marrow is uninjured, provided the conditions under which reflex movements arise are removed. In order to test this, the following experiments were instituted:—

Exp. XI. The skin of a frog was rubbed over with strong prussic acid (30 per cent.), so that the superficial peripheral extremities of the nerves were paralyzed. Strychnine was exhibited; no tetanus ensued, except when the animal was strongly shaken (thrown about, for example).

Exp. XII. After laying open the spine of a frog, the posterior nervous roots were divided. Same result as above, or as when the posterior column of the cord was irritated with a needle.

Exp. XIII. The same experiment was repeated in a frog in which tetanus was present; it ceased immediately on division of the posterior roots, and only recurred under the conditions first stated.

From these experiments, then, it may be concluded, that tetanus arising from the action of strychnine originates entirely and alone from reflex movements produced by the strychnine exciting to increased action the primary cause of the reflex motions themselves.

The next question that occurs is, what is the primary cause of these reflex movements? Here, however, we find ourselves in the field of physiological controversy; as far as regards the question first considered, it is a matter of little moment how the controversy may be decided. It may be remarked, however, that tetanus induced by strychnine cannot be attributed to a general increased excitement of the nervous system. Were this the case, stronger contractions of the muscles dependent on the anterior cord laid bare in Exp. VIII. IX. and X. should have occurred rather before the administration of the poison than after it; but this was not the case. Other experiments, which require to be repeated, however, seem to show that the cause of reflex motion is to be sought for in the grey matter of the spinal cord, that the strychnine acts, therefore, by exciting to activity this grey matter, or rather the ganglionic in masses composed of it. These experiments are the following:—

Exp. XIV. A needle was carefully passed down through the centre of the spinal marrow, of a beheaded frog, as far as the point where the nerves to the anterior extremities are given off, and then withdrawn. Strychnine was then administered. Tetanus occurred in the posterior extremities only.

Exp. XV. After laying open the spinal canal, the grey matter of the posterior half of the cord was destroyed as far as it gave off nerves to the posterior

extremities. The movements of the latter were little affected by the operation ; but tetanus did not ensue after the exhibition of strychnine.—*Zeitschrift für Rationelle Medizin*. V. Band. II. Heft.

SURGERY.

ON THE ORIGIN OF SOLID BODIES IN SYNOVIAL CAVITIES. Dr BIDDER.

THE author had charge of a case of long standing swelling of the knee joint, which finally opened, allowing a quantity of granular matter to escape. The grains composing this matter were of a uniform size, — $1\frac{1}{2}$ ''' long, $\frac{3}{4}$ ''' broad, $8\frac{1}{2}$ ''' thick,—regular, flat-oval, clumped together in masses of variable size by a glutinous transparent fluid, present in but a very small quantity ; they were highly elastic, presented no trace of a pedicle ; their cut surface seemed homogeneous to the naked eye, and presented no trace of organization under the microscope ; a chemical examination showed them to be composed of albumen. Mickel's opinion, so recently substantiated by Hyrtl, does not, therefore, hold good in every case ; our author acknowledging its correctness in many cases, as well as the possibility of some cases arising from hydatids (Dupuytren), viz. those in which the bodies are possessed of a laminated structure, and have an internal cavity, notwithstanding that other distinctive marks may be lost (vide Gluge. Anat. Mic.), throws out another hypothesis as an explanation of their mode of origin in cases like the present, viz., that in certain cases an increased flow of blood, and consequent secretion of synovia, may force off the epithelium cells ; that these subsequently increase, partly by endosmosis, partly by precipitation on their external walls ; the peculiar life of the cell wall in certain cases altering the contents both with respect to colour and consistency, no membrane being perceptible under the microscope, may proceed from its stretched and thinned condition, from its being originally structureless, or from its homologation with its contents. The bodies examined by him consisted almost entirely of albumen, easily obtained from the synovia : their uniform size likewise presupposed their origin to have been from similar forms, endowed with similar capacities for life, conditions fulfilled by the epithelium cells. This theory can only hold good where the synovial cavities have an epithelial covering, which is wanting in bursæ mucosæ, and mucous sheaths of tendons. —*Zeitschrift für Rationelle Medizin*, III. Bd. 1 ht.

EXCISION OF A PORTION OF THE TIBIALIS ANTICUS AND DIVISION OF THE PERONEUS LONGUS. By Dr BLACKMAN.

“ THE patient was a lad thirteen years of age, afflicted with a paralytic affection of the right inferior extremity, commonly called ‘ weak ankle.’ His father, who lives in New Jersey, called on me in October, 1841 (at which time I resided in New York), for the purpose of placing him under my care. He gave me the following particulars of the case :—The child began to walk at a much later period than usual ; he could never raise the right foot from the ground as readily as the left, and when raised it pointed downwards and outwards, assuming the form of talipes valgus ; the temperature of the limb was greatly diminished, and its sensibility considerably impaired. Owing to the awkward swinging gait so peculiar to this affection, and the general diminution in the size and length of the right leg, the spine had become considerably distorted in a lateral direction. His general health was good. I put the patient on the use of strychnine, both externally and internally, frictions, &c., and applied a shoe, to which was attached a spring calculated to assist the muscles on the front of the leg. This course was faithfully pursued for six months, without any appreciable benefit. Being satisfied that it would be useless to persevere further in this manner, I resolved to resort to a method different from

that usually adopted in such cases. On the 6th of April, 1842, I divided the peronæus longus, which seemed to play an important part in the eversion of the foot. In the next place, I commenced an incision just above the annular ligament, which was carried about an inch and a half above its superior margin. The tibialis anticus was laid bare by a careful dissection, and about three quarters of an inch of its tendon removed. The wound was immediately closed, and a roller applied with Stromeyer's board, for the purpose of maintaining the divided extremities of the tendon in apposition. The patient was directed to keep quiet, and to take a gentle cathartic. Little or no pain followed; the wound healed by the first intention, and the patient assured me that there was no perceptible difference in his sufferings subsequent to the operation, in the two wounds. At the end of the seventh day, I found, on removing the dressings, that the ends of the tendon had united sufficiently strongly to maintain the foot in its natural position. He was directed, however, to continue the use of the footboard, having it removed at times, only for the purpose of making some efforts to flex and extend the foot. In two weeks, the dressings were all removed, when the patient could, for the first time in his life, place the foot on the floor, and raise it again without any of that dropping downwards and pointing outwards, to which it had been so long accustomed. The patient informs me that his limb has continued to improve in growth and strength, and he is delighted with the result of the operation."

Dr B. remarks that he does not believe that tenotomy proves innocuous only because it is performed on the subcutaneous principle. In the Medical and Surgical Journal for October, 1841, Mr Braid states that he has treated a number of paralytic affections both of the upper and lower extremities, by similar operations to that above related, and with considerable success. And Dr Blackman states, that in two similar operations which he has performed (the one, in which he removed a portion of the semi-tendinosus for a paralytic affection of the inferior extremity, in which the knee projected backwards; and the other, where he excised a portion of the extensor communis digitorum for a "dropping of the hand;") he found no peculiarity in the healing of the wound. The following judicious remarks with regard to the benefits of tenotomy are added:—

"We are by no means disposed to underrate the importance of this branch of surgery, but we do believe that its advocates have exaggerated the benefits which it is capable of conferring. They have too often led the afflicted to believe, that in tenotomy and myotomy was to be found the great panacea for every form of distortion, and that with the shape they might expect the perfect restoration of all the functions of the affected part. This we conceive, in the majority of cases, to be impossible. When any part of the body has been for years retained in an unnatural position, the structures of that part become so greatly modified as to render it impracticable to restore them to their normal state. Patients would derive much greater benefits from orthopedic surgery, were operative proceedings not so much regarded as the *sine qua non*, but only as an important adjunct to other powerful means. Let the division of the muscles and tendons in cases of contraction, and the excision of a portion of the tendon where paralysis exists, be faithfully followed by bathing, friction, manipulations, and proper apparatus, and this department of surgery cannot be too highly appreciated."—*Amer. Journal of Med. Sciences, April, 1847.*

ON WOUNDS FROM FIRE-ARMS WITHOUT BALL. By Dr SWIFT of Philadelphia.

THE danger of a gun or pistol shot wound, even when the piece has not been loaded with ball, slugs, or shot, as well as powder, is well pointed out by the following case, reported by Dr Swift of Philadelphia.

"On last New Year's eve, at the Good Will Engine house, Wm. Simler, a minor, playfully, but heedlessly, fired a pistol charged with powder only, at his friend and companion Robert W. Pitt, inflicting a serious wound. Pitt, staggering into the arms of his friends, cried out, "I am shot." Simler,

thinking him frightened, but not harmed, said, laughing, "It was not loaded," or, as another witness testified, "It had no ball in it." The wound was on the fleshy part of the left hip, above and behind the trochanter major, about one inch in diameter, and four inches in depth; the integuments were destroyed, and the muscles presented a blackened, mangled mass; it bled but little, and was carefully probed with the finger, which readily passed to the bottom of the wound. No untoward symptom arose till the sixth day, when tetanus, in its most distressing form, opisthotonos, supervened, and he died on the morning of the seventh.

On a careful post mortem examination, no foreign substance was found, but a minute fragment of woollen cloth, about two inches from the surface, and *grains of gunpowder, with which the wound, through its whole extent, was blackened.*

Thus it was evident that this fatal wound was caused by the explosion of gunpowder in a pistol of ordinary size; a wound at least four times as large as a ball from the same instrument would have caused; and so mangled were the tissues, through this great extent, that vitality was utterly destroyed. Had the unfortunate young man lived, it must have been through great suffering, necessarily attending the tedious and exhausting sloughing of the dead mass.

At the legal investigation which followed, there was some discrepancy in the testimony in regard to the distance at which the pistol was held when the wound was inflicted, the witnesses differing from one foot to two or three yards; nor is this very strange, as it occurred in the night, in a place not well lighted, and in the midst of a moving throng of some twenty individuals. The patient himself, however, declared his belief that the weapon "almost touched him." The pistol was said, by Simler, to have had a paper wad; but no wad was at any time found, and the evidence given at the time rendered it quite probable none was used.

Being one of the professional attendants in this case, I was somewhat surprised at the character and extent of the wound; it was obvious no ball could have produced it; nor was it conceivable that a paper wad could have caused such extensive lesions. A considerable research in works on medical jurisprudence failed to give satisfaction, and though the circumstances forced upon my mind the inference that a heavy charge of powder, exploded near the part, had alone caused the mischief, yet, as young Simler had been bound over for trial before a criminal court, it seemed very desirable to possess farther data in the premises. With this view I sought, and, through the courtesy of the officers of Jefferson Medical College, obtained the facilities for making the following experiments, which were used at the subsequent trial, and are now offered to the public with a hope that they may be useful in future medico-legal investigations of this nature. The pistol—the same used by Simler—was wadded with paper, and has a bore about four inches in depth, and six lines calibre.

Experiment 1st.—Pistol with an ordinary charge was held 12 inches from fleshy part of hip, the part being covered with one thickness of broadcloth and a twilled cotton cloth under it—Clothes lacerated, and skin abraded; wad on the floor, on fire.

Experiment 2d.—Distance 6 inches; parts covered as before, clothes lacerated; wad lodged one inch and a half below the surface.

Experiment 3d.—Distance 2 inches—wound ragged, blackened with powder, and penetrating to the bone—one and a half to two inches—wad was found immediately beneath the integuments, and somewhat *on one side of the principal wound*—parts covered as before.

Experiment 4th.—Distance one and a half inch from the *ribs* of the right side—no covering of cloth—penetrated the cavity of the chest, the wad passing through the intercostals between the ribs.

Experiment 5th.—Distance the same—no covering of cloth, the integuments removed—wad penetrated the thorax, *carrying away a transverse portion of the rib.*

The subject, about 35 years of age, a male, not recent, had undergone a

preserving process with chloride of mercury, considerably hardening the muscles ; it was also much emaciated.—*Medical Examiner*, March 1847.

NEW MODE OF BANDAGING WOUNDS AND STUMPS. By M. BAUDENS.

At the meeting of the French Academy of 6th June, M. Baudens, chief surgeon of the Hospital Val-de-Grace, made known, in a short note, a method, which he had lately devised, of bringing together the edges of wounds in order to unite, and which he is now daily employing with success at the hospital. For instance, in order to unite the two flaps of integument after the operation for removing the foot at the ankle, as practised by him, a bandage is fixed circularly above the stump, and in it are inserted two strong pins, one in front and the other behind, leaving their heads and points free. Around the two ends of the pins thus left exposed a long and thick cotton thread is looped ; the threads from both sides are next brought down to the edge of the stump, and crossed over the lips of the wound, which are held together by the fingers of an assistant. The thread, being crossed over to the opposite side, is now passed under the ends of the pin of that side, from which again it may be made to recross : by this crossing of the threads of both sides over the wound, a support is given to it similar to that afforded by a bandage. The crossing of the threads may be repeated as often as it is deemed necessary ; and the course of the threads of opposite sides may be parallel or across, so as to make a figure of eight. The ends of the ligatures applied to the arteries, being also made fast to the pins, are in no danger of being torn away in removing any applications from the stump, as will sometimes happen with the old plan.

The advantages this plan offers, are,—the gentle pressure exercised by the thread ; the avoidance of impregnation by the discharged matter, which cannot long remain in contact with the end of the stump ; the open spaces left between the threads allow of a ready discharge of fluid matters from the wound ; and the constant pull upon the circular bandage above the stump tends to draw the flesh towards its extremity, so as to render it conical.

This mode of bringing about the union of parts is, according to M. Baudens, applicable to all kinds of wounds, a suitable support being first contrived for the pins, on which the traction is excited.

It may be remarked, however, that many surgeons object to a circular bandage being applied just above the stump, at least with any tightness, such as M. Baudens' method would require, on account of the impediment it offers to the circulation in the part, and the consequent tendency it has to produce an œdematous state, and even worse results. Nevertheless, even if the evils of constriction above a stump have not been magnified, the device of M. Baudens is very ingenious, and no doubt will prove very convenient and useful in procuring the adhesion of the opposite lips of wounds in many cases.—*Dublin Medical Press—Lancet*.

HEMERALOPIA IN WARM CLIMATES. By DR A. GUEPRATTE.

DR GUEPRATTE states, that in the mission to Madagascar in 1846, and also in all the voyages he has made during the last twenty years, he has had many opportunities of making observations on hemeralopia. Dr G. is inclined to consider the affection as a variety of amaurosis, consisting in a loss, or at least in an alteration of the power of the retina—that there is an increased sensibility of the component and accessory parts of the organ of vision—and at the same time that the sensorial nerve (retina) is surcharged with vitality, the power of the circulatory apparatus is more active in the nervous textures (*appareil nerveux tactile*) ; hence, the hemicrania, circumorbital pains, &c., which are seen as complications. Between the tropics or their neighbourhood, the causes of these affections seem to him to be the power of the sun, working upon deck exposed to a direct bright and shining light, which is not moderated either by an awning or a shade ; the prolonged action of moonlight after

days passed under the bright solar rays, rather than moisture and the use of salt provisions containing little nourishment, but which in his opinion are capable of acting as predisposing causes. Gastro-intestinal disturbance, when it does exist, he considers as only a complication, and it is the same with the cephalalgia. In a word, the retina fatigued, worn out by a too great excitement, continued for too great a length of time, becomes unable to perform its functions, or does so in an improper manner. Hemeralopia does not in general come on suddenly, but by degrees; the patient complains of his eyes being watery; he sees a little less distinctly; he is easily fatigued when reading or writing; his head feels heavy and dull without any real pain. Very soon these different symptoms become worse, and the blindness is almost complete from sunset to sunrise; the vision is only good during the day. As to treatment, the first thing to be done, according to Dr G., is to withdraw the affected organs from the influence of the light, and for this purpose he prefers to all others the bandage which is used after the operation for cataract. At the commencement it alone may suffice, and in a few days produce a cure. In more serious cases, whether or not there is derangement of the *primæ viæ*, he prescribes slight purgatives, as marsh mallows, sulphate of soda or magnesia, castor oil, and emetised whey. In strong plethoric subjects, of a high complexion, and with heat of the head, he precedes these means by a blood-letting from the arm or from the foot. He has rarely had occasion to have recourse to these energetic means when he had to deal with patients who were otherwise healthy. In from five to twelve days the majority were cured; it was only after this latter period that he considered it necessary to apply a counter-irritant as a blister to the nape of the neck.—*Annales de Thérapeutique*, Mai 1847. (From *Gaz. Med. de Montpellier*.)

ON CAUTERIZATION OF VESICO-VAGINAL FISTULA WITH CREOSOTE. By Prof. EMMERT of Bern.

A VESICO-VAGINAL fistula, subsequent to artificial labour, having been treated with lapis infernalis 17 times without any success, Emmert proceeded to remove the gangrenous scab produced by the last cauterization, after which he touched the fistular opening to a great extent by means of a pencil-brush immersed in creosote. The success was most surprising. The new scab was so firm, as to enable the patient to retain her urine the whole day, and to micturate without becoming moist. The detachment also occurred much later than after the application of other caustics, and when effected, the urine flowed in far less quantity from the somewhat diminished fistula. A second application of creosote, six days afterwards, induced a perfect cure of the fistula, and no urine has passed from it, during two months which has since elapsed. A fortnight after the last cauterization, Emmert found the fistula perfectly cicatrized, its previous existence being marked only by a bluish-red streak, two lines in length. He therefore declares creosote to be the best application in such cases, the nitrate of silver being too feeble, and the concentrated acids too energetic in their action.—*Journ. für Chir. und Augenheilk*, 1847. Heft. 4.

CASE OF NÆVUS MATERNUS LIPOMATODES. By Dr HOLLSTEIN of Fürth.

THE whole back of the boy, aged two and a half years, was covered with brown hairs an inch long, which, particularly on the left side, reached about the middle of the abdomen. To a greater or less extent there was the same anormal development of hairs on the upper part of the left thigh, the lower and anterior surface of the right thigh, and the right acromion. The nates were covered with two darkish brown sacs, which hung down over the femoral fold, and in the left inguinal region was an isolated fatty prominence five inches long. The skin covering the swellings was dense to the feel, smooth, with fissures here and there, and having on its surface several darkish blue or brown nævi, some with and others without hair. From the growth on the nates,

there ran a tuft of close hairs over the spinous processes of the vertebræ up to the occiput.

The child, which thus resembled an ape posteriorly, enjoyed a good appetite and slept well. He laboured from time to time under scrofulous œdema of the eyelids, and subsequently under scrofulous caries of the wrist, which continued for six months. Three months previous to his death the caries healed, and a fluctuating blue-coloured growth appeared below the left shoulder, which, after a time, increased to the size of a child's head, and furnished, on incision, a bloody glutinous fluid. On retracting the edges of the incision an encephaloid-like mass, covered with coagula of blood, was observed to occupy the base of the swelling. Ulceration now rapidly proceeded, diarrhœa supervened, and convulsions ushered in death.

Post Mortem Examination.—The lipomatous growth on the nates, although diminished in size during the last few months of the patient's life, still weighed five pounds. It consisted of a fibro-serous sac, partially filled with isolated fat masses. A microscopic examination discovered everywhere cellular filaments, and free non-crystallized fat. The coloured spots contained a quantity of granular pigment, both free and enclosed within cells, conjoined with numerous epithelial cells. The axillary growth exhibited in addition oblong and caudate cells, so as to justify the diagnosis of encephaloid.—*Ibid.*

PATHOLOGY AND PRACTICE OF PHYSIC.

ON THE NOSOLOGY OF APHTHÆ. By Dr EISENMANN.

THE author had been consulted in the case of a child four months old, labouring under the usual signs of acidity of the primæ viæ, with a general aphthous state of the mucous membranes, and twitching of the facial muscles. The symptoms were temporarily relieved by the use of remedies, but yielded completely only to a change of diet; milk paps being interdicted (the child was brought up by hand), and replaced by acorn coffee with a little milk twice a-day, thin soup and bread, and sugar and water as drink; as a remedy, the tinctura theæ aquosa with carbonate of potash. Under this regimen, and with two hours' exercise in the open air daily, weather permitting, the child soon became healthy and blooming; it belonged to the higher ranks, lived in a loftily situated, perfectly dry house, and lay under no suspicion of scrofula; yet it had a small scrofulous-like swelling on one of its fingers, which disappeared along with the apthæ. In considering the relation between the acidity of the primæ viæ and the apthæ, the author quotes the result of the enquiries of Liebig, Andral, and Gavarret, viz., that microscopic plants of the lowest grade arise in albumen sufficiently diluted, under the influence of an acid, and exposed to the atmospheric air. Now the apthæ (soor) consist of such plants, arising under similar conditions? The blood contains too much water as a consequence of the bad digestion and defective supply of other materials; a portion of the acid of the chyme passes doubtless with the chyle into the blood; the necessary atmospheric air of oxygen is supplied by the respiration; the fungi may then arise in two methods,—1st, The anormal blood may so work upon the centrifugal or motor nerves of the predisposed mucous membrane of the alimentary canal as to produce congestion (stasis) and exudation, in which, under the influence of the atmospheric air, the germs of the fungi may be formed and developed: or, 2d, The germs may be formed in the blood, and only developed after exudation. With respect to the scrofulous-looking swelling on the finger, Fuchs has shown that in scrofula a copious production of fungi takes place; apthæ (soor) and scrofula may then be allied affections, or the first may be a species of the latter; similar etiologic circumstances are found in both. Whether the damp air and damp dwellings in which scrofula

delights react upon the digestive organs, or cause the production of morbid fungi in some other manner, cannot now be decided. — *Zeitschrift für Rationelle Medizin*, III. Band. 1 heft.

ON PNEUMONIA IN CHILDREN, AND ITS TREATMENT. BY M. GUERSENT.

THERE are two forms of pneumonia in children, distinguished alike by their anatomical lesions, the conditions under which they are developed, the symptoms which characterise them, and the results of treatment. Lobular pneumonia, and lobar pneumonia, do not occur at the same epoch of life among children. From the age of eighteen months, to that of five or six years, inflammation only occupies a certain number of lobules. Beyond this age, however, it invades large portions of the pulmonary tissue, and resembles, consequently, pneumonia of the adult.

Lobular pneumonia presents two varieties; one lobular, properly so called; the other mammalated—that is, one where the inflammation is disseminated, and only occupies separate lobules; the other, where the inflammation, after having existed in this state, generalises itself, and extends over a greater or less part of the pulmonary tissue. In the first variety, the diagnosis is very difficult, if not impossible. The symptoms are those of bronchitis, with some rales, more or less coarse,—no dullness, no bronchial sound; and it is only the intensity of the dyspnoea and of the fever, which prevents our confounding this affection with simple bronchitis.

In the second variety, the catarrhal symptoms also predominate. The physical signs are still those of bronchitis; but in many parts of the chest, the dullness, bronchial sound, and bronchophony, indicate the existence of an inflammation, extended more or less over the lung. In some cases, the rales are so numerous and extended, that they mask, to a great extent, the phenomena of pneumonia. It is rare, however, that on auscultating with care the whole chest, there is not found, independent of the signs of bronchitis, a little rough respiration, or tubular sound; and in this case the diagnosis is no longer uncertain.

Lobular pneumonia is a very fatal disease. Between the ages of eighteen months and of three years, it is rare that children, entering the hospital with this disease, are cured. In private practice, however, amidst better hygienic conditions, and more careful and intelligent attendance, it has more frequently a favourable termination.

As regards treatment, the employment of emetics is very useful. Ipecacuanha, given alone, or with syrup, is of great service. As the catarrhal symptoms predominate in lobular pneumonia, it operates by unloading the bronchi, and relieving the disease of its most distressing complication. Bleedings are seldom followed by any perceptible benefit; nor does tartrate of antimony exert much influence, although its tolerance, in large doses, is rapidly established. Of all the remedies employed to combat the pneumonia itself, blisters are productive of the most marked and unequivocal good results.—*L'Union Médicale*, Juin 29, 1847.

CASE OF APOPLEXY OF THE LARYNX.

F., aged 43, admitted into the Zurich Hospital 17th January 1844, labouring under general symptoms of the action of mercury, salivation, &c., produced by friction with mercurial ointment with the intention of ridding himself of lice. His symptoms were partially relieved by gargles and the use of potass. hydriod. On the 18th he had been observed to cough occasionally; on the 19th, 6 P.M., shortly after having left the room for a short time lightly clothed, he was seized with a violent dry cough; the assistant found him at 7 P.M. coughing violently, with a croupy sound; voice hoarse, almost gone; breathing whistly, short, and anxious; eyes protruded; pulse frequent, oppressed; face livid, and covered with a cold sweat; auscultation showed nothing anormal; water

was swallowed easily. Venesection, tartar emetic and other remedies proved fruitless. He died at 11 P.M.

Section 48 hours after death. On the internal surface of the larynx, beneath the right ventricle, a darkish red blood coagulum extending downwards a quarter of an inch, and 1-3'' deep lay directly under the mucous membrane which was somewhat thickened; immediately beneath the left ventricle existed a less extensive sugillation. The mucous membrane also softened and thickened; the mucous membrane of the trachea and alimentary canal studded with ecchymotic spots. Here the proximate cause of the fatal effusion was undoubtedly the fluidity of the blood, caused by the absorption of an unknown quantity of mercury. The cough with which the patient was affected undoubtedly determined the seat of the effusion. Many cases of sudden death during mercurial inunction (*die grone Schmierkur*) are published by sundry authors, and ascribed to a *carditis*. The author is not aware whether the larynx have been in all cases examined. This is the first case of apoplexy of that organ that came under his observation.—*Zeitschrift für Rationelle Medizin*, III. Bd. 1 heft.

CONTRIBUTION TO THE KNOWLEDGE OF CONTAGION. By DR MÜHLHÄUSER.

THE diarrhoea of a man recovering from pneumonia was found to be chiefly composed of a species of fungus. The examination was always made immediately after excretion, the fungus remained present till death; there was no reaction. In regular typhus, the same forms constantly occur in similar quantity. Similar forms were also present in an idiopathic diarrhoea, unaccompanied by typhoid symptoms, but were absent in healthy and calomel stools, and in the tubercular and rheumatismal diarrhoeas. The fungus is not above 0.008'' in size, two to twelve globules are generally strung together in a winding form, rarely straight. Sometimes there is a small space between each two globules—they are more or less developed, circular or oval—the granular masses also present, vary in size from 0.03''' to 0.08''', are round or long, sometimes of an irregular form, surrounded with a cell wall. They are visible to the naked eye, forming the lowest stratum of the two into which diarrhoeal matter divides itself: the fungi are found in equal quantities in both, the masses are analogous to the fungi found in the aphthæ of children. Both forms pass into other vegetable formations, and finally into infusoria; they are, consequently, only found in recent excretions. The fungus is apparently a kind of mycoderma. Sometimes in recent typhous diarrhoeas, many long oval vesicles with a marked border 0.006'' long, somewhat bent, and containing two clear specks, and small granules are found, they are a species of frustularia, and related to or identical with that found by Andral and Gavarret in albuminous fluids—they speedily pass into long convoluted or branched bodies—mould is probably identical—the injection of five grammes from the upper stratum of a diarrhoea, containing plenty of fungi but no larger bodies into the jugular vein of a rabbit, had no apparent effect on the health of the animal.—*Zeitschrift für Rationelle Medizin*, III. Bd. 1 heft.

APHONIA CURED ON THE OCCURRENCE OF ŒMOPTYSIS. By DR BRACH of Bonn.

A PLETHORIC female 19 years of age was frightened, while menstruating, by a horse running away, and at the same time caught cold, in consequence of which she became affected with complete aphonia. No hoarseness, pain, or cough was present, the patient complaining of nothing but of a somewhat putrid taste in the mouth though the tongue was quite clean. This state continued for seven days, and in the mean time sal ammoniac and tartrate of antimony had been prescribed. On the fourth day the taste of the mouth became still more unpleasant, along with nausea and putrescent eructations. At last two masses of coagulated blood of considerable size were expectorated, after which the voice

was restored and all the morbid symptoms disappeared.—*Preuss. Vereinszeit*, 10 March 1847.

We think it very doubtful if the blood in this case came from the respiratory passages, it seems much more probably, at least for anything stated in the report, to have come from the stomach. We look upon the aphonia here to have had a hysteric character, under which view the mere effort by which the coagulated blood was discharged, whether one of expectoration or of vomiting, might remove it. We think the case mentioned in illustration of our author rather supports our view than his own. It is that of a young man who became blind after swallowing a quantity of fat, and recovered his sight on the operation of an emetic.

MIDWIFERY, AND DISEASES PECULIAR TO WOMEN.

CASE OF PUERPERAL MANIA RELIEVED ONCE BY THE SPONTANEOUS OCCURRENCE OF ABORTION AND TWICE BY THE INDUCTION OF PREMATURE LABOUR. By THOMAS SALTER, Esq.

THE subject of the following case is a lady of above thirty-one years of age, of delicate organization, fair and ruddy complexion, with dark hair and eyes, usually enjoying good health. She has been married three years and has been three times pregnant. In her first pregnancy a disordered state of the intellect commenced about the seventh month, and after continuing a fortnight, labour came on, and in a moderate time she was safely delivered of a female child, who is now living and in good health. She again proved pregnant, and this time the mental alienation showed itself about the termination of the sixth month, manifesting the same symptoms as in the preceding attack—constant restlessness, almost total absence of sleep, and such a degree of violence of manner, as often to require four or five persons in constant attendance upon her night and day. After continuing in this state for two or three weeks, it appeared to the medical gentleman, under whose care she was, necessary to bring on labour, by rupturing the membranes and evacuating the liquor amnii. This was accordingly done, and in five days she gave birth to a still-born child. The violent paroxysms of insanity which before threatened her life, two days after this event ceased, and she gradually recovered her former state of mind. Soon after her removal to this neighbourhood, from the north of England, her former place of residence, she became pregnant a third time, and on this occasion the unsoundness of mind showed itself shortly after she had passed the fourth month. From this period my attendance commenced. The preceding account I received from the lady's husband, whose intelligence and constant presence about her person, guarantee its correctness.

I was hastily requested to visit this lady at two o'clock, A.M., March 5, 1844. I was informed that previously to going to bed she had manifested considerable irritability of mind; at the time of my visit she laboured under total mental alienation, and was constantly talking in an incoherent manner. I prescribed a sedative mixture, with ether and camphor, which appeared to have some quieting effect, but scarcely any sleep was obtained through the night. After this time she became more violent and unmanageable, requiring sometimes four or five persons to keep her in bed, incessantly talking, either religiously or quite the reverse; she also had occasional fits of screaming, so loud as to be heard by the neighbours living at a considerable distance from her own residence. She continued in this state, and without any sleep, day after day.

Saturday evening, five days from my first visit (since which she has had no sleep), there being no improvement, and no signs of abortion occurring, in order

to ensure the safety of the patient, it appeared necessary to adopt the practice which had been so judiciously carried out in the previous pregnancy by her then medical attendant; but from the length of the cervix uteri, and its undeveloped condition at the period at which she had now arrived, I felt that greater difficulties would present themselves in rupturing the membranes than on the occasion referred to. On making an examination for this purpose, the os uteri was found high up, and situated backwards towards the sacrum, and was therefore with difficulty reached; it was, moreover, so closed, as not to admit the point of the finger. The cervix was, as far as could be ascertained, of the usual length of that of the unimpregnated uterus. By passing the stilet of a catheter on the front part of the fore-finger of my left hand, whilst that member was in the vagina, and pressing against the os uteri, I made an attempt to puncture the membranes. Much difficulty was experienced in passing the stilet through the cervix, which, however, I once affected, but no fluid followed its withdrawal. As I therefore did not succeed in my object, and fearing the cervix uteri might be injured by further trials, I desisted for the present from the purpose I had in view, and determined to make trial of the ergot of rye, in the hope by the influence of that drug to produce abortion. It was exhibited in the form of infusion, in proportion of three drachms of the powder to eight ounces of boiling water, giving one ounce of the infusion, with a drachm of the tincture, every two hours. From the violent and unmanageable condition of the patient not allowing it, the medicine was not given either as often or as regularly as could have been wished, and no sensible effect being produced by it, on Monday, the 11th, a scruple of the powdered ergot was added to each dose of the infusion.

Tuesday evening, March 12th. Another attempt was made to puncture the membranes, and which happily proved successful. On this occasion the os uteri was found to be slightly opened, and the cervix seemed also somewhat shortened,—changes favouring the operation, and evidently produced by the use of ergot. From the state of the patient's mind it was difficult to ascertain whether or not there were any uterine pains. Remembering the difficulty experienced in my first essay, and the danger there appeared of injuring the cervix uteri, I, on this occasion, employed an instrument (contrived by my son, Mr S. J. A. Salter, now a student in the Medical School of King's College), expressly with the view of avoiding the possibility of this accident. It consisted of a flexible gum male catheter, but of sufficient firmness to preserve its straight form, even when opposed to a moderately resisting body, having a perforation at its extreme point, just large enough to allow a common wire stilet to pass through.

The patient, lying on her left side, and near the edge of the bed, the catheter was introduced into the vagina with the right hand, and pushed forwards upon the anterior surface of the fore-finger of the left, and in this manner it readily entered the os uteri, and passed through the cervix; withdrawing the finger from the vagina and os uteri, the instrument was pressed forward until it met with steady resistance. The stilet was then introduced and passed on, making its exit at the opening prepared for it, when it was immediately found to have perforated the membranes. Renewed pressure being then made upon the catheter itself, that also entered the cavity of the amnion, and on withdrawing the stilet the liquor amnii flowed in a full and continuous stream, as urine is observed to do from a catheter introduced into a distended urinary bladder. The fluid collected amounted to about a pint and a half. Nothing could have been more successful than the use of this simple instrument, and I would venture to recommend its employment to my professional brethren, as equally adapted for the purpose for which it was had recourse to, as the ingenious cutting instrument contrived and recommended by Dr Ramsbotham, which I did not happen to possess, and the use of which, in unskilful hands, might not altogether be unattended with danger, and be especially hazardous in cases where the cervix and os uteri are undeveloped, as in the present instance.

After the evacuation of the liquor amnii, the patient took one or two more doses of the ergot of rye, but as its previous exhibition had not occasioned any very decided uterine action, as evidenced by the absence of the usual pains, and the liquor amnii being so entirely evacuated, the further use of the medicine was not urged, in the confident belief that the ovum would be soon expelled by the natural efforts.

In the middle of the night of Thursday, the 14th (nine days after the commencement of the mental derangement, and fifty hours subsequent to the puncturing of the membranes), expulsive pains came on, and the delivery was accomplished at two o'clock on the morning of the 15th, being fifty-two hours after the rupture of the membranes and the discharge of the liquor amnii. In a very short time subsequent to this there was a manifest improvement in the state of the patient, and though still incoherent, and having occasional paroxysms of violence, she became more manageable, and slept occasionally for a few hours. On the second day there were intervals of comparative soundness of mind. Since this period her improvement became gradual, and though slow, regularly progressive, and she is now (April 5th), in her usual health in every respect. She had, however, two or three violent paroxysms after her delivery, and frequently awoke from her sleep in a fright, screaming violently, and she, moreover, passed many nights without sleep; at such times, as well as previous to delivery, to procure rest, opiates were occasionally given, but they only appeared to answer the purpose one or twice, and from their heating and other effects, seemed to be rather injurious than otherwise.

During the whole progress of this case, the pulse was seldom more than 100 in a minute, and often not more than 80; the skin was cool, and there was upon the whole less disturbance of the functions than might *a priori* have been expected from the violence of the symptoms, and the long absence of sleep.

May 20th, 1847. This lady is now living, and in good health, but has aborted once without any unusual circumstance happening, the only time of her being pregnant since the above case occurred—*Provincial Med. and Surg. Journal*, June 30, 1847.

CASE OF CÆSAREAN SECTION.—By JOHN BALFOUR, Esq., SURGEON, H.E.I.C.S.

(Extract from a letter to Dr Balfour, of Corstorphine.)

ON the morning of the 21st March, 1847, I was asked to see a case of difficult labour in a native. I found a dwarf under four feet high, with strangely distorted limbs, and considerable curvature of the spine. She stated that she had been in labour ten days, her countenance was anxious, tongue loaded with a dark fur, and dry, pulse small and quick, retaining some strength; the bladder was much distended, and rising above the umbilicus. The uterus inclined towards the right side; the internal parts were very much swollen and livid; the vagina discharged a fetid sanies. On examination the head of the child was found resting on the brim of the pelvis, and one hand was lying in the vagina, the antero-posterior diameter about one and a half inches. An injection and opiate were given, and I came off to procure further aid. In the forenoon I called again with a friend, and attempted in the first place to relieve the bladder, in consequence of the urethra being completely doubled over the brim of the pelvis, and all endeavours to relieve it by pushing up the head, proved vain; the introduction of a catheter was after many patient attempts found impossible. The bladder was consequently punctured in the median line, a little above the pubes, and an immense quantity of urine evacuated. It was now, although the child was known to be dead, deemed safer to attempt the Cæsarean operation than embryotomy:—1st From the swollen and tender state of the parts rendering laceration to a very considerable extent, not probable but certain, with the ultimate fear that from inexperience, we should not after all be able to deliver by the latter method; and 2d, from a consideration of the wonderful manner in which natives bear operations. The

operation was now performed in the usual manner, the woman bore it well, and lost no blood; the bowels were carefully returned, and the wound, about six inches long, brought together with the interrupted suture, and cleaned with cold water, the catheter was inserted in the bladder, and an opiate ordered. During the first day the woman seemed to be doing pretty well, but on the afternoon of the 23d, pain suddenly set in on the right side, and she sunk before midnight; dissection showed a soft yellow liver (old disease), considerable peritoneal inflammation; the wound in the uterus dark-coloured, and almost sloughy, that in the bladder was closed, and showed no symptoms of inflammation, and the external wound was pretty firmly united throughout its entire length. The antero-posterior diameter of the brim was, after the removal of the bladder and rectum, under $1\frac{1}{8}$ of an inch. I feel convinced that had the woman been operated on, on the first or second day of labour, she would have survived; we constantly have extensive penetrating wounds of the abdomen, which do well. And I was informed by a civilian, who saw the woman and child, that in 1833, or 1834, a woman near her full time, was gored by a bull, at Sagor. The abdominal parietes and uterus were both lacerated to such an extent, that the child was immediately delivered. The case was ultimately under the case of Dr Spilsbury, and both mother and child did well.

FORENSIC MEDICINE.

CORONER'S INQUEST AT PUTNEY AT THE REQUEST OF DR CORMACK ON ONE OF HIS PATIENTS.

An important inquest was held at Putney on the 8th, and continued by adjournment to the 14th instant, before W. Carter, Esq., coroner for East Surrey, and a highly respectable jury. The coroner stated that the inquisition was held in consequence of a letter which he had received from Dr Cormack, "anxiously requesting" inquiry into the cause of the death of deceased, Sophia Dallett.

It appears from the evidence that the patient, a young woman of fifteen, was suddenly seized with shivering, nausea, and vomiting, on the evening of Sunday, the 4th of July, after having been poorly for a day or two, for which indisposition she had taken three days before, some antibilious pills prepared and sold by a chemist, in the neighbourhood, of the name of Farmer. On the occurrence of the shivering and nausea, her sister sent to Mr Farmer's for medicine, and he, on the symptoms being described, sent a powder consisting of four grains of the *oxidum cinereum hydrargyri*, and a black draught to be taken in the morning. Next day, Monday, she was no better, and shivered again in the evening, when Mr Farmer was again applied to for medicine. His first doses being rather gentle in their operation, he now sent another powder consisting of five grains of the *oxidum cinereum*, and three grains of Dover's powder, a saline mixture to be used at intervals, and another black draught to be taken the following morning. Mr Farmer's medicines had no unusual effect, but on Tuesday, the patient being rather low, her sister sent for Dr Cormack. We give the following extracts from Dr Cormack's evidence.

"I found the pulse at the wrist feeble and irregular; the heart's action was feeble and irregular. The tongue was loaded, but not parched. She seemed to be in an extremely depressed state. I thought the pupils were somewhat contracted. I examined into the state of the secretions, and learned that the urine was high coloured and scanty, and that on that morning the discharge from the bowels had been very abundant. I asked if I could see the discharge from the bowels, and was told I could not. I asked if it was thin and watery, or if it had some consistence; I was told that it had some consistence, and was "bad" in appearance and smell. I thought that this discharge was owing to

the operation of the medicine. There was a natural moisture on the skin—perhaps a little more than the natural moisture. What particularly struck me, was the extreme depression and the contracted pupils. These symptoms indicated, or rather, I should say, led me to dread pressure on the brain, such as is seen in the course of continued fever. I learned that the disease had set in some days before with headache and shivering. This circumstance, and the state in which I found her, led me to infer that she had been for four or five days, or longer, affected with continued fever of a typhoid character—that is a low nervous fever, which (if the patient lives) runs through a definite course. I intimated this opinion to the relatives. Nervous fever of this kind is a very serious disease. In reference to the abdominal pain, which was severe, I said, “Let us hope that it is only the medicine.” I did not convey, or wish to convey, the idea that the disease arose from improper medicine. I had a dread that it might be a severe form of abdominal inflammation, well known to medical men as a dangerous complication of continued fever. In prescribing, my object was to support the powers of life, and to allay the vomiting and purging. I prescribed a mixture to be taken in two doses. (The prescriptions, as written at the time, were produced in court, at Dr C.’s request). The following is the composition of this mixture:—Of the Edinburgh solution of the hydrochlorate of morphia twelve drops, tincture of orange-peel one drachm (a teaspoonful), and of camphor mixture two ounces. The morphia in this mixture may be estimated as equal to half a grain of opium. I gave so small a quantity of opium on account of the cerebral symptoms. I thought the camphor would be useful as a diffusible stimulant. I thought it would tend to support life. Along with this mixture, I sent one ounce and a half of the creosote mixture of the Edinburgh Pharmacopœia. Of this I directed the half to be taken, one hour after the last dose of the other mixture. Creosote is of use in allaying vomiting. The opium was intended to check the discharges, and arrest inflammatory action, by keeping the intestines in a state of repose. I left the house, saying that I would return in a few hours, but if required would be found at home. I went home.

“My second visit to deceased was between twelve and one. The pulse was improved; the vomiting was going on—it had changed its character. I saw a chamber-pot about half full of what at first appeared to me, from the darkness of the room, to be urine of a whitish colour. I said to Miss Maria, “I am glad to see she has made so much water;” and was answered, “She has made no water—all that has passed from the bowels.” On carefully examining the abdomen with the hand, I found that the pain on pressure had materially increased since my first visit, and the vomiting and purging, though diminished, still continued, in spite of the remedies. At this visit I directed one part of the creosote mixture to be taken; none of it had been taken, but both doses of the other mixture had been taken. I was still careful how I used opium, which, but for the cerebral symptoms, would have been freely resorted to. I was anxious also not to mask the disease. In all the circumstances, I considered that the safest course was to order a stimulating liniment containing some opium, in the form of laudanum, to be applied to the seat of pain. I calculated that part of this opium would be absorbed. The composition of this liniment was as follows:—Spirit of turpentine, five drachms; liniment of the sesquicarbonate of ammonia, of London Pharmacopœia (the mild harts-horn liniment), five drachms; and of laudanum, two drachms. A drachm is sixty drops, or, in other words, about a tea-spoonful. I directed part of this liniment to be applied to the abdomen by means of a piece of flannel, and I directed that above the flannel hot water fomentations were to be kept up for two hours. I prescribed three pills to be taken, one every hour, after the liniment had been used for two hours, in the event of the pain, purging, or vomiting remaining. When the liniment was on, and before the pills were commenced, I directed the creosote mixture to be given, should vomiting recur. I said that wine was to be given if she became suddenly faint. I said that I would call again. I did call about eight in the evening. I found a very great apparent improvement

in all the symptoms; the pulse in particular was much better, being less rapid, as well as stronger and firmer. I was told that there had been no recent purging, and almost none at all since my previous visit. The vomiting had not returned, in consequence of which the residue of the creosote mixture had not been given. All the pills had been taken as I prescribed. The last was taken a little after six, I was told. The liniment and fomentations had been employed as I directed. All my instructions had been fully carried out. I stated to the family that I was happy to leave her so much better. I did not anticipate such a change as took place. I was called between four and five in the morning, just at the conclusion of the thunder-storm. I went without delay. I found the deceased in a state of great prostration, apparently listless, and drowsy. The pulse was hardly perceptible at the wrist: the hands were cold; the extremities of the fingers were blue; and the lips were livid. I administered about a table-spoonful of brandy, and three or four glasses of wine. I asked if she had been alarmed by the thunder; and was told that she did not seem to have heard it. I examined the pupils; they were very much contracted. I endeavoured to converse with her, and was enabled in some degree to do so, after she had wine. I at first thought that she was in a state of collapse, which would terminate in death. The good effect of the wine suggested hope. I asked how much of the liniment had been used, and was shown the bottle, which might contain one or two teaspoonfuls out of the twelve. Knowing the extreme sensibility of the Dallett family to medicine of every description, it occurred to me, *as just possible*, that the stupor and collapse might have arisen from absorption of the opium of the liniment. I had never heard of so small a quantity of laudanum, when so applied, producing such an effect. I thought it my duty, under all the circumstances, to hazard the imputation of having used improper and dangerous medicine, as the only thing which gave the deceased a chance of life, was to take this view of the case, and act on it. I used every exertion to rouse her, by sinapisms, hot water and mustard to the feet, &c. I now treated the symptoms exactly as I would have treated narcotism from opium. I only conjectured that it might be narcotism from opium. I explained my treatment to the family, as the exertions to rouse the deceased might otherwise have appeared extraordinary. No improvement showed itself. I desired my assistant, who was in attendance with me, to call in Dr Wane and Mr Shilito. When Dr Wane came, I entrusted the case to him for a few minutes, as I was exhausted, and I went to Mr Shilito's house. He kindly came back with me immediately to Mr Dallett's. Both gentlemen then co-operated with me in pursuing the same line of treatment, until death took place. Just before Dr Wane arrived, I gave twenty grains of sulphate zinc; it did not act, and he suggested a repetition of the dose, and tickling the fauces. We used the emetics with reference to the symptoms; and not with reference to my prescriptions. We all co-operated in using the same treatment, till deceased expired between eight and nine A.M. on Wednesday morning, about twenty-three hours from the time at which I was first called in. Everything which I did was done with the view of saving the patient's life. The object of my treatment was to prevent death. The treatment was adopted according to the best of my judgment.

The following is the report of the post-mortem examination by Dr Wane and Mr Shilito:—

External Appearances.—The integuments of the abdomen were of a somewhat greenish hue from incipient decomposition. There was lividity of the back, trunk, neck, arms, and lips. The articulations of the upper extremities were flaccid. There was rigidity of the lower extremities and trunk. The general habit was rather slender, but there was no emaciation. The mammæ were well developed.

Head.—The vessels of dura mater turgid, with free effusion of blood when cut. Very great congestion of the vessels of the pia-mater, and very slight subarachnoid effusion. Consistence of the brain tolerably firm. Less than usual quantity of fluid in the lateral ventricles, and this effusion was coloured

with blood. Choroid plexus and surface of ventricles congested. The base of the brain was next examined. General congestion of the vessels. The encephalon was carefully examined in every part, and presented no further appearance of disease.

Chest.—On opening the left cavity of the chest, about *two ounces* of bloody serum was found; none in right cavity. General adhesion of the pleura pulmonalis and pleura costalis of right side, of old standing. Cadaveric congestion of posterior part of both lungs. Very little serum in pericardium, which appeared healthy.

Abdomen.—General distention of the intestines with gas. The small intestines in various parts were of a bright and blotched appearance, which appearance presented itself in a more marked degree when the intestines were raised; adhesions were observed, produced by recently effused coagulable lymph. The intestine in one part was coated with coagulable lymph. A small quantity of turbid bloody fluid was found in the lower part of the abdominal cavity. The redness of the small intestines was more remarkable towards the lower portions. On slitting open the small intestines, a very marked congested red state was found, the result or evidence of inflammation. The glands of Brunner and of Peyer were remarked of unusual distinctness and size. This unnatural state increased as the great intestine was approached, so that the mucous membrane of the small intestine at this point appeared thickened for ten or twelve inches. The same portion of the small intestine was the seat of several distinct ulcerations. The mucous membrane of the ilo-cæcal valve was much thickened and reddened. The mucous membrane of the large intestines was congested; the peritoneal coat of the intestines was natural. The stomach was externally of a reddish colour; internally of the same hue. This colour depended upon congestion of the vessels, and staining of the mucous membrane. The spleen was larger than natural.

Dr Wane and Mr Shilito, both of Putney, were examined, as was also Dr Willis of Barnes, and these gentlemen were all of one opinion as to the judiciousness of Dr Cormack's treatment, and the groundlessness of any suspicion of connexion between the patient's death and the previous use of opium. Dr Willis says,—“I attended the post-mortem examination; and fully agree with Dr Wane and Mr Shilito as to the cause of death,—(peritonitis, and inflammation with ulceration of the mucous membrane of bowels, and congestion of the brain, most probably occasioned by fever). In my opinion the opium was most judiciously used. I think that when Dr Cormack first saw the deceased, the use of opium afforded the only chance she had of recovery.”

The Coroner briefly charged the jury, stating that there were two questions for their consideration,—first, whether the deceased died from natural causes, or whether her death was in any manner occasioned by the treatment adopted, and of the latter; secondly, whether Dr Cormack's treatment was such as a medical gentleman, acting with due caution and without culpable rashness, would have adopted.

The jury, after a brief consultation, unanimously found—

“That the deceased died a natural death from peritonitis; and we are of opinion that the treatment of Dr Cormack was skilful, judicious, and proper, and such as a medical gentleman with an adequate knowledge of his profession would have adopted.”

The case excited the deepest interest in the neighbourhood, particularly amongst medical gentlemen, many of whom attended during the progress of the inquiry.

CHARGE AGAINST A WIFE OF POISONING HER HUSBAND WITH ARSENIC.—ACQUITTAL.

In this case the principal evidence against the wife Elizabeth Johnson, was that she had been known to cohabit secretly with another man, that she had been heard to say by more than one witness that she preferred him to Johnson, her husband, and that if she could not have him otherwise, she would poison Johnson; that when her husband was taken ill, she pronounced that he would

never recover, and that even when the surgeon in attendance had said he was better, she declared to one witness that he would die that night, as happened. Further that she had purchased arsenic, apparently on the very day on which her husband was taken ill, and yet when apprehended, that she affirmed she did not know what arsenic was, and that she had never been in the shop in which she had bought it, though the purchase was supported by the testimony of two witnesses, one of whom knew her for more than a year as frequenting the shop. In confirmation of these suspicions, it appeared in evidence, that the deceased was a healthy man, of 42 years of age, and apparently free from complaints till the sixth day before his death, that during his illness he suffered chiefly from pain and heat in the throat, and a choking sensation, severe thirst, vomiting, and pain in the bowels. The day after his death the body was inspected, "the viscera were generally healthy, the interior surface of the stomach was highly injected, and the same appearances were presented throughout the alimentary canal, diminishing in intensity from the stomach downwards; the lips were excoriated; the gullet and bladder highly inflamed, and an unusually red or scarlet appearance of the lungs. It appears that an attempt was made to test for arsenic, at the time of the post-mortem examination by means of Reinsch's process, but that the film on the copper was not satisfactory. The body was exhumed in the beginning of March, three months after burial, when the chemical experimenters were satisfied of the presence of arsenic. Nevertheless the jury acquitted the prisoner.—*Northern Circuit, 30th March, as reported in the Times' Newspaper, April 1, 1847.*

We fear there was some mismanagement in reference to the detection of the arsenic in this case, which led the jury to distrust the medical evidence. Nevertheless we wish to speak with great reserve on this point, as the report above referred to, is not detailed with much minuteness. What strikes us as most unfortunate in the case, is the failure to detect arsenic satisfactorily at the first post-mortem examination; and its alleged satisfactory detection after the body had been interred for three months, the chief experimenter being the same chemist who had failed on the previous occasion. We can hardly doubt that this piece of mismanagement was of much weight with the jury in coming to their verdict. The only apology we can conceive for the little pains taken to ensure the same success in the first as in the second instance, is that the circumstances of strong suspicion which led to the exhumation of the body, may not have become apparent at the time of the first inspection.

Part Fourth.

MEDICAL NEWS.

EDINBURGH OBSTETRICAL SOCIETY.

SESSION VI.

FIFTH MEETING.—*May 12th, 1847.* Dr SIMPSON in the Chair.

Dr E. Houston was admitted an ordinary member.

EXPULSION OF THE ENTIRE OVUM AT THE FULL TIME.—*Dr Martin Barry* read the following case :—

Mrs M., aged 22, Dyer's Close, Cowgate, gave birth to her second child on Monday, the 2d of February 1846. States that the last catamenia began and terminated at the end of the preceding April.

The medical attendant on arriving found the first stage of labour complete; the presenting parts being the nates and right foot, in a position cor-

responding to the third position of the head; *i. e.* with the sacrum of the child directed towards the right sacro-iliac synchondrosis of the mother. "Pains" returned every three or four minutes, and the passages were not only well lubricated, but apparently very ample. With a breech presentation, however, it was not anticipated that the labour would be immediately at an end, and still less that it would be completed in the following remarkable manner.

The patient having expressed a wish to obey a call of nature, the medical attendant withdrew; but before many minutes had elapsed he was urgently requested to return, and found the child not only born, but lying with the liquor amnii in its unruptured bag of membranes, and the placenta expelled along with it; the whole having been precipitated almost without a "pain" into the *pôt de chambre*. The membranes were ruptured without delay; after which a tap on the nates and the dashing of a few drops of cold water on the chest were found sufficient to establish the free respiration of the child.

The mother is a very little woman. Some flooding followed. Forty hours after birth the child weighed 5 lb. 3 oz. 3 drachms. When born it must have weighed more, for it had not received nourishment nearly equal in quantity to the evacuated meconium.

Various similar cases were mentioned by other members.

CYST IN THE UTERUS OF A CHILD THREE MONTHS OLD, WITH ANOTHER CYST LIKE A CORPUS LUTEUM IN ONE OF THE OVARIES.—Dr T. M. Lee showed to the Society the uterus and ovaries of a child that had died of an acute disease after a few hours illness, and had previously exhibited no symptoms which indicated any derangement of the genito-urinary organs. It was during an examination of the body after death, that a vascular protuberance was discovered on one of the ovaries, similar to that which is observed when a *corpus-luteum* is present. A section of this ovary disclosed a cyst corresponding to the bulge upon its surface, having a dense fibrous capsule, thinly coated with a yellow curdy matter, and containing serous fluid. Both the ovary and the Fallopian tube of this side were much larger than those of the other side. When the vagina and the uterus were slit open, the lips of the *os uteri* appeared somewhat thicker and softer, and more pink in colour than usual; and a round cyst about the size of a small pea, and of a leaden hue, was found in the womb closely and organically attached to it, just below the opening of the larger Fallopian tube, by a fibrous base, the diameter of which was fully half of that of the cyst itself. This cyst which was filled with fluid blood, was also composed of dense fibrous tissue, and became quite white like the texture of the uterus when the blood contained in it was washed away.

IMPERFORATE ANUS.—Dr Keiller communicated two very interesting cases of *Imperforate Anus*, from Dr Lyell, Dundee. In one of the cases Dr L. had recourse to the measure usually adopted in order to establish a perineal outlet for the fæces. The operation of cutting down upon the gut, was not followed by any permanent benefit, although performed thrice during a period of six weeks, about the end of which time, however, some feculent matter was observed to pass with the urine per urethram, and in a few days afterwards the fæces totally ceased to be emitted by the artificial anus, which soon became obliterated. *The child continued quite well, passing its fæces by the urethra until it was exactly twelve months old*, when convulsions and death supervened in consequence of a complete stoppage, both of urine and fæces having taken place from obstruction of the urethra. On dissection Dr Lyell discovered that in operating, he had always cut into the urinary bladder, into which a small and imperfect gut was found to terminate, there being no regular rectum, sigmoid flexure, or descending colon present. The obstruction was found to have taken place in the prostatic portion of the urethra, which was completely filled with a substance resembling skins of raisins, coated over with brown calcareous deposit. It was somewhat remarkable that during the *first six weeks*

of this child's life, its urine was passed in a *limpid* stream, without the slightest admixture of feculent matter, which latter was, throughout that period, evacuated by the opening made by Dr L. in the perineum, from which moreover no urine was ever observed to pass.

The second case was remarkable from the child having *lived upwards of twelve weeks without any fecal outlet but that of the mouth*. Dr Lyell was prevented from attempting the formation of an artificial anus, in this case; a post-mortem examination proved, however, that no operation could have been successful, as the blind pouch of the gut was far beyond the reach of the knife. The anatomical peculiarity in this case, consisted in the colon terminating in a large globular cul de sac, which was found floating loosely among the other intestines in the umbilical region. Dr Keiller exhibited preparations from the above cases. Both were male children.

VARICOSE TUMOUR OF THE LABIA DURING LABOUR.—*Dr Burns* had seen several cases of enlarged varicose veins of the labia. In one instance they formed an immense tumour which burst shortly before the head passed, and caused a considerable loss of blood. *Dr Simpson* had met with the same several times. Some of the tumours were large; one only ruptured. The hemorrhage being venous, is like venous hemorrhage elsewhere, generally easily restrained by a little compression. If it is troublesome, the labour may be hastened by the forceps or otherwise. Dr S. stated in proof of the very vascular character of the parts at the root of the labia externa, that three instances had occurred in Edinburgh, during the last 20 years, of death from cuts or stabs in the pudendal venous plexus. In all these three cases the persons inflicting the wound had been tried in our criminal courts for murder.

Wednesday, 12th May.

PREGNANCY WITH TUMOURS OF THE UTERUS.—*Dr Martin Barry* read the following case, which had been communicated to him by Mr Robert Dashwood, of Beccles, Suffolk.

"Mrs W. B., æt. 29, had never been pregnant, though married 10 years. In the latter part of January 1846, she complained of constant pain in the left iliac region. After the bowels had been freely emptied, the pain still continuing, I examined the abdomen, and found a firm resisting tumour about the size of an orange, with inequalities on its surface, situated above the pubes towards the left side, and perfectly moveable. Mrs B. had not ceased to menstruate until at the last period, when she described herself as being ill only one day, and supposed that she had at the time taken cold, and that her illness was attributable to it. I obtained an examination per vaginam, a few days afterwards, and with difficulty reached the os uteri, it being situated high up. The neck of the uterus seemed healthy, and the body of that organ did not appear to me enlarged: the os itself was closed. Lifting the uterus with the finger evidently affected the position of the tumour. Considering Mrs B. not to be pregnant, and that the enlargement was probably a tumour in some way connected with the uterus itself, I urged her to proceed to Norwich and take the opinion of my friend Mr P. N. Scott, whose extensive experience in obstetric practice pointed him out as most capable of deciding on the nature of the case. Accordingly, on the 9th of February, Mr Scott examined Mrs B. with me; and, after a long and careful investigation, came to the conclusion that there was no ground to believe her pregnant (the state of the mammæ and umbilicus militating against that opinion), and that the tumour was not ovarian, but probably connected with the uterus itself. He advised the progress of the case to be watched, and only temporizing measures to be employed.

"From this time the tumour kept rapidly increasing; three separate masses became distinguishable,—one very hard, tender, and projecting from the mass

of the tumour, was situated just below the umbilicus,—the two others occupied respectively the right and left iliac regions. Mrs B.'s health became much impaired. The os uteri gradually ascended above the pubis, out of reach of the finger; and an incompressible and apparently immovable mass—pushing forwards the posterior wall of the vagina—gradually descended, filling up the pelvic space so as to admit a finger with difficulty between it and the pubis.

“Mr Scott visited Mrs B. in the month of June, when, while examining the size of the tumour through the abdominal parietes, he suddenly felt what he could not doubt to be the movement of a living foetus. From about this time Mrs B.'s health improved, the usual signs of pregnancy were developed, and every thing went on well; when, on the 1st of September, slight pains set in, which continued until mid-day of the 4th, at which time parturient pains commenced. Mr Scott, who had kindly offered to give me his valued assistance at the time of delivery, and Mr Henchman Crowfoot of Beccles, visited her with me soon after; and I am much indebted to the judgment and skill they evidenced on this occasion. The os uteri was found with difficulty by turning the palmar aspect of the finger towards the pubis. It was dilatable and dilating, with a foetal head presenting through the membranes. The tumour, situated as described above, gave no hope of its containing fluid, so as to admit of being reduced in size by puncturing. At about 9 p.m., the membranes gave way, and the pains became stronger. While making an examination, Mr H. Crowfoot perceived a yielding of the tumour; which by forcible pressure was made to retire higher up into the abdominal cavity, thus allowing more space between it and the pubis. This space was gradually and by the same means increased, until with the assistance of the lever, it allowed the passage of the foetal head, the pains being rendered more powerful by a dose of ergot; and delivery of a still-born child was happily accomplished at about 1 a.m. of the 5th.

“Six weeks subsequently, an examination was made; when the mass of the tumour was found to be equal in size to a uterus at the 7th month, of a lobulated character, and having three remarkable projections from it,—one of them extending up to midway between the umbilicus and ensiform cartilage. Inferiorly it pressed strongly on the rectum, and filled the whole cavity of the pelvis.”

LARGE FIBROUS TUMOURS OF THE UTERUS, COMPLICATING PREGNANCY AND PARTURITION.—*Dr Simpson* stated reasons for believing that in the above case, the existing compound tumour was the common fibrous or fleshy tumour of the uterus. It was well known that fibrous tumours were very common organic changes in the unimpregnated uterus. He had seen several cases in which, when still small, they were found present, and showed themselves in the form of nodulated irregularities on the surface of the pregnant and puerperal uterus; so that they did not prevent conception. Further, he mentioned the particulars of three cases which he had met with, where fibrous tumours of a large size complicated pregnancy and parturition.

Case 1.—A patient after being married for 10 or 12 years without any issue, passed two menstrual periods. A very large pelvic tumour, which had been long present, began to increase in size. Dr Simpson then saw her along with Mr Dixon and Dr Taylor. There was a very large hard fibrous tumour of the uterus, and low on the left side and in front, a soft elastic part, having much the character of a dropsical ovary. And it is well known, that fibrous tumours of the uterus and multilocular dropsy of the ovary sometimes coexist; but Dr Simpson knew no other kind of cystic collection ever existing along with fibrous uterine tumours. In this case, however, the soft fluctuating part was not the ovary, for it was situated *anteriorly* to the hard uterine tumour, whilst diseased ovaries lie *posteriorly* to the uterus. This led Dr Simpson to suspect pregnancy, improbable as it otherwise was, and to hazard a diagnosis to that effect. It proved correct. The foetal heart was heard about the fifth month,

and pregnancy went on the full time. During labour, one portion of the tumour filled up so much of the brim of the pelvis, that the child required to be extracted by turning. It was still-born. The mother made a good recovery.

Case 2.—A patient applied to Dr Simpson with a large pelvic tumour. She was suffering from dysuria, &c., and had not menstruated for three months. Dr Simpson found the uterus retroverted, and an enormous fibrous tumour in the walls of the organ. The case was at this time seen by Dr Renton, Dr Ziegler, and others. The uterus was replaced by Dr Simpson with difficulty, and some weeks subsequently the foetus was expelled. About a year afterwards, the same patient again became pregnant and went to the full time. The labour was extremely tedious—a portion of the tumour diminishing the brim of the pelvis—and at last the child was expelled by the spontaneous efforts of the uterus, but with the head greatly compressed and flattened. The mother recovered rapidly. In the unimpregnated state, the fibrous tumour in this instance reaches to the umbilicus, and is as large as a uterus at the fifth or sixth month of pregnancy. The uterine cavity is found by the uterine sound to be six inches in length.

Case 3.—A patient, pregnant for the first time, who had been long delicate, arrived at the full term of utero-gestation, and after a somewhat tedious labour, was delivered of a dead child. There was a slight degree of hemorrhage, but it was easily arrested. From the time, however, of delivery onwards, the patient continued to sink—became faint and listless, and then comatose—and died in this state five or six hours subsequently. She seemed never to rally from the “shock” accompanying delivery. Dr Malcolm saw her with Dr Simpson. On opening the body, they found the uterine parietes thickly studded with fibrous tumours, and counted as many as forty hanging in a more or less pediculated form, from its external or peritoneal surface; and of all sizes, from an orange downwards. They were easily diagnosed through the abdominal parietes during her life. The cavity of the uterus contained no collection of blood.

In addition, Dr Simpson alluded to two cases of large fibrous tumours complicating labour, published in the Dublin Medical Journal, the one by Dr Montgomery, the other by Dr Beatty. In the former, the fibrous tumour having entered the pelvis, formed such an obstruction to the passage of the child that the Cæsarean section was required. In Dr Beatty’s case, it was supposed, before the commencement of the labour, that the same proceeding would be necessary; but, in the course of the labour, the tumour was gradually raised by the uterine contractions out of the pelvis, and the child spontaneously expelled.

WHAT PRACTICE SHOULD BE FOLLOWED IN CASES OF LARGE FIBROUS TUMOURS COMPLICATING PREGNANCY.—*Dr Simpson* adduced the opinion of Dr Ashwell, who, in discoursing on this subject, inculcates the propriety of inducing premature labour in order to evade the danger of inflammation of the pelvic tissues and peritoneum, and the still more hazardous evils of unhealthy softening, suppuration and ulceration of the tumours themselves. But Dr Simpson stated that he entertained very serious doubts of the correctness of the observation of Dr Ashwell, that fibrous tumours had a tendency to soften during the latter months more than at any other period of pregnancy; and disapproved of the induction of premature labour as a general rule of treatment in such cases, believing, as he did, that the excitement of the uterus by artificial means to the premature expulsion of its contents, would be as likely to induce such anticipated morbid actions, as the supervision and completion of a natural pregnancy and labour. He believed, that the only cases of this kind which demanded the induction of premature labour were those in which the tumour encroached upon the brim of the pelvis, and thus produced such contraction of the maternal passages as rendered a natural labour impossible.

TREATMENT OF FIBROUS TUMOURS IN THE UNIMPREGNATED UTERUS.—*Dr Simpson* called attention to the manner in which nature sometimes proceeds to stop the progress of these tumours; namely, by the gradual transformation of their fibrous tissue, firstly, into a cartilaginous and then into an earthy and almost inorganic mass, not prone to enlarge or change its condition. He stated, that this alteration seemed to indicate the death, or at least the cessation of the reproductive action of those cells which form the essential growing constituent of the tumour. He cited, in illustration, the analogous case of some entozoa, particularly the *trichina* and *cysticercus*, which, after their death, were sometimes found, with their containing cysts, ossified. Further, *Dr Simpson* mentioned the interesting experiments of *Rayer*, in which that pathologist induced the artificial transformation of normal fibrous tissue (such as the ear of the rabbit), into cartilaginous and osseous substance by the repeated or continued irritation of it. And, as a result of these remarks, *Dr Simpson* suggested the possible induction of osseous transformation as an indication of treatment in fibrous uterine tumours. He thought that the repeated transmission of a galvanic current through the tumour might possibly produce the required degree of irritation and its desired result.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

Wednesday, 2d June 1847.—*Dr HAMILTON*, President, in the Chair.

Dr Lee read a paper entitled two fatal cases of Tetanus, with dissections.

The patients were two girls; twins: residing in Jamaica Street. They had both been complaining for about two days, before unequivocal tetanic symptoms set in on the morning of the 15th of April, during which they suffered from some derangement of the bowels, with anorexia, white tongue, and a tendency to spasm. They had no diarrhoea. The urgent tetanic symptoms terminated fatally in the one case in from three to four hours, in the other in eight or ten hours. Contagious fevers were prevalent at the time in the same common stair.

On dissection, both the brain and spinal cord were found to be quite healthy; and there was no evidence in either of inflammatory disease in any part of the body; but the intestinal glands exhibited a morbid state, which *Dr L.* considered characteristic of entero-mesenteric fever in a very early stage, an opinion in which he stated he was joined by *Professor Goodsir*, who had also examined the intestines.

Dr Lee concluded by reading an unpublished case of a similar kind which occurred to *Mr Goodsir* in *Anstruther* in 1834, when dothenenteritis was prevalent there. In this case the patient was a young man of nineteen years; the tetanic symptoms came on early on the seventh day, and death followed the same night. The only morbid appearance was that of the glands of the intestines, which characterises the entero-mesenteric fever. *Dr L.* exhibited preparations (injected and uninjected) of the intestines of the two children, which belong to the Anatomical Museum of the Edinburgh University, and mentioned that *Professor Goodsir* had informed him that specimens of the diseased gut in his case are preserved in *Dr Bennett's* private collection.

Dr Simpson described the case of a child at the ninth month of pregnancy passing, without operative aid, through an oblong opening under 1 inch in its short or transverse, and $2\frac{3}{4}$ inches in its long diameter. This communication was printed at length in our last number.

Dr Wiseman of Cupar, who had been engaged to attend on the mother, said that on his first visit, discovering the pelvis to be very much distorted, he had requested the advice of *Drs Graham* and *Grace* also of Cupar. This was

from six weeks to two months before delivery. They agreed in considering delivery impossible, and the question arose as to the induction of premature labour. In this they found they could not succeed, as they could not reach the os uteri. They then thought of the Cæsarean section, and Dr Simpson, on being consulted, agreed to perform it. When Dr Wiseman was sent for on the occurrence of labour, he found the head bulging down through the external parts. The mother had a good recovery. Dr Simpson had correctly stated the dimensions of the pelvis, and the singularity of the case could only be explained by the great extent of the putrefaction. The pains had begun about six hours before delivery was accomplished.

Dr Sanderson of Musselburgh asked what was the state of the brim of the pelvis?

Dr Simpson said it could not be ascertained with any exactness,—there was a doubling in at the pubes, but no depression of the sacrum posteriorly.

Dr Sanderson made some observations in reference to the state of the brim in Dr Osborne's case.

Dr Simpson said that Dr Osborne had represented the narrowest diameter as less than it was in fact, as he had drawn the uterus to one side where the dimensions were greater than those stated by several British authors as sufficient for delivery by the natural passages.

In answer to a question by Dr A. D. Campbell, Dr Simpson said the weight of the infant was 3 lbs. 2 oz.—the length $18\frac{1}{2}$ inches, and that it was in a true state of intra-uterine maceration.

SESSION XXVI. MEETING 13th. Dr HAMILTON, President, in the chair.

Dr Balfour of Corstorphine read a paper entitled “Notes on the Practice of Skoda,” contrasting his statistics with those of the Edinburgh Infirmary; and showing, amongst others, that under his semi-expectant system, pneumonia has a mortality of only 1 in 7.26, the average duration of treatment being 10 days; while in the Edinburgh Infirmary the mortality from pneumonia amounts to 1 in 2.78, and the average duration of treatment of that disease is 22 days. Other diseases were also commented upon, and the general inference drawn,—that heroic treatment is at least unnecessary, and the natural-eclectic system of treatment entitled to a fair trial.

Dr Gairdner was not disposed to agree with Dr Balfour's conclusions. Without at all impeaching the good faith of the author's account of the results of Skoda's practice, other explanations than those he had adopted were applicable to the cases, and he thought it wiser to stand by such explanations. Nothing was more certain than that important changes had taken place on the character of acute diseases. He felt no doubt that great changes had occurred in the acuteness of inflammatory diseases in the experience of all who had been any considerable time in practice. For the truth of this he appealed to the President. Foreexample, croup was much more frequent in Edinburgh when he was a student than now. Then, croup was a frequent and fatal disease in the old town, where now it is almost unknown. Acute pneumonia was then also much more severe, and required larger blood-lettings than are usually requisite now. He thought that nothing was better established than the good effects of blood-letting in the acute pneumonia of Edinburgh, whatever might be the case at Vienna. And he must insist on it being kept in mind, that the practice recounted in the paper belonged to Vienna, not to Edinburgh. Dr. G. then proceeded to offer various illustrations of the same views from his own experience, and the experience of others; and remarked on the character of the diseases in hospital patients in general, as ill fitted to show the effects of active treatment; contending that in Edinburgh the experience gained in dispensary and private practice was a safer guide as to the requisite extent of active measures than hospital practice, inasmuch as hospital patients have usually reached that more advanced period of the disorder when every judicious phy-

sician would either avoid bleeding, or employ it much more sparingly. Of the benefits of *early* blood-letting he entertained no doubt whatever; they were positive, immediate, unequivocal, and admitted by almost every physician whose experience and judgment entitled him to consideration; and if Dr Balfour or any one else could shake his conviction in the truth of this opinion, he would also succeed in producing in his mind a general distrust of medical evidence in all cases of every description; since in no case whatever can we have evidence which is stronger and more satisfactory. The question being, therefore, with regard to the utility of the medical profession in the cases calling most strongly for the interposition of one of the most efficient means which they possess, he felt himself called upon, with all due respect for Dr B., to make these few remarks.

Dr Bennett fully agreed with the remarks of Dr Gairdner, but attributed much of the change which has latterly occurred in practice to an improved pathology and diagnosis, whereby the nature of diseases was better understood, and their detection rendered more exact. Dr Balfour had attempted to establish the benefits of "a do-nothing practice," from the results of statistics. Medical statistics, as he would endeavour to show at the next meeting, were altogether fallacious, and undeserving the slightest confidence. In no Institution in the world was greater attention paid to statistics than in the Royal Infirmary of Edinburgh; and yet he was convinced that, for practical purposes, they were utterly useless. Dr Bennett concluded by strongly condemning the system of practice lately sought to be introduced by Dr Forbes and others, founded upon fallacious statistics, to the exclusion of pathology, diagnosis, and the experience of the most eminent men.

Dr Taylor referred to the results of the heroic treatment (of which copious blood-letting formed a principal part) in some of the Italian hospitals, under which the mortality amounted to only 2, 14-15ths per cent., the incurables being excluded.

Dr Balfour would only ask gentlemen to go and witness the practice for themselves.

Dr Gairdner did not affirm that patients in pneumonia never recover without blood-letting, he merely thought it safer to bleed.

Dr Ransford wished to know the prevailing temperament of the patients in Skoda's hospital; in China, owing to the physical constitution of the patients, it was impossible to take blood in inflammation.

Dr Balfour said the lymphatic temperament was predominant among the patients in the German hospitals; but Skoda's practice was equally successful in persons of the sanguine temperament; the merits of Skoda's treatment were speedy recovery without loss of strength.

ON A NEW FORM OF SEROUS INFLAMMATION. By Dr HUGHES BENNETT.—In many cases which had come under Dr Bennett's notice of apparent inflammation in the serous membranes, nothing whatever could be discovered after death but a slight effusion of serum. The membrane itself presented its usual white glistening appearance, free from all vascularity or deposition of lymph. The effusion in consequence had been considered non-inflammatory, and the connexion between the symptoms and morbid appearances had been involved in the greatest mystery. An examination of such effusions with the microscope, however, had proved that in many such cases they contained pus cells in considerable quantity, and it seemed possible that such purulent formation was intimately connected with the symptoms which preceded death. In illustration of this observation, Dr Bennett described three cases out of several he had met with.

The first case was one of fever under the care of Dr Andrew, which appeared to be going on well, when one day, during the visit, the patient was seized with a distinct rigor, and pyrexia for two subsequent days, and died during the following night. On examination no lesion whatever

could be anywhere discovered, with the exception of about one ounce of serous fluid of a golden yellow colour, and very slightly turbid in the peritoneum at its most depending part. The membrane itself was perfectly healthy. A microscopic examination of the fluid showed it to contain numerous pus corpuscles.

The second case, was that of a woman who entered the Infirmary under Dr Douglas, with occasional convulsions, which had come on two days previously. She presented such marked symptoms of head affection as led to a suspicion of meningitis. She died in three days. On examination the membranes and substance of the brain were found quite healthy. Both lateral ventricles, however, contained each about 3j of an opalescent fluid, on examining which microscopically, numerous compound granular corpuscles were observed in it.

The third case occurred to himself. It was that of a man who, for some time, had laboured under bronchitis. The day previous to his death, he was seized with pain in the cardiac region, tumultuous action of the heart, but without any murmur which could be detected by auscultation. He died during the night, and there was found, on dissection, considerable emphysema of the lungs with bronchitis. The pericardium presented a perfectly healthy appearance, but contained about half an ounce of an amber coloured fluid, slightly turbid. A microscopic examination of this fluid exhibited numerous pus corpuscles in it, as in the first case.

Several other instances had occurred to Dr Bennett, where inflammatory products could, in like manner, be detected in the effusions of serous cavities, which had been considered by practitioners of little importance, although, in his opinion, capable of explaining many anomalous symptoms.

Dr Bennett also alluded to the importance of minutely examining the fluids in contact with mucous membranes. He had, on several occasions, detected pus globules in the urine, when nothing was visible in that fluid to the naked eye, and when it seemed perfectly healthy after a most careful chemical examination. In one such case in which Dr Christison was consulted, and in which the urine was examined with great care, the symptoms of chronic pyelitis under which the individual laboured were inexplicable, until the microscope detected the pus globules. On the other hand, he had seen several cases, where fluids having, to the naked eye, all the appearance of pus, had been found in the bladder, ureters, or pelvis of the kidney, and yet not one pus corpuscle could be discovered, but only an immense number of epithelial cells, which had been thrown off in large quantities, and communicated to the urine a turbid, purulent looking character. This had been well seen in two kidneys he had recently examined with Dr MacLagan.

Dr Tait read a portion of his paper entitled "Observations on Opium-eating."

PROFESSOR SYME, AND MR CÆSAR HAWKINS OF LONDON, ON THE TREATMENT OF FUNGUS OF THE TESTICLE.—Mr Cæsar Hawkins of London in a lecture which he lately delivered on the Treatment of disease of the Testis, made the following allusion to Mr Syme's practice: "I was amused lately by seeing this practice running the round of the journals as an improvement lately introduced in Edinburgh by Mr Syme, whereas you may see it in Sir Astley Cooper's work on Diseases of the Testis, which is in every body's hands." In reply to a letter sent him by Mr Syme, inclosing a copy of the paper formerly published in this Journal, Mr Hawkins remarks, "I meant the observation to apply solely to the printers or conductors of the several Journals, who put in the table of contents on the covers and on the headings of the pages, the words 'New Operation for the Cure of Fungus Testis;' and who, I *imagined*, had mistaken an allusion by you in a clinical lecture, to Sir Astley Cooper's practice for a new proposal of your own which you had not intended to call it."—*Med. Gazette*, June 25.

From this curious explanation, it would seem that Mr Cæsar Hawkins' sur-

gical reading is confined to the covers of Journals, and that his pupils are favoured with the reflections which naturally result from such profound literary researches into the labours of his contemporaries!

ARMY SURGEONS MADE EXECUTIONERS OF THE LAW.—The following order has been lately issued from the Horse Guards:—"June 25, 1847.—Circular Memorandum.—The mode of marking deserters by means of an instrument prescribed by the circular letter of 5th May 1842, having been found ineffectual and easily erased, the Commander-in-Chief has, on the recommendation of a board of influential medical officers, to whom the subject has been referred, been pleased to direct that the use of the said instrument be discontinued, and the method of marking with needles resorted to throughout the service. His Grace the Commander-in-Chief farther desires that the operation of marking deserters may henceforth always take place in the hospital, under the superintendence of a medical officer, who will be held responsible that it is effectually performed, and that the letter 'D.' shall be indelibly impressed on the skin.—By command of Field-Marshal the Commander-in-Chief.

JOHN MACDONALD, Adjutant-General."

This command has already excited the attention of foreign journalists. The *Union Medicale* observes, "We do not know what impression this circular has made upon the medical men in the English army. We are ignorant whether they will submit to this revolting measure, but we assure them with confidence, that there is no surgeon in France who would sully the dignity of his profession by such an odious operation."

In the record book of the Tolbooth of Edinburgh for the 16th century, there is an entry in regard to a woman who had been imprisoned for "drinking the health of the devil and all his angels." For this crime she was sentenced to have her "tongue bored with a red hot iron." But even at that rude period of our history, this was to be executed "by the hands of the common hangman." But now, it is said "on the recommendation of a board of influential medical officers," army surgeons are to be held responsible that the new operation is effectually performed. Are teachers then, in future, to instruct medical students in the new art of tatooing deserters,—is the Professor of Military Surgery in our University to show his pupils how to perform this degrading service? We can scarcely believe it, and we call upon him, and all who have the honour of the profession at heart, to protest against this insult offered to the medical body.

BOOKS RECEIVED.

1. On Pulmonary Consumption, and on Bronchial and Laryngeal Disease, &c. By Sir Charles Scudamore, M.D., F.R.S., &c. London. 8vo. 1847.

2. Anecdota Sydenhamiana: Medical Notes and Observations of Thomas Sydenham, M.D. Second Edition. Oxford. 18mo. 1847.

3. On the Duties of Physicians resulting from their Profession. By the late Rev. Thomas Gisborne, M.A. Oxford. 18mo. 1847.

4. An Account of the late Epidemic of Scarlatina in Newcastle and its Neighbourhood. By Edward Charlton, M.D. Edin. Newcastle-upon-Tyne. 8vo. 1847.

5. Vaccination considered in relation to the Public Health, &c. By John Marshall, Surgeon, &c. London. 8vo. 1847.

6. Report on the Recent Progress of

Chemistry, in its relations to Medicine. By George Edward Day, M.A., &c. (From Ranking's Abstract)

7. Report of the Directors of the Montrose Lunatic Asylum, for the year ending 1st June 1847. Montrose. 8vo. 1847.

8. The Consciousness of Right and Wrong, a just test of the Plea of Partial Insanity in Criminal Cases, illustrated by the case of William Stalker, indicted at the Cumberland Assizes, 1847, for the Murder of his Wife. By Charles Lockart Robertson, M.D., Medical Staff, attached to the Military Lunatic Asylum at Yarmouth, &c. 1847.

9. Report on the Fever at Boa Vista. By Dr M'William. Folio, pp. 112.

10. Letter addressed by Sir William Pym to the Lords of the Council, relative to a Report on the Fever at Boa Vista. By Dr M'William.

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No. 15. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Etherization in Surgery*: PART I.—*Its Effects; Objections to it, &c.* By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh.

“The multiplied experiments to prevent pain in surgical operations, which bear so delightful a testimony to the humanity of their authors, will certainly, in the course of time, be crowned with success.”—*Marx’ Akesios—Letter to Herman Boerhaave.*

MERE OPINIONS AND PREJUDGMENTS NOT SUFFICIENT TO SETTLE THE QUESTION OF THE PROPRIETY OR IMPROPRIETY OF ETHERIZATION: ILLUSTRATION FROM THE HISTORY OF VACCINATION.

DURING the latter half of the last century, 30,000 individuals were computed to die annually of small-pox in England.¹ From the official returns of the Registrar-General, it appears, that in England and Wales the number that perish annually of this same disease at

¹ Dr Gregory observes, “The total deaths by small-pox throughout England were estimated at about 45,000 annually.”—*Cyclopaedia of Medicine*, vol. iv. p. 402. Dr Haygarth calculated the annual number of deaths from small-pox to amount to 38,000 in 8,000,000 of inhabitants.—See the data of his computation in his “*Sketch of a Plan to exterminate Small-pox*,” 1793, p. 144. In making the various computations regarding vaccination in the text, I have, in order to avoid the possibility of error, kept all the calculations considerably below the ascertained data.

the present time is reduced to less than 10,000.¹ In England alone, therefore, the absolute mortality from small-pox is less by 20,000 a year than it was half a century ago. If a similar rate of reduction in the number of deaths from small-pox holds good—as we have every reason to believe is the case—in the other kingdoms of Europe, then, out of the 220 millions of people that inhabit this quarter of the globe, 400,000 or 500,000 fewer die now of small-pox, than, with a similar population, would have died from this malady fifty years ago.—In other words, according to this rate of computation, there are now preserved from death by small-pox in England, during the currency of a single half century, a number of lives greater in amount than the whole existing population of Wales. There are preserved in Europe, during the same period, a number of lives greater in amount than the whole existing population of Great Britain.

For this mighty triumph of medicine over one of the most loathed and dreaded forms of human disease and death, science stands indebted to the inestimable discovery of Dr Jenner;² and every medical man is ready to allow, at the present day, that his discovery is not less remarkable in consequence of its gigantic results and amazing success, than in consequence of the singular simplicity and safety of the means with which that success is obtained. For no one now dreams of ever expecting any deleterious or dangerous consequences to ensue from Vaccination; and, indeed, the performance of it has been mainly or entirely conducted, in some districts, by non-

¹ During the five years from 1838 to 1842 inclusive, there died, on an average, 8893 individuals yearly of small-pox. In 1842, only 2715 died.—See *Sixth Annual Report of the Registrar-General*, p. 514. Formerly, 1 in about every 250 of the general population died annually of small-pox; now, only 1 in about every 1700. In England, the registration of every birth and every death is properly enforced by law. If the registration of the vaccination of each child were enforced as rigorously as the registration of its birth, much disease, and many thousand human lives, would thus undoubtedly be saved annually in Great Britain. Surely it is a subject well worthy of the attention of a benevolent legislature. We see the good effects of such interference in other European states. For, whilst in England (the native country of Jenner) still 1 in every 1700 inhabitants dies annually of small-pox,—in Austria, 1 in 4800 dies of this disease; in France, 1 in 11,000; and in Sweden, only 1 in 27,000. On the great extent of the number of individuals in society who remain unvaccinated, see some excellent remarks by Dr Stark, in the *Edinburgh Medical and Surgical Journal*, No. 161.

² In answer to those who have affected to doubt entirely the utility of physic and physicians, medical science may proudly point to the results of vaccination. During the long European wars connected with and following the French Revolution, it has been calculated that five or six millions of human lives were lost. In Europe, vaccination has already preserved from death a greater number of human beings than were sacrificed during the course of these wars. The lancet of Jenner has saved far more human lives than the sword of Napoleon destroyed. On these devastating European wars England lavished millions of money, and freely bestowed honours, peerages, and heavy annual pensions upon the soldiers who were most successful in fighting her battles, and destroying their fellow-men; she grudgingly rewarded Jenner with twenty thousand pounds for saving twenty thousand of her subjects annually.

professional individuals,—by the priest, as well as by the physician,—by the nurse, as well as by the surgeon.

Yet at the time of Dr Jenner's first public announcement of Vaccination in 1798, and for many years subsequently, the proposal of substituting vaccine for variolous inoculation was encountered by various members of the profession, with incredulity, and ridicule, and direct and determined opposition. The measure by which he taught medical science to save annually from death, thousands of human lives in England, and hundreds of thousands throughout Europe, was, on its first introduction, bitterly denounced and decried in different quarters,—its effects doubted,—and its own safety and propriety strongly and strenuously called in question.

Dr Squirrell earnestly and publicly supplicated his Majesty George the Third to suppress “the destructive practice of vaccine inoculation throughout his dominions.”¹ “It ought,” observed Professor Monro of Edinburgh, “to be prohibited by Act of Parliament.”² “The College of Physicians have,” exclaimed Dr Moseley, “a duty to perform, and I trust this business will not escape them.”³ Others, despairing of interference on the part of the King, Parliament, or Colleges, appealed to the people themselves. “It would,” said Dr Brown, “undoubtedly be downright madness to imagine *they* will condescend to encourage it.”⁴ The Anti-Vaccinarian Society called upon the public “to second their efforts in supporting the cause of humanity against cow-pox injuries,” and besought their aid to suppress “the cruel despotic tyranny of forcing cow-pox misery on the innocent babes of the poor—a gross violation of religion, morality, law, and humanity.”⁵

Frightful, and even fatal, consequences were boldly averred to be the direct and immediate results of vaccination.

Deaths from cow-pox inoculation were published in the mortality bills of London.⁶ “I have,” alleged Dr Moseley, physician to the Chelsea Hospital, “seen children die of the cow-pox without losing the sense of torment even in the article of death.”⁷ Dr Rowley, physician to the St Marylebone Infirmary, professed to publish true accounts of fifty-nine deaths from “cruel vaccination;” and added, that “when humanity reflects” on these and (to use his own words) “a great heap of victims diseased for life, and likely to transmit to posterity, for ages, beastly chronic diseases,⁸ it is enough to

¹ Observations on the pernicious consequences of Cow-pox Inoculation. 2d edition. London, 1806. P. vi.

² Edinburgh Medical and Surgical Journal, vol. xv. p. 64.

³ A Treatise on the Lues Bovilla. 2d edition. London, 1805. P. xiv.

⁴ A Letter in Reply to the Surgeons of the Vaccine Institution. Edinburgh, 1809. P. 96.

⁵ See their Address of 1806 in Blair's Vaccine Contest, p. 56.

⁶ Mr Blair's Pamphlet. P. 95.

⁷ Treatise, p. 95.

⁸ Cow-pock Inoculation; with the Modes of treating the Beastly new Diseases produced by Cow-pock. 2d edition. 1805. P. 128.

freeze the soul with horror." And "it is," he exclaims, "the duty of honourable men in the medical profession to alarm mankind of the impending danger of vaccination; to warn society of the multifarious evils that await them in the form of this mild catholicon, of a sweetened potion that carries fatal poison in all its destructive particles."¹ He elsewhere eloquently declaims against "affectionate parents being robbed of their serenity, and the minds of tender mothers being wrung with eternal suspense," "while a few projectors or visionists are pursuing their deleterious projects on human victims," and perpetrating a "dangerous innovation which so many fatal facts illustrate."²

Mr Lipscomb urgently maintained, in an essay on small-pox inoculation published in 1805, that cow-pox, the "new scourge industriously dispersed to afflict the children of men," is "sometimes fatal of itself, and that the diseases introduced or brought into action by it may be also sometimes fatal, and can never be completely guarded against."³ One author had seen "numerous instances" of vaccination producing eruptions, remaining "for months and even years afterwards, undermining the constitution, and very frequently terminating in phagedenic or corroding ulcers." And he had likewise witnessed coughs, dyspnœa, hectic marasmus, tedious and difficult to eradicate, &c., result from cow-pox. "Shocking reflection (he adds) to a humane mind, that a poison should thus be introduced into the human constitution without the plea of necessity, or the support of reason and experience."⁴ "Several children," observes Dr Moseley, "have died from diseases brought on by the cow-pox where no ulcerations had appeared, and others have lost their nails and ends of their fingers, several months after the inoculation."⁵ "My accounts from the country are (he continues) full of dismal histories of ulcerated arms and mortifications."⁶ "Blindness (it was averred), lameness, and deformity had been the result of employing the vaccine in innumerable instances, and its fatal venom had removed many an infant untimely from the world."⁷

Nay, it was strenuously maintained and believed, that not only were various old maladies, peculiar to man, thus excited into action by the "cow-pox poison," but that different new diseases peculiar to the cow were sometimes communicated to the human constitution by vaccination. "Various beastly diseases," writes Dr Rowley, "common to cattle, have appeared among the human species since the introduction of cow-pox, as cow-pox mange, cow-pox abscess,

¹ Cow-pock Inoculation; with the Modes of treating the Beastly new Diseases produced by the Cow-pock. 2d edition. 1805. P. 14.

² Ibid. p. 128.

³ Inoculation for the Small-pox vindicated, &c. 1805. P. 40.

⁴ Observations addressed to the Public on the Cow-pox, pointing out the dreadful consequences of this new Disease, so recently and rashly introduced into the Human Constitution. By R. Squirrell, M.D. 1805. Pp. 16, 17.

⁵ Treatise on the Lues Bovilla, p. 118.

Ibid. p. 92.

⁷ Moore's History of Vaccination, p. 39.

cow-pox ulcer, cow-pox gangrene, cow-pox mortification, and enormous hideous swellings of the face, resembling the countenance of an ox, with the eyes distorted, and eyelids forced out of their true situation; diseased joints,"¹ &c.

This was published in 1806, eight years after Dr Jenner's first essay on vaccination appeared. During the year subsequent to the first public announcement of his discovery, Dr Moseley suggested the possibility of the "bestial humor" of cow-pox producing "a brutal fever exciting incongruous impressions on the brain;" and "who knows (says he) but that the human character may undergo strange mutations from quadrupedan sympathy, and that some modern Pasiphæe may rival the fables of old?" Some, after vaccination, were actually supposed to "cough like cows," and "bellow like bulls."² And one anti-vaccinist ingeniously suggested that if cow-pox were known to have existed in a family, this fact might debar the members of it from the chances of matrimony. For³ "it would (he remarks) be no letter of recommendation, and it would be cruel for the world to know, who had laboured under the cow-pox mange, evil, ulcer, or any other beastly disease; it might infallibly injure their fortune in life, particularly in matrimonial alliances. Who would marry into any family, at the risk of their offspring having filthy beastly diseases?"

Nor were theological reasons, of course, wanting for calling in question the orthodoxy of vaccination, as of other new discoveries and practices.⁴ "Small-pox (argues Dr Rowley) is a visitation from

¹ Cow-pox Inoculation, p. 105. See prefixed to the work the coloured portrait "of a cow-poxed, ox-faced boy," with two scrofulous abscesses, which were at one time alleged to indicate sprouting horns!—"This boy (observed Dr Rowley, in a clinical lecture on the case) is gradually losing the human lineaments, and his countenance is transmuting into the visage of a cow." (*Moore's History*, p. 46.) He further wrote—"A great number of new complaints, the diseases of beasts, filthy in their very nature and appearance, in the face, eyes, ears, with blindness and deafness, spreading their baneful influence over the whole body, have been not unfrequently the consequence evidently of cow-pox inoculation; either originating from the grease in horses, or the natural diseases of cows." P. 12.

² Mr Ring, in his treatise on cow-pox, mentions "a lady who complained that, since her daughter was inoculated, she coughs like a cow, and has grown hairy all over her body; and Mr Blair was told, on a late excursion into the country, that the inoculation of the cow-pox was discontinued there, because those who had been inoculated in that manner bellowed like bulls!"—*Blair's Vaccine Contest*, p. 69.

³ Introduction to Rowley's pamphlet, p. vii.

⁴ As for example, small-pox inoculation: see a "Sermon against the dangerous and sinful practice of Inoculation, preached at St Andrew's, Holborn, in 1722, by Edmund Massey, M.A. He urges various theological arguments against the 'diabolical operation' of inoculation, and at last maintains that, even if it were medically successful, it was not to be courted, for he believes if mankind should thus 'happen to become more healthy, it is a great chance but they would be less righteous.'" P. 26. In his admirable "Account of the Inoculation of Small-pox in Scotland (1765)," Dr Monro (*primus*) states, "the first and most general prejudice against inoculation is its being deemed a

God, and originates in man; but the cow-pox is produced by presumptuous, impious man. The former, heaven ordained; the latter is perhaps a daring and profane violation of our holy religion." And he subsequently proposed, "whether vaccination be agreeable to the will and ordinances of God, as a question worthy of the consideration of the contemplative and learned ministers of the gospel of Jesus Christ; and whether it be impious and profane, thus to wrest out of the hands of the Almighty the divine dispensation of Providence!"¹ "The projects of these vaccinators seem (it was affirmed) to bid bold defiance to heaven itself, even to the will of God."² "Providence (reasoned another author) never intended that the vaccine disease should affect the human race, else why had it not, before this time, visited the inhabitants of the globe. The law of God (he continues) prohibits the practice; the law of man and the law of nature loudly exclaim against it."³

In short, vaccination was opposed and denounced on a variety of grounds. It was alleged to be occasionally fatal in its consequences; to be liable to excite various diseased actions and predispositions; to produce diseases new to the human constitution; to be "impious, unthinking, profane and irrational;" to be an innovation, neither "established on the basis of reason, nor supported by the foundation of truth."⁴ "The vaccine," exclaimed one enemy to cow-pox inoculation, "was the damndest thing ever proposed; he wished the inventors were all hanged, and he would give his vote for its being done."⁵ And strong pictures were held up to the public eye of the miseries it would infallibly lead to, in case of the recurrence of epidemic small-pox. "In many families,"⁶ writes an author whom I have already quoted, "there will be none to attend the sick; nurses will quit their patients for their own safety; and servants fly from their masters' houses to shun the pestilence. Then we shall experience an horrid scene of public and private calamity—brought on by a medical experiment; embraced without due consideration; extended by a rash transgression over the bounds of reason; and, after the fullest conviction of its inutility, obstinately continued, by the most degrading relapse of philosophy that ever disgraced the civilized world."

tempting of God's providence, and therefore a heinous crime." P. 5. "Clergymen (observes Dr Baron, in his *Life of Jenner*, vol. i. p. 231) preached from their pulpits in this style of argument, if so it might be called. Some went so far as to pronounce inoculation an invention of Satan himself, and its abettors were charged with sorcery and atheism. These things (he adds) would scarcely obtain credence were it not that similar arguments and assertions have been employed against vaccination itself."

¹ Rowley's pamphlet, p. 9.

² Vaccine Contest, p. 84.

³ Preface to the Second edition of Dr Squirrell's *Observations*, p. iv.

⁴ Blair's *Essay*, p. 83; and Lipscomb's *Pamphlet*, p. 28.

⁵ Moore's *Reply to the Anti-Vaccinists*, 1806. P. 14.

⁶ Preface to "*Treatise on Lues Bovilla*," p. 23.

Such were the chief forms of opposition and argument that were stoutly and vigorously urged against vaccination during the earlier years of its progress. They are the same by which many of the happiest and greatest improvements in our profession have each in turn been assailed at their first promulgation. From time to time in the march of medicine and other allied sciences, some earnest and expanded mind conceives and elaborates a great and novel thought, destined in its practical application to ameliorate the condition and promote the happiness of mankind. But hitherto almost as often as the human intellect has been thus permitted to obtain a new light, or strike out a new discovery, human prejudices and passions have instantly sprung up to deny its truth, or doubt its utility, and thus its first advances are never welcomed as the approach of a friend to humanity and science, but contested and battled as if it were the attack of an enemy.¹ Practical medicine, in its past career, is full of instances illustrative of this remark. Witness the history of the immense and now almost forgotten difficulties accompanying the first introduction of mercury, antimony, and cinchona-bark, into medical practice; or the stern obstinacy with which the ligature of arteries after amputation was long, long rejected, and cauteries and caustics preferred; or the professional and religious prejudices which the propriety of saving human life by inducing premature labour has encountered up to within the last few years. Further, every proposed improvement seems to be met with the same invariable array of objections and arguments.

¹ This remark holds true for instance with regard to small-pox inoculation, &c. &c. Lord Wharnccliffe, in his edition of the letters and works of Lady Mary Wortley Montagu, after giving the history of her Ladyship's introducing the practice of small-pox inoculation into England from the East, observes:—"What an arduous, what a fearful, and, we may add, what a thankless enterprise it was, nobody is now in the least aware. Those who have heard her applauded for it ever since they were born, may naturally conclude, that when once the experiment had been made and had proved successful, she could have nothing to do but to sit down triumphant, and receive the thanks and blessings of her countrymen. But it was far otherwise. Lady Mary protested that in the four or five years immediately succeeding her arrival at home, she seldom passed a day without repenting of her patriotic undertaking; and she vowed that she never would have attempted it if she had foreseen the vexation, the persecution, and even the obloquy it brought upon her. The clamours raised against the practice, and of course against her, were beyond belief. The faculty all rose in arms to a man, foretelling failure and the most disastrous consequences; the clergy descanted from their pulpits on the impiety of thus seeking to take events out of the hand of Providence; the common people were taught to hoot at her as an unnatural mother who had risked the lives of her own children.

"We now read in grave medical biography, that the discovery was instantly hailed, and the method adopted, by the principal members of that profession. Very likely they left this recorded; for whenever an invention or a project—and the same may be said of persons—has made its way so well by itself as to establish a certain reputation, most people are sure to find out that they always patronised it from the beginning; and a happy gift of forgetfulness enables many to believe their own assertion." *Letters and Works of Lady Mary Wortley Montagu. Edited by her great grandson Lord Wharnccliffe. Vol. i. p. 55.*

The discovery may be new, but the grounds of opposition to it are not new,—they are merely the old forms of doubt, and difficulty, and prejudice, used on former occasions, recalled and reproduced anew. Thus not only in their leading principles and spirit, but in most even of their minute details, identically the same arguments that forty or fifty years ago were urged against the propriety and safety of vaccination, or a hundred years ago against small-pox inoculation,¹ have, within the last few months, been again invoked and used against the employment of etherization. Time has amply proved how futile and inapplicable these arguments were as directed against vaccination. In truth, those forms of reasoning and opposition against the employment of cow-pox that, some forty or fifty years ago, appeared to many members of the profession to be perfectly conclusive and insuperable, now read and appear to us at the present day as in the highest degree illogical and absurd. History has been compared to a mirror in which we may study the faults of our predecessors, with a view of avoiding the same errors ourselves. The history of cow-pox is certainly calculated to teach us this one lesson, that in relation to the truth of any novel doctrine or practice, such as vaccination or etherization, adverse opinions and prejudgments are—however strongly entertained, or however strongly expressed—not in themselves adequate, as some, at the present time, would seem to believe, to decide the whole matter in dispute, either in one direction or another.² And the moral is obvious,—that while minds anxious to promote new and probable inquiries should not be intimidated and deterred from their pursuit by such prejudgments on the part of others, those who are, on the contrary, anxious to suppress them, should not venture to base their opposition upon mere impressions and mere opinions only. The ultimate decision upon such investigations ever comes to be founded,

¹ “The very same objections (writes Mr Moore in 1805), accompanied with the same species of proof, were adduced against it (small-pox inoculation), as are now brought against vaccination.”—See his reply to the Anti-Vaccinists, p. 70.

² I have been told, that any comparison between the progress of vaccination and etherization cannot be true in one respect, that vaccination was at once and generally received. The quotations in the text show the contrary; and many of the strongest adverse opinions which I have quoted were published in 1805-6, seven or eight years after Dr Jenner published his first admirable essay on the subject in 1798. After Dr Jenner published this essay, he went to London, and resided there for nearly three months; but during this time, “with all his efforts and those of his friends, he was unable in the metropolis to procure one person on whom he could exhibit the vaccine disease.” Not one individual would submit to it. After Jenner left London, Mr Cline made the first experiment tried in London with cow-pox, by inoculating it as a counter-irritant on the hip of a patient affected with morbus coxarius.”—(*Baron's Life of Jenner*, Vol. i. p. 150). Jenner first tried artificial vaccination in May 1796. In March 1800, Mr (afterwards Sir Matthew) Tierney wrote him from Edinburgh, where he was then a student, stating that “Dr Gregory, the professor of physic, knew very little about it, and, of course, did not encourage it. Mr Anderson, a surgeon at Leith, is the only person here who has tried it.”—(*Ib.* p. 376). As late as 1805, the popular opinion in London was much shaken

not upon preconceived beliefs or hasty deductions, but upon the careful examination and evidence of a sufficient body of accurate and well ascertained facts. During the last six months, etherization has been used to a considerable extent in British surgery; and at the present time, we are perhaps in a condition to turn and look back upon this past experience with it, in order to endeavour to form, from the existing facts and cases, a proper judgment upon its merits or demerits, and especially in order, if possible, to obtain some satisfactory light upon that all important question in relation to its employment, viz. whether its adoption increases or diminishes the usual mortality consequent upon surgical operations. It was with this view that the present communication was undertaken.

But, in the first place, and before engaging with this more difficult part of the inquiry, let me briefly adduce the positive evidence which we possess of the effect of etherization in cancelling and abolishing the sufferings attendant upon surgical operations, and the best means of effecting this desirable object, the *a priori* objections to it, &c.

PROOF OF ETHERIZATION POSSESSING THE POWER OF ANNULLING THE PAIN ATTENDANT UPON SURGICAL OPERATIONS.

Abundant evidence to convince any unprejudiced mind upon this subject has already been accumulated in our periodical literature; and no one who has felt in his own person, or witnessed in others, the proper and perfect effects of etherization, can reasonably entertain any scepticism upon this point. In regard to it, I shall content myself with adducing the evidence which Dr Forbes some time ago offered as the result of his inquiries on the subject, in the London hospitals. "For the purpose," he says, "of obtaining information on all the points of this most interesting subject, we personally questioned all the patients in the

with regard to the propriety and safety of vaccination. "The influence," says Mr Blair, "of false rumours and distorted facts operated so strongly in the district of Bloomsbury and St Giles, as to preclude even a single person from applying for vaccination at that dispensary."—(*Pamphlet*, p. vi.) Dr Moseley boasts (p. 13 of his Treatise, second edition), that at that date, 1805, the middle and inferior classes in London had "renounced the delusion," and would not "expose their children to cow-pox." Instances of more marked popular hostility against it were not wanting in the early history of cow-pox. Mr Gooch states, that the first people he vaccinated in Hadleigh, Suffolk, "were absolutely pelted and drove into their houses if they appeared out."—(*Baron's Life*, Vol i. p. 382). In the town of St—n, Kincardineshire, a surgeon was lately used in a similar manner, for venturing to etherize a patient for extraction of a tooth. But still etherization has made more progress in months than vaccination effected in years; and already within a few short months a knowledge of it has spread over almost the civilized world. Within these few days, I received a letter of consultation from a lady, asking some directions for the use of etherization at her approaching confinement, in October next. The letter was from the Far West, and dated "Mount Morris, Illinois, United States."

London hospitals, who, at the period of our visits, still remained in the wards after the ether operations. They were in all *fifty-four*, and the great majority had been the subjects of capital operations. They were unanimous in their expressions of delight and gratitude at having been relieved from their diseases without suffering. In listening to their reports, it was not always easy to remain unmoved under the influence of the conceptions thereby communicated, of the astonishing contrast between the actual physical condition of the mangled body in its apparent tortures on the operating table of a crowded theatre, and the really happy mental state of the patient at the time. The old story of the magician in the Arabian Tales seemed more than realized before us, the ether being like the tub of water, one moment's dip of the head into which produced a life-long vision in the dreamer's mind."¹

CONDITIONS FOR INSURING SUCCESSFUL ETHERIZATION.

To produce, however, the full and perfect effects of etherization, it is necessary to conduct the process in conformity with certain conditions. These conditions it is not the object of the present communication to consider. But I will take the liberty of mentioning two or three leading points, the importance of which, in relation to the attainment of complete success, has become strongly impressed upon my own mind by a somewhat extensive experience in etherization during the last few months.

First, The patient ought to be left, as far as possible, in a state of absolute quietude and freedom from mental excitement, both during the induction of etherization, and during his recovery from it.² All talking and all questioning should be strictly prohibited. In this way any tendency to excitement is eschewed, and the proper effect of the ether inhalation more speedily and certainly induced. And,

¹ See Dr Forbes' very able article on Etherization, in the British and Foreign Review for April 1847, p. 554.

² The area of an hospital operation theatre is hence, perhaps, not the most favourable place for securing all the advantages of etherization, or rather for shunning all its disadvantages. Lately, in a case in which Professor Miller performed partial amputation of the foot, in the Royal Infirmary, I etherized the boy who was the subject of it, in his bed in the wards. He was carried in this state up stairs to the operating theatre—the amputation performed—and the patient brought back again to his bed before he was allowed to awake. He was thus, at one and at the same time, entirely spared the moral shock and pain of being transported and carried in before a formidable collection of surgeons and students, and saved from the physical sufferings attendant upon the amputation itself; for he was perfectly unconscious of aught that had occurred, and, when he awoke, he was not aware that he had been operated upon, or had even left his bed. While being carried from the ward to the operating room, the sponge with which he was etherized was kept fixed over his face with a couple of common elastic letter bands. In our surgical hospitals, if a ward immediately adjoining the operating theatre were set aside for operation cases, it would in this way facilitate the process of etherization, and ensure more certain and perfect results from it.

secondly, with the same view, the primary stage of exhilaration should be entirely avoided, or at least reduced to the shortest possible limit, by impregnating the respired air as fully with the ether vapour as the patient can bear, and by allowing it to pass into the lungs both by the mouth and nostrils, so as rapidly and at once to superinduce its complete and anæsthetic effect. Latterly, I have found that for surgical purposes, and when it is not necessary to keep up the etherization above five or ten minutes, by far the best and most perfect inhaler is formed by a large sponge of the common hollow conical shape, perforated artificially with a pretty large aperture at the apex, and placed over the face like a mask, so as to include both the mouth and nose in its concave base. At first, it should be held at a little distance from the face, and afterwards gradually advanced to it, in order to avoid exhibiting the vapour in too powerful and irritating a form for the first few inspirations. Its interior should, immediately before using it, be fully and freely saturated with ether—a very common but certainly a very unpardonable error being to exhibit an imperfect and exciting, instead of a perfect and narcotizing dose of the vapour.¹ Many of the alleged failures and misadventures are doubtless entirely attributable to the neglect of this simple rule;—not the principle of etherization, but the mode of putting it in practice being altogether to blame.—But *thirdly*, whatever means or mode of etherization is adopted, the most important of the conditions required for procuring a satisfactory and successful result from its employment in surgery, consists in obstinately determining to avoid the commencement of the operation itself, and never venturing to apply the knife until the patient is under the full influence of the ether-vapour, and thoroughly and indubitably soporized by it.

In the operating theatres of the Paris hospitals, the most triumphant successes, in the original trials with ether, were obtained by M. Velpeau, who differed from his fellows in one all-important particular only, namely, in the forbearance with which he waited for the complete insensibility of his patients before venturing to take his operating knife into his hand. Few men have had more practice in etherizing than Dr Snow of London, who has been in the habit, for some time past, of thus assisting with their operations some of the first surgeons in the metropolis. Speaking of the stage of etherization required, he draws a proper distinction between two degrees of this state,

¹ When a prolonged effect is required, as in midwifery cases, an instrument is necessary—were it for no other reason than the saving of ether, and the prevention of its diffusion through the apartment. Within the last few days I have seen a pamphlet dated Boston, May 30, 1847, in which it is stated that for three months previously, all apparatus had been laid aside, and the sponge alone used for etherization, by Dr Morton of that city—the gentleman to whom, I believe, the profession and mankind are really and truly indebted for first reducing into practice the production of insensibility, by ether inhalation, with the object of annihilating pain in surgical operations.—See *Some Account of the Letheon*, by Edward Warren, p. 87.

the first, and slighter, in which the patient moves, and winces, and seems to feel pain at the moment, but without afterwards remembering it; and the second and deeper state in which there is no evidence whatever of pain being felt, far less remembered. And he adds, "In full four-fifths of the cases in which he had administered the ether, there was not the least flinch or groan during the cutting by the surgeon's knife. He considered cases of this kind the only truly successful ones, and believed that with proper care every case might be of this nature. When the patient exhibited signs of pain, although he might have no knowledge or recollection of it afterwards, the ether was only partially successful."¹ As a proof that such deep states of etherization are not accompanied with danger, I may mention here, though in the way of anticipation, that out of 39 surgical operations, "nearly all serious ones," in which Dr Snow has exhibited ether at St George's hospital, 2 only of the patients died, namely, 2 on whom amputation of the thigh was performed, after they had been previously reduced to an "extremely weak and emaciated"² state. Now, 2 deaths in 39 hospital operations, or 1 in 18, would certainly be regarded as a very satisfactory and favourable result under almost any circumstances, and either with or without ether. Dr Peacock, in his official reports of the Edinburgh Infirmary for 1842 and 1843, has published³ two tables showing the results of the "principal operations" in that Institution, from amputation and lithotomy down to the operation for harelip. The tables include 150 cases in all; and 32 of the 150 patients operated upon died, or about 1 in every 5. Excluding 57 cases of "extirpation of tumors in various parts," 32 in the 93 individuals on whom other operations were performed, or nearly 1 in every 3, died.

THE PREVENTION OF PAIN IN SURGICAL OPERATIONS ALLEGED TO BE UNNECESSARY AND IMPROPER.

There is one strange episode which has been frequently reacted in the early history of several of those practical improvements, which we all now proudly refer to, and rejoice over, as among the greatest and most undoubted advances made in the past march of medical science. It is this. Some striking discovery happens to be made, or some great improvement suggested. Its worth and importance, betimes, are acknowledged by different members of the profession. Others, however, full of doubts and difficulties, conjure up to their own minds, and to the minds of their brethren, all the usual forms of

¹ Medical Gazette, February 26, 1847, and Braithwaite's Retrospect of Medicine, Vol. xv. 1847, p. 409.

² Lancet for May 29, 1847, p. 553.

³ Statistical Tables of the Royal Infirmary of Edinburgh for 1842, p. xix., and for 1843 p. xviii. In the reports of other years the operations are unfortunately not tabulated on the same plan, and do not show the mortality dependent upon them.

objection to the new view that has been propounded; and the more ardent among these opponents always fix, and insist among other points, upon this special and singular ground of objection that—the disease or evil proposed to be remedied is comparatively and truly less obnoxious in its character than was in general previously imagined,—that its removal is, consequently, not a matter of much moment,—and that the new and artificial measure now suggested for that purpose is, probably, in its action and effects on mankind, really more dangerous and deleterious than the very state or malady which it was proposed to remove by it.

Take, for instance, as an example of the remark, the strong opposition offered first to variolous inoculation, and afterwards to vaccine inoculation.

It has been repeatedly calculated in regard to Europe, that before the introduction of variolous inoculation, small-pox regularly decimated the human race, one out of every ten deaths that occurred being a death from small-pox; and further, the disease was reputed fatal to at least one out of every 6 or 8 individuals attacked by it. Inoculation was introduced, and its effects were so marked that of those who submitted to the disease in this form, about one only in every 3 or 400 seems to have perished.¹ Vaccination was discovered, and this preventive affection was found to prove rarely or never *per se* dangerous, or fatal to life. But science in substituting—first, inoculated for natural small-pox, and secondly, cow-pox for inoculated small-pox, was anxiously combatted by the double argument, that the old evil was not so great an evil as it was usually represented to be, and that perhaps it was in reality safer and better than the new measure proposed as a substitute for it.

Thus, a hundred and twenty years ago, among the various pamphlets published with the view of contesting and opposing the propriety of the newly introduced Variolous Inoculation, Mr Howgrave, whose Essay on the subject is one of the best of the period, maintains that “the small-pox in the natural way very rarely affects life where the habit of body and constitution are good;”² “that the hazard of inoculation is not only considerably less, but considerably greater than that of the natural small-pox;”³ that this “new and strange method,”⁴ “more frequently produces accidents than the natural way;”⁵ that it is “not only unsafe, but uncertain,”⁶ “and

¹ In reference to the mortality of natural and of inoculated small-pox, Dr Gregory, Physician to the Small-pox Hospital of London, observes, “It is commonly stated that one-fourth of those who are attacked by small-pox in the natural way perish.”—*Cyclopædia of Medicine*, vol. iii., p. 742. And “The average number of deaths (from inoculated small-pox) at the Inoculation hospital, was only three in a thousand (or about 1 in 330.) In the wards appropriated to the casual disease the deaths were, and continue to be, three in ten,” or more than 1 in 4.—*Ibid.* vol. iv. p. 749.

² Reasons against the Inoculation of the Small-pox. London, 1724. P. 67.

³ *Ibid* p. 72.

⁴ P. 8.

⁵ P. 61.

⁶ P. 5.

that," all persons who will suffer reason to determine their opinion, must be convinced that their (the inoculation) method has no degree of safety in it."¹ Hence we can scarcely wonder when he "affirms that the best method is to trust Providence, and not allow mortal man to inflict diseases upon us at his pleasure"²—Other cotemporary writers against small-pox inoculation, maintained upon the same grounds the same views of the impropriety of the practice.³

Again, forty or fifty years ago, in opposition to the proposal of Dr Jenner to introduce vaccination, or, in other words, to substitute the inoculation of cow-pox for the inoculation of small-pox, the same objections to the practice were reproduced. Dr Moseley stated,⁴ for example, several years after vaccination was introduced, that "the mischiefs of the natural small-pox, we all know, are great enough, but they may be prevented by inoculation;" that, "instead of 1 in 300 dying of the small-pox from inoculation, there need not be one in 300,000;" "that the inoculated cow-pox is not a milder or safer disease than the inoculated small-pox;" and that "the necessity of an immediate restoration of small-pox inoculation must strike every person interested in the welfare of society, and the happiness of his fellow-creatures." Cow-pox (averred Mr Drew and Mr Forrester) was a "*far more severe disease than small-pox.*"⁵

"Out of many thousands, nay millions (so wrote Dr Rowley in 1805),⁶ it has been fully proved scarcely any one died from small-pox inoculation;" "it was safe, mild, and certain; therefore, cow-pox inoculation as a substitute was absolutely unnecessary;" besides, the substitute itself "stands condemned by the experience of veterans in the profession;" "disagreeable events (eruptions, abscesses, gangrenes, chronic diseases, &c.) have, in great numbers followed, and can be traced to arise from cow-pox inoculation," while small-pox inoculation was free from them; and "fifty-nine have died, and a great number of victims become diseased for life," in consequence of vaccination; in fact "the senses," says Dr Rowley, "are appalled, and the pen is tired of recording its dreadful disasters." (P. 61.)⁷

But, in our own day, exactly the same line of argumentation that was thus zealously directed against the adoption of artificial variolation and vaccination, at *their* first introduction into practice, is now

¹ Reasons against the Inoculation of Small-pox. p. 28. ² Ibid, p. 63.

³ See, for instance, the Rev. Mr Delafaye's Sermon on "Inoculation, an In-defensible Practice," p. 22, 23; or "A Letter in Reply," by N. Bolaine, Surgeon, p. 17. Sparham's "Reasons against the Practice of Inoculating the Small-pox, p. 27. Dr Wagstaffe's Letter to Dr Friend, shewing the danger and uncertainty of Inoculating the Small-pox, pp. 48, 49, &c.

⁴ Treatise on the Lues Bovilla, 2d Ed. 1805, p. xv. xx. xxii. 110. &c.

⁵ See Lipscomb's Essay, p. 7, and Lipscomb's own similar opinion, p. 39.

⁶ On Cow-pox Inoculation, pp. 4, 31, 100, 128, &c.

⁷ See the works also of other anti-vaccinists for the same line of argument. Thus, observes Dr Squirrell, the small-pox inoculation, "whatever impediment it might have met with at its introduction, owing, at that

as zealously directed against the adoption of etherization. For precisely in the same way some minds, averse to the employment of ether inhalation, anxiously argue that there is not only no call for its employment in surgery, but that its employment, and the result which it produces—namely, immunity from the physical sufferings inflicted by the knife of the surgeon—is probably a direct and positive evil.

The common opinion of mankind and of the profession in regard to the pain attendant upon surgical operations, seems, till of late, to have been unanimous and unchallenged. The human agony and torture following the surgeon's knife, have hitherto been borne with and submitted to, merely because, while they seemed absolutely necessary for the preservation of health and life, they were considered at the same time absolutely unavoidable. It is true that differences in the mental and physical constitutions of different men, enable them to encounter the surgical mangling and mutilation of their limbs and bodies, with different degrees of equanimity and hardihood. And under special morbid states of the nervous system,—in the way both of great excitement and great overstraining and collapse,—flesh and bone have sometimes been extensively cut and divided without the common accompanying feelings, and common manifestations of acute suffering. But the everyday experience of mankind in every age, shows how greatly and truly the reverse of this is usually the fact. And all past human testimony on this point goes fully to corroborate the truth of the sentiment which England's great epic poet puts into the mouth of the wounded Nisroch, on the first occasion in which wounds were ever inflicted, and living beings first "knew pain :"—

" But Pain is perfect misery, the worst
Of evils, and, excessive, overturns
All patience."

Nor have we any proper test, either of the fortitude with which it will be borne, or of the amount of pain actually endured in individual cases. For whilst the degree of outward manifestation of suffer-

time, to a want of experience in the practitioners, is now a mild and harmless disease," and hence "there is no necessity to forsake its practice," for that of vaccination, with "all its difficulties, ambiguities, and malignant effects." "The cow-pox (he argues more at length) produces malignant effects, vitiates the blood, and other juices, and is tedious as well as difficult to cure; the small-pox inoculation produces no ill consequences whatever. The cow-pox produces very ill health in children; the small-pox inoculation improves the health and constitution, and carries off many complaints, which were very uncomfortable both to the parents and children. The cow-pox matter is taken from an animal diseased, and is of a specific scrofulous kind, as is proved by its effects; the small-pox matter is taken from a healthy subject, and produces no disease whatever, but the one for which it was intended."—*Observations on the Cow-pox*, pp. 24, 49, 55, &c.

ing, shown by the patient on the rack of the operating table, affords no perfect evidence of his actual feelings (the greatest torture being incapable in some of contorting a muscle, or eliciting a groan, and the slightest scratch forcing screams and cries from others); neither, on the other hand, is the degree of equanimity and endurance with which the agony of a surgical operation is submitted to, any true and direct criterion of the natural moral strength and mental courage of the patient. Sometimes the sailor and soldier, who would not feel one moment's dread in facing, under the most desperate circumstances, the sword and fire of the enemy, will quail before the very thought of having his living flesh deliberately cut and mutilated by the cold steel of the surgeon.¹ And thus, the mere fear and horror of encountering the pain and agony of a surgical operation, will sometimes, by unnerving the strongest mind and boldest heart, bring on, as a consequence, such a depressed state of the system, as certainly by no means places the patient in a state favourable for securing a happy and successful result. "An extraordinary excitement of mind," observes Mr Travers, "such as is produced by dread, or by the screwing up of the system for the endurance of painful operations, when it is already much depressed and enervated by continued suffering, or apprehension of it, sometimes proves suddenly fatal."²

In the greater operations of surgery, the employment of etherization is not only, thus far, a great blessing to the patient, but it is a great boon also to the practitioner. It is a benefit to the operator, as well as to those operated upon. For, whilst it relieves the one from all the feelings of agonizing pain, it relieves the other from the feel-

¹ The following illustrative anecdote refers to Lord D——, assuredly one of the bravest admirals in the English service, and a man of all others whom his country has for long looked upon as the very personification of valour and courage. The injury alluded to was received in cutting off the Spanish frigate A——, one of the most daring feats attempted in modern warfare. I extract the account from an article on Etherization in the "North British Review," excellent in matter, in manner, and in feeling, and written by one of the most distinguished surgical practitioners and authors of the present day. "We remember," says the reviewer, "the case of a gallant admiral—one of the bravest hearts that ever beat, in a service where men of every grade are, to a proverb, dauntless—who, in the opening of his distinguished career, had been engaged in cutting out an enemy's frigate. From the gun-boat, he climbed up the ship's steep side, and, foremost of his crew, had reached the bulwarks, when, receiving a stunning blow, he fell backwards into his boat again, striking his back violently on the tholpin. Many years afterwards, a tumour had grown on the injured part, and at length the admiral—grey, and bent in years—found it advisable that this growth should be removed. The man that never feared death in its most appalling form while in the discharge of duty, now shrank from the surgeon's knife; the removal, contemplated with a feeling almost akin to fear, was long deferred; and at length, half stupefied by opium though he was, a most unsteady patient did he prove during the operation."—*North British Review* for May 1847, p. 169.

² Travers on Constitutional Irritation, Vol. I. p. 22.

ings of inflicting such pain upon a fellow-creature. Humanity, in the fullest sense of the term, is the great object of the healing art, and the aim alike of the surgeon as of the physician. Hitherto the professional duty of the surgeon has compelled him to inflict present suffering upon his patients, with a prospective view to their own ultimate benefit and advantage. And surely there is no one, however much inured to the sight and shriek of pain, who would not rejoice to be able to obtain these advantages for his patients, without compelling them to pass through so severe an ordeal as the tortures of the operating table. If, as some few operators themselves have indiscreetly boasted, their hearts have become so hardened by education and custom as not to be affected by the sufferings of those submitted to their knives, it is still pleasing and refreshing to know that this change in the human feelings, and this artificial violation of one of the first laws in human nature—namely, sympathy with the sufferings of others—is by no means necessary to make a man a perfect surgeon.

Perhaps no surgeon ever operated, either more frequently or more successfully, than the celebrated Cheselden. In St Thomas' Hospital alone, and exclusively of his practice elsewhere, he performed lithotomy upwards of two hundred times, and with results that have never yet been surpassed by any other operator. But he adds, in language bearing all the simple impressiveness of truth, "If I have any reputation in this way, I have earned it dearly; for no one ever endured more anxiety and sickness before an operation; yet, from the time I began to operate, all uneasiness ceased; and, if I have had better success than some others, I do not impute it to more knowledge, but to the happiness of a mind that was never ruffled or disconcerted, and a hand that never trembled during any operation."¹ It was under this great master that John Hunter received his first lessons in surgery; and the biographer of Hunter tells us, that (to quote his words) "Cheselden's manners were exceedingly kind and gentle, and, notwithstanding the extensive practice he had enjoyed, he always, before an operation, *felt sick at the thoughts of the pain he was about to inflict*; though, during its performance, his coolness and presence of mind never forsook him. Such feelings," adds Mr Ottley, "are, in a less marked degree perhaps, far more commonly experienced than is generally supposed, by the very best surgeons, previously to undertaking operations of importance."² And, no doubt, it is the desire to diminish the actual amount of pain endured by patients, by curtailing the actual duration of it, that has led many of our best surgeons, with praiseworthy earnestness, to attempt to invent new and more rapid modes for performing particular operations,—a field in which no small degree of success has consequently been met with.

¹ Cheselden's *Anatomy of the Human Body*, 5th edition, p. 333.

² Ottley's *Life of John Hunter*, in Mr Palmer's edition of his works, Vol. I. p. 9.

A new era, however, arrives in chirurgical science, and a measure is, at last, brought to light, through the influence of which surgeons may perform operations, and patients submit to them, even when of a prolonged nature, without the necessity of pain. It is found that the excruciating tortures, and writhings, and shrieks of patients on the operating table, may be saved; and yet the required operations be as well and perfectly executed as before. Scarcely, however, is this glad and glorious discovery announced and acted upon, than another new, and, if possible, still stranger discovery, is broached and anxiously promulgated; namely, that in cutting the living flesh of man, the surgeon's knife does not, after all, produce any very remarkable, or very important amount of pain, and that immunity from this pain during operations would be, perhaps, an evil rather than a good to humanity,—a calamity rather than a blessing.

At a meeting of the South London Medical Society, held in April last, Dr Gull read a paper on the injurious effects of ether inhalation, and ended his communication with queries as to the "*desirability* of removing pain," &c.¹ Mr Bransby Cooper, surgeon to Guy's Hospital, afterwards affirmed it as his opinion, "that pain was a premonitory condition, no doubt fitting parts, the subject of lesion, to reparatory action, and, therefore, he (Mr Cooper) should feel averse to the prevention of it."² "Pain," argues Mr Nunn, surgeon to the Colchester and Essex Hospital, in some observations against ether inhalation,—"*pain [toothache?] is, doubtless, our great safeguard under ordinary circumstances; but for it we should be hourly falling into danger; and I am (he continues) inclined to believe that pain should be considered as a healthy indication, and as an essential concomitant with surgical operations, and that it is amply compensated by the effects it produces on the system, as the natural incentive to reparative action.*"³ Arguing in a similar but still more bitter strain against etherization, Dr Pickford affirms, that "*pain during operations is, in the majority of cases, even desirable; and its prevention or annihilation is, for the most part, hazardous to the patient.*"⁴ Upon one of the first communications being given in to the French Academy of Sciences upon etherization, M. Magendie, the distinguished physiologist, maintained⁵ that "*pain has always its usefulness;*" he doubted if there was a true advantage "*in suppressing pain, by rendering patients insensible, during an operation;*" and argued, that "*it was a trivial matter to suffer (*c'est peu de chose de souffrir*); and a discovery*

¹ See Report of the Meeting in the London Medical Gazette for April 30, 1847, p. 777.

² Ibid.

³ London Medical Gazette for March 5, p. 415.

⁴ On the Injurious Effects of the Inhalation of Ether, in the Edinburgh Medical and Surgical Journal for July 1847, p. 258.

⁵ Gazette Medicale de Paris, 6th Feb. 1847, pp. 112, 113.

whose object was to prevent pain was of a slight (*mediocre*) interest only." When the effects of ether were discussed before the Medico-Chirurgical Society of Edinburgh, at one of their meetings in February, Professor Syme stated, that "he did not attach much importance to causing extinction of pain during operations;"¹ and more lately, he has published the opinion, that on "many" of those occasions in which he had seen the severest surgical operations performed, and "under the greatest liberties ever taken with the human frame, such as the removal of large tumours, amputations, and lithotomy," the attendant sufferings of the patient were "nowise unbearable."²

It would be as idle as it would be useless to confute by mere argument the preceding allegations, in regard to the supposed necessity and advantages of pain in surgical operations. For, in fact, the whole question amounts to this:—Mankind are perfectly agreed, that the cutting and mutilation of the living human body *is* painful, however loudly surgeons may preach to the contrary; and medical men have hitherto assented, without one contradictory voice to the self-evident aphorism of Galen, that pain is useless to the pained (*dolor dolentibus inutilis est*). If we find then, as we do now, a few men entertaining and expressing opinions on these points so very different from the general ideas and general experience of mankind, these opinions can scarcely be looked upon as aught else than indications of a strange degree of eccentricity of thought upon one special subject. And if the same line of thought that they apply to pain were extended (as it should be if true in principle) to other diseases or effects of disease, the untenable and irrational character of it would lead to conclusions that might perhaps surprise and startle even those minds that at present employ it against etherization. For if physical pain and agony be a blessing and benefit, and not an evil and a calamity to be eschewed, then all other morbid symptoms and affections should equally, upon the very same grounds, be included under the same category,—their presence endured and courted, and their removal forbidden and avoided; diseases and death are parts of the great economy and general police of nature, and the labours of the physician and surgeon to counteract their agency, should be denounced and decried as unnecessary and improper.

Let us view the subject, however, calmly, and as a question in pathology. And that pain is, *per se*, and especially when in excess,

¹ Monthly Journal of Medical Science for April, p. 784.

² Ibid. for August, p. 75. Mr Syme seems to believe that the very removal of a pain or "irritation of extraordinary intensity" may even produce death. In commenting upon the case of a patient, who had suffered "for nearly three years all the symptoms of stone in a degree of extraordinary severity," and who died four days after lithotomy was performed, Mr Syme observes, "the fatal result in this case may, I think, with most probability be ascribed to the effect of suddenly removing a source of extreme irritation in a very irritable system." On dissection, the lungs were found "gorged with mucus," "white and firm."—*Edinburgh Med. and Surg. Journal*, vol. xxxv. p. 248.

a condition which is not only trying and difficult for any exertion of human fortitude to bear, but at the same time directly injurious and damaging in its action upon the constitution, and sometimes very fatal in its effects, is a practical truth that many of our best surgeons and soundest pathologists have long amply acknowledged and attested, and that, too, without any prejudgment in regard to it, or, at all events, without any view to such an extraordinary proposition as the now alleged impolicy and impropriety of abolishing it.

Speaking of the pains of wounds, the great father of French surgery, Ambrose Paré, pithily tells us, that it "ought to be assuaged; because," adds he, "nothing so much dejects the powers of the patient (*prosterne et abatte les vertus du malade*)."¹ "Mere pain," observed the late distinguished Dr Gooch, "can destroy life."² "Pain," according to Mr Travers, "when amounting to a certain degree of intensity and duration, is of itself destructive."³ "Pain in excess," he again remarks, "exhausts the principle of life; so that either its continuance without intermission, or the superaddition of the slightest shock subsequent to its endurance for a certain period, is fatal. In operations protracted by unforeseen difficulties, as in cases of lithotomy, in which the stone is of such magnitude as to require crushing, the patient has begun to die upon the table. The same happens in parturition, &c., protracted by mechanical impediments, &c."⁴ Again, "Bodily exhaustion from continued pain, spasm, and other causes, not unfrequently proves suddenly fatal. I have often observed a sudden deliquium at the conclusion of an operation for recent injury, arising evidently from the exhaustion of the nervous system in the effort to support it."⁵ "Great sensibility or excessive pain attendant on an injury (or operation) has," remarks Professor Burns, "two effects. First, it exhausts both the system and the part; and, secondly, it acts as an exciting cause of inflammation, a disease apt enough to take place from the injury itself."⁶

¹ Paré's Works, Johnston's Translation, p. 329.

² Merriman's Synopsis of Midwifery, 1826, p. 239.

³ An Inquiry concerning that disturbed state of the Vital Functions, usually denominated Constitutional Irritation, 1826, p. 65.

⁴ Travers on Constitutional Irritation, Vol. I. p. 76. The statistical data published by Dr Collins in his excellent Report of the Dublin Lying-in Hospital, proves how true Mr Travers' remark holds as a general principle with regard to the effect of pain in protracted parturition. In fact, the maternal mortality attendant upon parturition, regularly increases in a ratio progressive with the increased duration of the woman's sufferings. Thus (according to calculations which I have made from Dr Collins' data), while in the women whose sufferings were terminated within two hours, only one in about 320 of the mothers died; where the labour varied in duration from two to six hours, one in 145 died; in those in whom it continued from seven to twelve hours, one in 80 died; when it endured from twelve to thirty-six hours, one in 23 died; and out of those whose sufferings were prolonged beyond thirty-six hours, one in every 6 perished.

⁵ Travers, p. 24.

⁶ The Principles of Surgery. By John Burns, M.D. Vol. I. p. 502.

"We have many facts," observes Professor Alison, "to prove that various violent and overpowering sensations, intense pain, &c., when acting in the utmost intensity, affect the circulating system just as a concussion does, and sometimes with fatal effect,—especially when they take place in a state of unusual weakness or exhaustion."¹ Dr Ranking has lately published a striking instance of the fatally depressing effects of extreme pain. It occurred in a case in which a ligature was applied for the cure of an erectile tumour of the breast. "The patient," he relates, "a healthy female, bore the initiatory steps of the operation without a murmur, without failure of pulse, and without change of countenance. The instant the ligature was tightened, which it was with the full force of two surgeons, she gave a yell of agony, the pulse became imperceptible, the countenance became ghastly pale, and in eighteen hours she was a corpse."²

But it is, I believe, needless to accumulate superfluous proof on a point on which the observations and feelings of the profession and of mankind are sufficiently agreed; namely, that bodily pain, particularly such excess of pain as, with all its concomitant fears and sickening horrors, accompanies the larger operations in surgery, is, with very few, if indeed any exceptions, morally and physically a mighty and unqualified evil. And, surely, any means by which its abolition could possibly be accomplished, with perfect security and safety, deserves to be joyfully and gratefully welcomed by medical science, as one of the most inestimable boons which man could confer upon his suffering fellow-mortals. Few now deny, and, no doubt, ere-long fewer and fewer will venture to doubt, that with a generality as certain and constant, or indeed more so, than we see exemplified in the action of most other therapeutic agents upon the human constitution, etherization *does* possess the desired and entire effect of abolishing and annulling the pain following the scalpel and saw of the surgeon. But, as I have already stated, a question of the most important moment remains behind. Many surgeons, who cannot gainsay the indisputable effects of ether-inhalation in cancelling the pains of surgical operations, still ardently maintain that this admitted amount of present good, can only be purchased and obtained by the patient, at the hazard or certainty of a greater and disproportionate amount of future evil. In other words, they allege and maintain that the condition of etherization is one which, from its marked powerful effects upon the economy, must produce, in some cases, immediately dangerous results, which must affect the system so as to impede and interfere with the condition of wounds, and the recovery of patients from them, and hence that it will render opera-

¹ Outlines of Pathology and Practice of Medicine, p. 13. See also p. 203 and 317, on peritonitis proving directly fatal, "by virtue, probably, of the intense and peculiar sensation (of pain) it excites, acting as a powerful sedative on the heart." On pain as a cause of exhaustion of nervous power, see also Dr Holland's Medical Notes and Reflections, p. 618.

² Abstract of the Medical Sciences, by Dr Ranking. Vol. V. 1847, p. 383.

tions more perilous in their consequences, and more fatal in their ultimate results.

The correctness or incorrectness of this last feasible and assuredly most formidable objection to etherization in surgery, is a problem that no mere reasoning or mere opinion could ever certainly and satisfactorily solve. The evidence of simple prejudgment and argument could never perfectly settle it, however plausible and ingenious the grounds of the prejudgment and argument might be. It is one of those allegations, the accuracy or inaccuracy of which is a matter that can be fully and finally determined by one method only,—namely, by an appeal to the evidence of facts, and to the evidence of facts alone. For the purpose of assisting in the decision of this question, I have, through the great kindness of my professional brethren, collected the results of above two hundred amputations of the thigh, leg, arm, and fore-arm, performed within the last six months upon patients in an etherized state, in the civil hospitals of England, Scotland, Ireland, and France. The statistical analysis of these two hundred amputations with ether, and the comparison of *their* results with the results of various similar collections of the same amputations without ether, in the same and in other similar hospitals, will, I believe, enable us to arrive at some more definite ideas and deductions than we yet are in possession of, in regard to the debated question of the danger or safety of etherization in the operations of surgery. But want of space compels me to reserve this inquiry for the second part of this communication.

(*To be continued.*)

ARTICLE II.—*On the Structural Relation of Oil and Albumen in the Animal Economy, and on certain Physical Laws connected with the Origin and Development of Cells.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Lecturer on Pathology and the Practice of Physic, and Director of the Poly-Clinic at the Royal Dispensary, Edinburgh.

(*Read to the Royal Society of Edinburgh, April 19th, 1847.*)

ALL mysterious phenomena occurring in organized bodies are ascribed to that unknown principle, force, or action, which physiologists term vitality. As knowledge advances it is made apparent that many of these are the result of physical laws, and the history of discovery for some time has exhibited a continual struggle between those who contend for the vital, and such as maintain the physical nature of certain changes occurring in the organized world. As it is easier to influence the functions of vegetable and animal life through physical laws, which are readily understood, than through vital laws, which are quite unknown, any fact which can be snatched

from the domain of the latter, and added to that of the former, must be considered of the utmost practical importance. The object of the present paper is to show, that certain physical laws, hitherto very little taken into account, are intimately connected with the vital actions which preside over the origin and development of cell growth.

Recent chemical researches have shown that vegetables and animals are composed of similar proximate principles, which have been divided into nitrogenized and non-nitrogenized. It is probable that all the nitrogenized principles of food are subservient to the formation of albumen, and that the non-nitrogenized are to a great extent converted into fat or oil. The mode in which this is effected is as yet unknown, and constitutes no part of the present inquiry. The formation of the oily and albuminous principles, however, is essential; numerous physiological experiments having proved, that singly they cannot support life, and that their union is necessary to nutrition.

The chemist endeavours to explain this fact by pointing out, that albumen constitutes the basis of the tissues, and that oil furnishes the elements of respiration and of animal heat. According to him the animal body is a species of furnace, which is continually preyed upon by the combustion of the tissues, the different excretions being results of the process. This ingenious theory, however it may account for the tear and wear of the animal machine, in no way explains the origin and maintenance of cell growth, which anatomists and physiologists, by another series of researches equally exact, have shown to be essential to the vital functions. By not paying attention to structure, also, the chemist has overlooked the fact, that oil is not merely a material for combustion, but is as essential to the formation of the tissues as albumen; for, as we shall subsequently see, there is no elementary cell into which both oil and albumen do not enter as constituent parts.

The production of oil and albumen, as has lately been illustrated by Dr Alison,¹ is so far a vital process, that these principles can only be formed in organized bodies. Once produced, however, they exert a peculiar physical action on each other. In a paper read to the French Academy of Sciences in 1838, Dr Ascherson of Berlin pointed out, that the moment oil is brought into contact with albumen, the latter coagulates and forms a membrane. A drop of oil consequently cannot for a moment be surrounded with an albuminous fluid, without its being enclosed in a vesicular membrane or cell. This fact may be easily demonstrated in the following manner:—If we place a drop of oil and another of albumen on a slip of glass, and allow one to flow over the other, a pellicle will be observed to have formed. This examined microscopically, presents the appearance of a membrane, sometimes puckered and thrown into elegant

¹ Observations on the Principle of Vital Affinity. Trans. of the Royal Soc. of Edinburgh. Vol. XVI. Part iii.

folds. The formation of this membrane may be watched, and may be observed to arise where the two fluids come into contact, by the formation of exceedingly minute molecules, which rapidly become more numerous, approach each other and unite, forming a continuous surface, which at first is slightly granular, afterwards becomes smooth, and then acquires firmness, so that it may be mechanically broken up or thrown into folds. If now we unite the two globules by means of friction, we form an emulsion. If this be done dexterously, and the two substances are in proper proportions, the compound exactly resembles milk to the naked eye, and, when examined under high magnifying powers, is found, like the milk, to consist of minute globules floating in a transparent fluid. The membrane formerly seen has entirely disappeared, no debris or traces of its existence remain. Facts, to which I shall immediately allude, render it certain that the membrane is so broken up as to constitute a layer or envelope to the minute drops of oil, which in this way are transformed into cells composed of an albuminous wall and oily contents.

Whether the milk globules are simply loose globules of oil floating in the caseous fluid, or whether they are surrounded by an envelope, has been much disputed. That they do really possess an external membrane, however, seems to be proved by the following facts:—

1. They float in the fluid, and roll over each other without uniting.

2. Endosmosis and exosmosis may be produced in them, as in undoubted cellular structures, by the addition of fluids of different densities. Water causes them to swell out and enlarge, and syrup to shrivel up and lose their roundness of outline.

3. An excess of ether causes the globules to disappear, leaving behind a molecular mass.

4. Acetic acid dissolves the albuminous envelope, leaving the oil unaffected. After the addition of this agent, consequently many of the globules unite spontaneously. The same fact is shown in another mode by an experiment of Dumas, who says,—“If milk be mixed with pure ether, the two liquids soon separate into two layers, the ether having dissolved an extremely small quantity of oily matter. But if the milk has been previously boiled with a little acetic acid, this substance dissolves the caseous envelopes of the globules, and the butter is at once removed by the addition of sulphuric ether.”

5. That mechanical means are necessary to abstract the oil or butter from milk, is continually proved in the dairy by the operation of churning, which we may conclude acts by lacerating the minute envelopes, and allowing the oil to flow out. If the milk be first allowed to become slightly acid, the process, as is well known, is facilitated, which we can now readily understand by the action thus produced on the membranous envelopes.

I have repeated these experiments on the globules formed artificially by the union of oil and albumen, and have satisfied myself

that the addition of water, syrup, ether, and acetic acid produces exactly the same effect on them as on those found in milk.

That a delicate albuminous membrane possesses the property of rolling up, and uniting its edges so as to form shut sacs, may be proved by actual demonstration. I have frequently seen under the microscope fragments of nerve tubes elongate, break across, and form two or more globules, in the manner represented in the figure. The milk globules also, by pressure or friction, may be divided into several portions, each of which will assume the round granular form, however minute they may be rendered, and possess the same physical properties as when first observed.

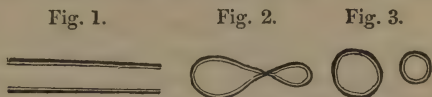


Fig. 1. represents a fragment of nerve tube. Fig. 2. The same united at the extremities, and attenuated at the middle. Fig. 3. The same, broken across, and forming two round globules.

I have made numerous experiments with oil and other glutinous substances, such as gelatine, gum, and syrup. I have found that any viscous substance, as well as the caustic alkalies, will enable us to separate mechanically portions of oil into minute drops or molecules. In none of these cases, however, could I produce a membrane as when oil and albumen are brought in contact; and that the molecules so produced are not surrounded by a distinct membrane, is presumable from the readiness with which, in many instances, they unite together, either spontaneously or under pressure. Again, when a membrane is formed by chemical precipitation (*membrano-molecular* structure of Harting), it differs from the Haptogen membrane of Ascherson, in the circumstance that friction resolves it into minute molecules of inorganic matter, and never into a cell structure, as when oil and albumen are rubbed together. Neither, according to the observation of Mulder, does the former possess the property of endosmosis and exosmosis, which the latter undoubtedly does.¹

From all these facts I think it may be concluded, that the globules of milk, as well as those formed mechanically by the union of oil and albumen, are structures composed of an albuminous envelope and oily contents, and that they are endowed, from the moment of their formation, with the physical property of endosmosis and exosmosis. This property is defined by Dutrochet in the following manner,—“Two heterogeneous and miscible liquids being separated by a membranous layer, there is established through the capillary conduits of this layer two currents, directed inversely, and of unequal intensity”² Dutrochet, in his latest work on the subject, distinctly tells us (p. 11) to attach no importance to the etymological acceptance of endosmosis and exosmosis; and that by the first he understands only the strong,

¹ Mulder's Chemistry by Fromberg, p. 376.

² Mémoires pour servir à l'Histoire Anatomique et Physiologique, des Animaux et des Vegetaux. Paris, 1837. T. i. p. 10, et suiv.

and by the second the feeble currents, which pass in contrary directions through a permeable membrane. He exhibited by numerous experiments, that this phenomenon was influenced by the nature of the membranous layer, and by numerous circumstances too well known for me to describe them at length. Suffice it to say, that the structures I have alluded to, formed by the mechanical union of oil and albumen, may be seen to exhibit the peculiar phenomenon which he discovered.

When it is remembered that oil and albumen pervade all organized bodies, that they are continually coming in contact, and that membranes and cells must thereby be necessarily produced; moreover, as the other soluble elements which enter into organized structures must communicate to the fluids various kinds of densities,—it will be clear that all the physical conditions necessary for endosmosis and exosmosis must be present. When, in addition, it is considered that modern anatomy and physiology have demonstrated that all organized structures originally consist of cells, composed, in like manner, of a membranous envelope, and endowed with the same physical properties, the importance of these facts must be recognised.

I consider, therefore, that the blastema containing the necessary nutritive principles in solution, precipitates minute oily particles, which are the elementary granules of histologists. These, either separately or united, constitute nuclei composed of oil, surrounded by an albuminous membrane. In this condition, they become subject to the physical law of endosmosis and exosmosis, and absorb or exude materials, according to the circumstances in which they are placed, and the unknown vital power to which they are subjected. The different isolated corpuscles are not formed from them directly, as Ascherson supposed, but are the result of a series of physical and vital changes occurring in the elementary granules and nuclei, which, however, are themselves produced in the manner he pointed out.

Hitherto there has been a difficulty in explaining how oily matters find their way into the lacteals by physical laws. It has long been known, that when animals are fed on fatty substances, the chyle becomes richer in fat. That this substance *does* get in, therefore, cannot be denied. According to Dutrochet, endosmosis and exosmosis only occur between *miscible* fluids; and it might be objected that oil and albumen do not come under this denomination. The difficulty, however, has lately been solved by some experiments of Professor Matteucci. He has shown, that if water be rendered very slightly alkaline, so as scarcely to act upon test-paper, and some oil be then shaken up with it, at a temperature of from 95° to 104°, an emulsion is formed, which exactly resembles milk. “Having,” he says, “filled a portion of intestine with this emulsion, I plunged it into the alkaline solution just described, keeping the temperature at from 95° to 104°. After a certain lapse of time, the latter became turbid, and assumed the characters of the emulsion within; so that

it may be fairly concluded that some of the latter had escaped through the membrane, and diffused itself through the liquid around." The following experiment appears still more conclusive:—"I filled an endosmometer with a very weak alkaline solution, and immersed it in the emulsion which I have shown you. The membrane employed was, as usual, that of the urinary bladder of the ox; and the two liquids were at the temperature of 86° at the commencement of the experiment. Endosmosis took place, and the emulsion penetrated the alkaline solution, so as to raise the columns of liquid 30 millimetres in a very short time."¹

Now, it is well known that the action of the stomach and intestines, conjoined with that of the bile, produces an oily emulsion, so as to furnish one of the conditions necessary for endosmosis; whilst the salts dissolved in the liquor sanguinis and chyle furnish the other. An elevated temperature and motion have also been shown to favour the process, both which exist in the animal economy. In the minute cells of the chyle, and animal structures generally, however, it must be carried on by fluids in which either oil and albumen are held in solution, or in such a minute state of division as not to be recognised by the most powerful instruments. Donné has shown, that when all the globules have been separated from the milk, the caseous fluid, when treated with ether, yields a considerable quantity of fat. It is therefore probable that minute cells assimilate oil and albumen in solution; that they become afterwards precipitated, so as to cause increase of bulk, and to manifest their physical properties. No doubt, also, both the oily and albuminous principles undergo modifications and transformations which it would be premature now to follow, and which are intimately connected with the process of development. How far this is owing to their union with saline matters, to their passage through the glands and lungs, to endosmosis and exosmosis, or to vital causes superadded to each of these respectively, or all combined, must be the subject of further research.²

¹ Leçons sur les Phénomènes Physiques des Corps Vivants. Par C. Matteucci. P. 105.

² It must be the future endeavour of histologists to determine what are the physical, and what the vital changes, which cells once formed undergo; and how far these are connected with or dependent upon each other. Some very interesting facts have already been discovered in connexion with this part of the inquiry. Thus, as regards physical changes, Professor Oesterlen of Dorpat, has shown that even insoluble substances can pass through the intestinal walls, and be detected afterwards in the blood by the microscope. He fed rabbits on powdered charcoal and Berlin blue, and subsequently found portions of these substances, varying in size from $\frac{1}{300}$ th to $\frac{1}{30}$ th, and was $\frac{1}{42}$ d of a line, in diameter, in the portal vein and right cavities of the heart. (*Monthly Journal*, May 1847.) If substances of such size can pass through membranes, the fact ascertained by myself some years ago, that charcoal in a molecular state passes into the epithelial cells of the air vesicles, constituting the black spit of colliers, need not excite surprise. The chemical changes, also, which occur in the cell-wall and the interior of cells, have lately been shown by Mulder to influence the contents, appearance, and development of these structures. Thus the salts,

Raspail has stated, parodying the celebrated words of Archimedes—"Give me a vesicle capable of absorbing, and I will make an organism." The discovery of Ascherson has placed the first at his disposal, but the possibility of producing an organism is as far removed as ever. The structures found in milk, and produced mechanically by the union of oil and albumen, are not vital structures, but when formed in the animal body, under certain conditions, they become so. The physical relations I have pointed out are only necessary preliminary steps for the addition of that unknown force we call vitality, which directs the ultimate form these structures assume. They are a *sine qua non*, without which vitality cannot be called into existence. The introduction of organic matter into the animal economy is necessary to support life. This matter becomes assimilated as it is called, that is, transformed into vital tissues. Without venturing to penetrate the mystery which is the ultimate cause of this transformation, I think the facts that have been brought forward will add another link to the chain of physical processes by which it is accomplished. This chain, I think, may now be composed as follows.

1st. Introduction into the stomach and alimentary canal of organic matter.

2d. Its transformation by the process of digestion into albuminous and oily compounds. This process is chiefly chemical.

3d. The imbibition of these through the mucous membrane in a fluid state, and their union in the termini of the villi and lacteals to form elementary granules and nuclei. This process is physical.

4th. The transformation of these into blood, which is a vital process.

The blood is a fluid so compound in its nature, that it becomes impossible to follow the changes which the matter absorbed from the food undergoes. It contains not only the results of digestion in the

and even organic matters, in solution, not only form new combinations, and are deposited in and around the cell, but, by being precipitated in the substance of the cell-wall itself, impede endosmosis and all further change in it. (*Mulder's Vegetable and Animal Physiology*, p. 383, *et seq.*) These facts prove to us the great importance of studying the physical laws in connexion with cell growth. On the other hand, the importance of the vital powers does not admit of question; and the laws whereby cells are arranged and grouped together, so as to constitute distinct and separate organisms, ought justly to merit our attention. The influence of the nucleus on cell development is at present a point of interest, and has led to the doctrine of germinal centres; concerning which I may remark, that it seems to me opposed to many facts revealed by morbid anatomy. The theory of pre-existing germs appears consistent enough so long as it is confined to physiology; but from what germinal centres, or previous nuclei, do the pus, exudation, cancer, and other cells arise, which had no former existence in the economy? Numerous observations, on the contrary, have convinced me that they originate from primitive granules and nuclei, in the centre of a perfectly fluid and structureless blastema, and that the theory stated in the text is the only one hitherto proposed capable of explaining their origin.

alimentary canal, but the products of the secondary digestion, or disintegration going on in the tissues, and whether one or the other, or both, are essential to its functions is unknown. It further undergoes important changes in the act of respiration. The moment we recognise the matters exuded from it, however, to form the structures, the same order of facts is to be seen, viz.—

1st. Exudation of liquor sanguinis, containing oil and albumen in solution.

2d. The mechanical union of these to form elementary granules and nuclei—and

3d. The vital transformation of these into the various tissues of the economy.

All the facts with which we are acquainted, derived from the study of dietetics, chemistry, anatomical structure, physiological experiment, and the laws of development, prove that the relation shown to exist between oil and albumen, and the physical laws described as resulting from it, are in continual operation. This is still further supported by the discoveries daily making in pathology, which point out various conditions of the animal economy, in which the healthy relations of the oily and albuminous principles are more or less deranged.¹

Some individuals are characterised by a tendency to obesity, others by excessive leanness. Such persons may live upon the same diet, and yet in the one case there may be large accumulations of fat, whereas in the other no such effect is produced. Such differences must evidently depend upon the variety of the assimilating process, that is, upon vital action, the laws governing which have hitherto escaped our observation. Still, as this vital action cannot operate without previously existing physical causes, a regulation of these, by management of the diet and exercise, will enable us to diminish or increase the oily and albuminous elements in the animal economy.

It has been long known, that under certain circumstances different tissues become loaded with, or even transformed into fat. The conditions which seem necessary for this result are a certain degree of inactivity of the structure, whilst the elements necessary for the formation of fat continually enter the animal frame in the food. The liver of geese is rendered fatty at Strasbourg, by confining the

¹ It seems to me, that of all the writers who have directed their attention to this subject, Dr Prout has taken up the most correct view regarding it. He observes, "The subject, in many points of view, is one of great importance; as I am satisfied, from long attention to the matter, that the oleaginous principle is much more deeply implicated, not only in the operations of organic life, but in those higher operations connected with the animal functions, than is commonly supposed. If such be the case, its pathology must be commensurate; and I venture to foretell that this will hereafter prove to be the case; and that some of the most formidable diseases to which animal existence is liable, will be found to be connected with, or to result from, the mal-assimilation of the oleaginous principle."—*On Stomach and Renal Diseases*. Fifth edition, pp. 259-60.

animal, and feeding it highly. By this process respiration is impeded, and the fat formed, instead of being burnt in the lungs, (according to the language of chemists), is stored up in the tissues, and especially in the liver. In man we occasionally find the same degeneration of the liver in drunkards, that is, where fluids abounding in carbon, the essential element of oils, are continually drunk. It is also found in consumptive people, that is, when the function of the lungs is for some time impeded, and especially in those who have been well fed and use little exercise. The oil so formed, was shown by Mr Bowman to accumulate within the hepatic cells.

There is a certain disease of the kidney, which the recent researches of Professor Gluge of Brussels have shown to be associated with a fatty degeneration of the organ, and which he has called *stearosis*; the oily particles in this case, as in that of the liver, accumulate within the cells of the organ.

There are certain disorders of parenchymatous tissues, known to pathologists under the name of inflammatory softening, or *ramollissement*. This alteration in structure may be found, on examination, to consist of an accumulation of minute oily granules, sometimes collected together in masses, at others surrounded by a distinct cell wall. These bodies, called by some compound granular, and by others exudation corpuscles, are evidences of local fatty collections.

The muscles themselves are often converted into fat, caused by the accumulation of this principle between the fibrillæ. The transverse and longitudinal striæ may be seen to be ultimately obliterated, and the whole converted into a mass of fatty granules. Fat cells also accumulate between the fasciculi. This kind of degeneration occurs in voluntary muscles, when from injury to the spinal cord, or disease of the joints, their motions are prevented, whilst the general nourishment of the individual proceeds.

Other kinds of fatty collections are known by the terms *lipoma*, *atheroma*, *cholesteotoma*, *steatoma*, and *meliceris*. These constitute various forms of tumour, either lobulated or encysted, and are either excessive local growths of adipose tissue, or an accumulation of oily granules within the structure of glands. *Encephaloma*, also, is a morbid growth often very rich in fat.

In the same manner as we have morbid conditions dependent on an excess of the oily principle, so we have others resulting from an increase in the albuminous.

Thus many tissues are converted into a dense albuminous or fibrous mass, known by the names of *cicatrices*, *indurations*, *strictures*, and *fibrous*, *cartilaginous*, and *scirrhus* tumours.

The whole range of morbid lesions denominated *tubercular* belong also to this class. *Tubercle* is a product varying much in constitution, but most frequently composed of an amorphous molecular matter, little altered by the addition of ether, and rendered more transparent by acetic acid.

In all these cases there is an excess of the albuminous and deficiency of the oily principles.

The great emunctory of fat, as a fatty compound, is by the liver in the form of bile. The great emunctory of albumen is by the kidney in the form of urea. There is a considerable analogy between the lesions produced in these two organs by the excess or diminution of oil and albumen, and by the concretions there occasioned.

Thus, in the liver, the albuminous or fibrous tissue may be increased, and cause atrophy of the cellular or fatty structure, as in cirrhosis; or the cellular structure may be greatly increased, causing enlargement of the organ, as in the fatty degeneration. The same thing occurs in the kidney, which may become enlarged and fatty from the accumulation of oil within its cells, or contracted and atrophied from an increase of the albuminous material. Again, the oily excretion of the liver may produce a crystallized matter called *cholestrine*, which accumulates in the bile ducts, forming calculi, and causing obstructive diseases. The nitrogenized excretion of the kidney, urea, likewise gives rise to a crystalline compound, uric acid, which also forms calculi, and brings on obstructive diseases.

When injuries or disorders attack the body suddenly, the individual having been previously in good health, certain lesions are produced which are denominated inflammatory, which are distinguished by the formation of corpuscles containing granules, nuclei, and cell walls, composed of oil and albumen in certain proportions. These corpuscles vary infinitely in exuberance of growth and power of reproduction. This also may depend upon the relative amount of oil and albumen entering into the blastema or structure of such growth. Healthy inflammatory products may be considered as the type of such formations, which we observe to diminish in organizable power as they abound in pure albumen, or become tubercular. On the other hand, they assume an excessive growth and power of reproduction as they abound in the oily element, as one observes in cerebriiform or soft cancer. This view is favoured by the circumstance, that we scarcely ever find tubercle and cancer associated, and observe that tubercle attacks in preference organs destitute of fat, such as the lungs, while it is rare in fatty organs such as the brain and liver. On the other hand, cancer attacks in preference fatty organs, such as the mamma and liver, and is exceedingly rare in the lungs and fibrous tissues.¹ Again, tubercle is common in the young, in whom assimilation does not produce an excess of fat; whereas cancer is most frequent in advanced life, where obesity and fatty accumulations are especially apt to occur.

In this manner we observe that an excess and diminution of the oily or albuminous principles are unfavourable to a healthy organiz-

¹ Cancer of the uterus will be considered a frequent exception. I have satisfied myself, however, that the great majority of so-called cancers of that organ, are simple hypertrophies and ulcerations, and that, when true cancer is produced, the uterine tissue is swollen, soft, and very fatty.

ation, and that a proper nutrition depends upon a certain relation existing between them. The exact amount of this relation, or the range which may exist within the limits of health, are not yet ascertained; observation, however, shows that there are certain derangements of the economy, which depend upon an excess or diminution of one or the other, and that these possess distinctive characters.

Although many of the diseased actions now alluded to, have long been known to pathologists, it has not yet been recognized, nor in many of them indeed has it been known, that they depend upon an altered relation between the oily and albuminous constituents of the body. If the opinions now advanced be correct, their importance in regulating the diet of animals, and in a system of therapeutics, must become evident. The facts and details connected with this part of the subject, indeed, are not only interesting in their practical application to the treatment of the disease, but afford further confirmation of the correctness of the theory advanced. The length of this communication, however, obliges me to postpone their consideration for the present.

ARTICLE III.—*On the Recent Difference of Opinion as to the Cause of Scurvy.* BY ANDREW ANDERSON, M.D., one of the Surgeons to the Eye Infirmary, Glasgow.

(Read to the Medico-Chirurgical Society of Glasgow, August 10, 1847.)

I HAVE no intention of describing this disease. There has come under my notice but one fact in its history which I believe to be new: the occurrence of well-marked scorbutic amaurosis, and its cure by lemon juice. On this, however, I shall not at present linger, my object not being to fill up the history of scurvy, but to offer a very few remarks upon its cause.

Dr Christison, in his interesting paper in the *Edinburgh Monthly Journal* (June, July, 1847), proves that scurvy can, in certain cases, be cured by adding milk to the food, and concludes that fluid to be anti-scorbutic in virtue of the casein which it contains.

This opinion at once struck me as being at variance with that usually held; and, thinking that I could reconcile the two, I drew out the substance of the following remarks.

Since then, it has appeared that Drs Lonsdale and Curran (*Edinburgh Monthly Journal of Medical Science*, and *Dublin Quarterly Journal*, for August 1847) have, as I did, thought Dr Christison partly in the wrong, and advanced proof that milk is not essentially, or at least not in all cases, anti-scorbutic. But as neither of these gentlemen has given that explanation of the matter which occurred to me, I venture to submit it still:

Dr Christison's *observation* is, that in the Perth prison, scurvy

broke out from treacle having been substituted for milk in the prisoners' diet, and that it disappeared when milk was given. He found that there was no deficiency in the mere quantity of solid food provided to them, and none in that of its nitrogenous elements; but that (excluding gluten, which has been proved to be, when taken alone at least, not nutritious) there was in the food a marked deficiency of *nutritive azotised matters*,—viz. fibrin, albumen, and casein.

His *deduction* is, that it was the want of these which caused the scurvy; and that it was by supplying such matter, in the shape of casein, that milk proved anti-scorbutic.

Now in the first place it must be obvious to all, that this doctrine will not account for every outbreak of scurvy, or explain every cure; for it is notorious that lemon juice, without a drop of milk, or any nitrogen, has prevented and cured the disease in instances quite innumerable. But the question is, may not scurvy be the offspring of different causes, and owe its origin now to the absence of fresh vegetables, and again to that of nutritious azotised food?

It seems to me that this must be answered in the negative; and that it is not very difficult to reconcile the opposite opinions, and to show how lemon juice and milk may both, and both in one way, be anti-scorbutic.

Briefly to recall facts, we know—

1. That it has been proved by authors, that while any debilitating and depressing agency may predispose to scurvy, its essential cause is improper food.

2. That food, to support human life, must contain fibrin, albumen, or casein—it signifies not whether they be of animal or of vegetable origin: and that to provide sufficient fuel for maintaining animal heat, non-azotised substances must in general be added, no matter whether they be amylaceous, saccharine, or oily.

3. That if nitrogenous food be absent, the body cannot be nourished, give as much of the other as you choose; and that if the latter be withheld, and the former taken in quantity sufficient to supply carbon to the oxygen absorbed, the system is apt to get loaded with superfluous nitrogenous matter, and gout, or worse, may follow.

But how is it that, both being given, the blood, if they alone be given, still becomes depraved, and scurvy is the consequence? It were easy to see a reason for this, if in all instances in which scurvy occurs, the quantity of nourishing food were, as in the Perth prison, too small; but there is abundant evidence to disprove this. Thus Dr Curran (*loc. cit.*) records a very bad case, in the person of a woman who had been fed on plenty of butcher meat, and bread and butter, tea and coffee, wine and porter. Dr Ritchie saw one parallel to this; and during the discussion on Dr Christison's paper in the Edinburgh Medico-Chirurgical Society (*Edinburgh Journal of Medical Science*: June), it was stated that the disease had occurred in a boy overfed with animal food, and in a lady whose diet was butcher

meat and milk. In these instances the food abounded in both varieties of the elements needful for nutrition, and yet it did not nourish well. How was this?

To find what was yet required, let us revert to the history of the prevention and cure of scurvy by lemon juice. This substance contains but 2 per cent of solid matter, of which 1.77 is citric acid.—(Christison's Dispensatory, *in loc.*) Four ounces daily will shortly cure a bad case of scurvy, and yet can contain no more than 40 grains of solids—solids too, which are not nitrogenous. This cannot possibly furnish pabulum to make the blood nutritious; and yet it removes the malady. The lady whose case I have just cited was rapidly cured by oranges, her diet being otherwise unchanged; in explanation of which I venture to suggest the following hypothesis:—

Food appropriate to man must consist of three parts—the nitrogenous, to nourish; the non-nitrogenous, to produce the extra heat required; and a third element, to aid in the assimilation of these. This element is furnished by the various, usually more or less acid, juices contained in vegetables and fruits. In favourable circumstances man may dispense with these, and live in tolerable health, but withal obnoxious to disease; and if he be weakened, as by confinement, or depression, or previous disease; or if the nourishment in the food be moreover scanty, as it was in the case of the prisoners at Perth, scurvy comes on.

All the substances eminent as anti-scorbutics contain more or less acid; and it is proved that the more acid the fruit, as Dr Trotter found of guavas, the greater is its virtue.

I presume then that these juices and acids act, not by themselves supplying nourishment for the blood, but by in some way promoting the assimilation of the nutritive part of the food; and I would compare their efficacy in scurvy to that of iron in anæmia. The anæmic patient may be fed exceedingly, but he cannot assimilate the nutriment he digests without the aid of a metal which itself can be no pabulum to the blood. And so the scorbutic may eat enormously of flesh, which is blood (Liebig), yet cannot change it into *his own* blood without the aid of certain vegetable matter; an aid which seems essential, though how it is given no man can tell. Vinegar and nitre, and one or two substances besides, have been found to cure scurvy now and then (by Henderson, &c.), but they have failed at other times. They probably exert in the process of assimilation an agency more or less like that of citric acid, although not so uniform or powerful; and so I suppose that milk may, by means of the *lactic acid* produced while it is digested, and not by means of its casein, effect in some instances the cure of scurvy. If it did so in virtue of the casein it contained, cheese should have the same effect, —a thing I never heard of. And milk, like vinegar and nitre, seems to fail more often than it prospers; for Drs Curran, Lonsdale, and Ritchie record cases of the disease as having happened in persons making free use of milk.

When the diet is nitrogenous enough, lemon juice will be more useful; but when, as in the Perth prison, it is not so, milk may be the best anti-scorbutic, because it will supply the essential food of the blood, as well as what I may call its condiment. That is, milk alone may in such a case be preferable to lemon juice alone; but, no doubt, "both are best."

Why is there sugar in milk? Not to form flesh of course. Not to produce caloric either; butter would serve that purpose well enough. The casein to nourish,—the oil to heat;—is it not possible that the lactine may be intended to promote the assimilation of these by being first changed into lactic acid (as all nurses know it is), and by then supplying to the infant the want of that vegetable food which he does not receive, and could not digest; for the purpose, in short, of preventing scurvy? Dr Andrew Buchanan well suggests that the *experimentum crucis* were to try whether *whew* would cure a man of scurvy.

And how do potatoes nourish men so well (*teste* Hiberniâ) that no other article of food will singly keep them in such health? Mr Crum had shown (Philosophical Society of Glasgow) that four pounds of potatoes contain about as much albuminous matter as an egg:—the root then affords starch to heat, and nitrogen to nourish; but peas and beans, which do as much, will not serve as man's sole food. They are deficient in the third, the anti-scorbutic element, which Baly has shown to be possessed by the potatoe; hence its surpassing value,—it is *milk for men*.

May we not in some such way as this reconcile Dr Christison's observations with common notions? The analyses which I am about to quote prove that there is no necessary lack of fibrin in scorbutic blood; nay, that even in severe cases that element may be unnaturally plentiful, and the corpuscles, moreover, not much diminished. The darkness of the colour of the clot, and its occasional jelly-like consistency, indicate no doubt a depraved condition of the blood: but the startling rapidity of the cure by lemon-juice appears to show that the error is in its vital structure, rather than in its chemical composition; and is repaired by putting into their right places, as it were, nutritive molecules already present in the fluid, rather than by the more tedious process of absorbing and adopting new materials.

When scurvy, however, does break out, it soon brings on anæmia; and then, of course, the corpuscles and the albumen are both progressively reduced in quantity, while the fibrin and the salts remain unchanged. This will be seen in the last analysis which I shall quote, which yet must not lead us to confound the essential with the accidental.

But Dr Curran, too, has a chemical theory of scurvy:—

"Dr Christison, by overlooking *the salts* contained in food, has committed an error which totally destroys all the value of his reasonings. That vulgar experience, which leads the bird-fancier to supply his birds with lime, should not be lost upon those who have

to direct the nutrition of the masses of the human species. Dr Aldridge is, however, the only chemist who has made his knowledge practically available on this subject; and from his excellent little work, I extract the following short, intelligible, general directions." (*Loc. cit.*, pp. 113-14.)

Dr Aldridge's theory, it would appear, is, that it is the want of lime and saline matter in the food which causes scurvy. This were remarkable, if true,—because if so, how should pure citric acid cure the malady; and why, in the celebrated historical case of the continental army, did dry herbs, containing of course all their salts, fail to alleviate the scurvy like the fresh plants. But we need not discuss this point; for Dr Aldridge's theory is not more correct than new. It is simply a *rechauffée* of that which, broached by Dr Stevens fifteen years ago, was refuted by Dr Kerr in the *Cyclopædia of Practical Medicine* (iii. 694). But as it was possible that Dr Aldridge might have analysed scorbutic blood before constructing his little work, I searched for his experiments, but in vain. Yet the materials may be found from which to form our judgment: in a paper by M. Fauvel, in the *Archives Générales* (Août 1847), there are recorded some analyses of such blood by the experienced hands of MM. Becquerel and Rodier: and these I copy, as well as a portion of two imperfect ones from Dr Ritchie's paper.

ANALYSIS OF THE BLOOD IN FIVE CASES OF SCURVY IN FEMALES.

B. and R's { FEMALE Analysis of BLOOD, of HEALTHY. (See Simon's Animal Chemistry, i. 233, 234.)		Average of two of Dr Ritchie's cases.	Average of two cases of little severity.	Average of two cases of much severity.	A very severe case indeed.
Corpuscles,	Average. 127.		116.	110.	79.
Fibrin,	2.2		2.8	3.8	2.2
Animal mat- } ter in serum, }	72.		65.	67.	56.
Saline mat- } in serum, }	*6.8	*6.	6.4	6.5	7.8
					(Anæmia coming on.)

* I have deducted 0.54 in this analysis for the iron, as not forming part of the saline matter of the serum.

Hence we perceive that in point of *fact* the worst scorbutic blood may contain rather *more* saline matter than the healthy fluid does. May I venture to add, that

Dr Curran, by overlooking the salts contained in the *blood*, has

committed an error which totally destroys all the value of his reasonings. That vulgar prudence which leads the reasoner to base his theories on facts, should not be lost upon those who wish to direct the opinions of the members of the medical profession. MM. Becquerel and Rodier are, however, the only chemists who have acquired any available experimental knowledge on this subject; and from their excellent little production I have extracted the preceding short, very intelligible, and particular analyses.

That scurvy is a "blood disease" no man can doubt; and I suspect that British blood has, by the past year of dry feeding, been depraved more generally than may be thought. All the blood drawn by my directions, during the last three months, from patients of the Glasgow Eye Infirmary (who presented no external sign of scurvy), has been of most unhealthy aspect: the clot large, soft, and dark, like black currant jelly; and the buffy coat, when present, gelatinous and pale.

It also appears to me indubitable, that scorbutic blood may be found in many differently perverted states. My friend Dr Ritchie (Edinburgh Monthly Journal, August, p. 83), has met with some containing just half the usual quantity of fibrin; in other cases we have seen that substance, on the contrary, augmented. The scorbutic state may be complicated with various other conditions of the system, which will of course tend to produce changes on the vital fluid, just as pyrexial blood varies according to the local lesion present. The effect of treatment furnishes a proof of this; for while bleeding is usually most hurtful in scurvy, and mercury quite a poison, cases do occur in which each of these means is indeed remedial. (Dr Bogie in Lonsdale, *loc. cit.* p. 102.)

It is reasonable also to conclude, that when anæmia has come on, the treatment peculiarly suited to that state may with advantage be combined with that for scurvy; so that while the simple examples of the disease may, as I have seen in patients of my own, be cured by pure citric or tartaric acid, the anæmic forms, in which the lips and the exuding blood are pale, will, as in a lady about whom I was consulted, be, by the use of iron, hurried on more rapidly to health.

ARTICLE IV.—*On some Peculiar Intestinal Concretions.* By Dr Schlossberger, Professor of Chemistry in Tübingen, formerly Laboratory Assistant in the University of Edinburgh.

1. *Groups of Incrusted Pear-cells occurring as Fæcal Concretions.*—Through the kindness of Dr Haerlin in Stuttgart, I obtained, last month, a number of small concretions which had been passed along with the fæces of a hypochondriacal married woman affected with diseased liver, and handed by her to that physician. They were in colour greyish-brown, and varied in size and form; none were, however, larger than a pea, and most of them were round or some-

what flattened; though of considerable hardness, a sharp knife could divide them into thin lamellæ; at the first glance they appeared homogeneous, but on long boiling in water they separated into small grains, most of which swam on the surface of the water, though some sank; exposed to red heat on a platinum leaf, they were, without previous liquefaction, immediately carbonized, and left behind a greater or less quantity of white ashes. Water, even after long boiling, dissolved only traces of them, which was also the case with diluted hydrochloric acid, which occasioned, only with a few, an effervescence of short duration; those grains which thus effervesced were exactly those which sank in water; it was therefore clear, that they consisted of an organic nucleus surrounded by an inorganic deposit consisting of carbonates and phosphates of the earthy salts. This inorganic coating, however, formed always a very thin layer; was sometimes deposited only on isolated spots of the surface of the organic body; and the most of these concretions gave, with hydrochloric acid, no trace of earthy salts. After these preliminaries, a single glance through the microscope revealed the nature of these bodies, for on being magnified 250 fold they were seen clearly to be incrustated cells resembling in form that of many bony corpuscles often described as spindle-shaped; the cells were partly round, partly elongated, all very rich in incrusting material, with generally a very small central cavity and very beautiful porous canals, whose arrangement bore an evident relation to that of those in the neighbouring cells. This appearance at once recalled to my memory the description and beautiful plates which Turpin has given of the calcareous concretions of many kinds of fruits, particularly pears and quinces.—(*Memoirs de l'Institut. t. xvii. p. 101. 1840*). Though, in other respects, often extravagantly fanciful, the identity of the microscopic cells here figured by him with the faecal concretions in question, could not for a moment be doubted; only those peripheral cells, little if at all incrustated, which, in Turpin's figures, often surround the concretions of the pear like the rays of a syngenesian blossom, were wanting: nor is this astonishing. Most probably those thin-walled external cells which seem so clear and transparent under the microscope are filled either with sap only, or at least only with some similar fluid, and must, during the passage through the intestines, be easily broken up and dissolved. The microchemical examination completely confirmed the idea already obtained, through the microscope, of the origin of these concretions. Iodine coloured them tolerably equally yellow; and a subsequent treatment of cells, so coloured with sulphuric acid, produced in some a greenish-blue colour; this, however, only by the action of strong acid for a considerable time; most of them were, on the contrary, coloured dark-brown; all were, finally, both cell and contents, completely dissolved. The cellular tissue of the primary cell membrane was manifestly completely changed into incrusting substance, probably after the manner of pseudo-meta-

morphosis in the mineral kingdom. Nitric acid coloured the cells dark yellow, becoming lighter on the addition of ammonia. The action of strong sulphuric acid showed very decidedly that the incrusting matter of those cells was formed in layers of varying thickness, which resisted the action of the acid variously. The explanation of the appearance of these vegetable concretions is apparent; these cells, almost entirely incrustated, in a manner resembling wood, and would therefore resist the action of the digestive juices as much as saw-dust, for example, if we swallowed it. To set the matter at rest, I put it to the direct proof on men and animals, and found the same concretions in the stools after the use of the stony bergamotte pear. This indubitable proof of the passage of such concretions through the intestines may also have a practical importance, in so far as they may perhaps form one of the most frequent causes of those concretions so often formed in the processus vermiformis, which in it, so unimportant a portion of the body, too often lead to a fatal peritonitis. Dr Volz has, indeed, lately in his well-written work on perforation of the vermiform process (*Carlsruhe*, 1846), denied the occurrence of organic foreign bodies as nuclei of these concretions; he gives, however, no sufficient theoretical reason for his opinion, and experience is decidedly against him. Those fruit concretions first described would lead most easily to the opinion of Volz, as even although frequently occurring in the vermiform process, and forming there the nuclei of further deposition from the fecal matter, they would be taken for inorganic bodies so long as the microscope and chemical reagents were not employed in their examination.

2. *Calculus from the Stomach of a Llama*.—I received from Prof. von Rapp, a concretion, the size of a bean, which he had himself removed from one of the numerous cells in the first stomach of the Llama; it had a greenish-brown colour, and yielded to ether, when boiled in it, a green pigment apparently identical with chlorophylle. On being broken in pieces it was found to consist of a grain of corn as nucleus, coated with numerous thin and differently coloured layers of earthy salts, manifestly precipitated from the fluids of the stomach and those of the nutritious matters contained in it, upon this organic nucleus thus imprisoned within one of its cells. Most indubitably this precipitation took place in the cell, which was, consequently, considerably dilated; the opening into it being, however, much too small to admit of the entrance of the calculus, and scarce large enough for that of the barley-corn, an incision had to be made into the opening of the cell to extract the calculus: this case is also against Volz.

The Lamellæ of this concretion consisted in 100 parts, of

46·61 Phosphatic earths (with traces of earthy carbonates and chloride of sodium).

14·59 Organic matters.

38·80 Water.

Part Second.

REVIEWS.

Researches on the Chemistry of Food. By JUSTUS LIEBIG, M.D.
Edited from the Manuscript of the Author, by W. GREGORY,
M.D. Pp. 144.

The Chemistry of Vegetable and Animal Physiology. By Dr G. J.
MULDER, Professor of Chemistry in the University of Utrecht.
Translated by Dr FROMBERG; with an Introduction and Notes
by JAMES F. W. JOHNSTON. Parts I. II. and III. Pp. 614.

THE two works whose titles we have placed above, are remarkable from the contrast they present between the methods pursued by two distinguished men, in the investigation of one and the same department of science. Every one who has paid the slightest attention to the recent progress of physiological chemistry, must be more or less acquainted with the controversy which has been going on for some time past between Mulder and Liebig. Of this controversy Liebig's present work must be considered as the latest manifesto, the first twenty-seven pages being wholly occupied by a renewed attack upon Mulder, and the methods of investigation pursued by him;—in the course of which we are informed that Mulder has adopted a totally erroneous method of research, owing to which physiological chemistry has made no real progress during the last ten years;—a very strange admission on the part of the most voluminous writer on the subject during that very period. We are unwilling to occupy the pages of the Journal, or to embroil ourselves in a controversy in which so much acrimony has been displayed. Two observations, however, have suggested themselves during a perusal of the works before us. The one is, that Professor Liebig ought to learn prudence and forbearance, from the recollection that up to last year he was the warm advocate of the views he now condemns; that the experiments by which they were supported were over and over again confirmed in his own laboratory; and that the bitter attacks which he now directs against the supporters, were then levelled at the opponents, of the proteine theory. The other observation which occurs to us is, that it is impossible to read the works of these two authors without coming to the conclusion that Mulder brings to his task acquirements far more extensive than those possessed by Liebig; for he is not merely a skilful chemist and cautious experimenter, but is, moreover, a philosophical physician and practical histologist, while Liebig is essentially a chemist; and so long as he confines himself to pure chemistry, his observations and researches are characterised by the

highest clearness and precision; it is when he comes to the more strictly physiological part of his work that he appears to least advantage.

The Researches on the Chemistry of Food contain the result of an interesting series of investigations on the constituents of the juice of flesh, to which we proceed to call the reader's attention without further reference to controversial matters.

Professor Liebig commences by recapitulating the previous researches of Berzelius and Chevreul on the extract of meat, in which he alludes to the supposed discovery of lactic acid by Berzelius, and of *kreatine* by Chevreul. This latter substance was observed by Chevreul in portable soup about twelve years ago, but in so small a quantity, that Berzelius, in repeating the experiments, was unable to obtain it, although Wöhler and Schlossberger were more fortunate in preparing it in very small quantity. The method of obtaining it with certainty and facility is detailed by Liebig so fully and clearly, that no experimenter can now fail in obtaining it, provided he works on a sufficiently large scale; for the proportion in which it is contained in flesh is exceedingly small,—in proof of which we subjoin the following table, which is given in the note, page 46:—

1000 parts of the flesh of fowl yielded	3.05	kreatine
1000 ... the horse ...	0.72	...
1000 ... the ox ...	0.697	...

Kreatine, as obtained by the process described in this work, is in the form of perfectly colourless and transparent crystals belonging to the klinorhombic system, which dissolve in 74.4 parts of water at 64°, and in a much smaller quantity of boiling water, but are nearly insoluble in alcohol and ether. The analysis of this substance gives for its constitution the formula $C_8H_9N_3O_4 + 2H_2O$; from which 2 equivalents of water are expelled by the temperature of 212°.

The most interesting and curious fact, however, regarding kreatine, is its decomposition when treated with acids. By simple evaporation of its solution in hydrochloric acid, there is obtained the salt of a new organic base, agreeing in all its chemical relations with the vegetable alkalies, to which Liebig gives the name of *kreatinine*. The decomposition which takes place is of an extremely simple character; kreatine being converted into kreatinine by the loss of 4 equivalents of water, which leaves for the constitution of kreatinine $C_8H_7N_3O_2$. Liebig has further shown that kreatinine exists, though in a small quantity, in the juice of flesh.

In addition to muscular fibre, both of these substances are found in the urine in its normal condition. Some years since a peculiar substance in crystals was discovered, almost simultaneously by Heintz and Pettenkoffer, in the urine, which Liebig has now shown to be a mixture of kreatine and kreatinine, though the presence of the former had been previously established by Heintz, whose name, curiously enough, is never once mentioned by Professor Liebig.

Liebig has found that kreatine exists only in fresh urine, and that during the process of putrefaction it is converted into kreatinine, which renders it probable that the small quantity of the latter substance found in the muscular fibre may be a product, and not a true educt, as Liebig himself supposes it.

Liebig enlarges considerably on the function of these substances in the vital processes, which he considers to be highly important, and in the latter portion of the work attributes to them an exaggerated degree of influence on the nutrition of the body. The presence of kreatine in food is, according to him, intimately connected with its nutritious qualities. In this view, however, we cannot coincide; it seems to us impossible to suppose that the nutritive powers of a pound of beef-steaks can be very much or at all influenced by the $4\frac{1}{2}$ grains of kreatine which it contains; moreover the actual practice of mankind is opposed to such a view, for the most common food, namely, beef, contains also the smallest portion of kreatine, and the fox and marten, which contain the most, are universally avoided as food.

A different and more probable view has been proposed by Heintz, which is probably the reason why his name has been excluded from the work of Professor Liebig. He considers kreatine to be a product of decomposition of the muscular fibre, an excrementory substance, applicable to no further use in the system. The separation of it, in what must be considered a comparatively large quantity in the urine, is strongly in support of this view, and various other confirmatory facts are scattered through the work; thus, the fox killed in the chase, after violent muscular exertion, during which, of course, active transformation of the muscular fibre was going on, contains (page 45) ten times as much kreatine as one killed after being kept in confinement for 200 days; again, the heart of the bullock, an organ in constant muscular action, is recommended as an abundant source of kreatine. Sarcosine is another base, obtained by the action of baryta on kreatine; the elements of urea are separated, and sarcosine, having the formula $C_6 H_7 N O_4$ is left; but for the properties of this substance we must refer to the work itself.

The acid constituents of the juice of flesh are then investigated, and found to be two in number; one a new nitrogenous acid, inosinic acid—a very interesting compound, but forming too minute a fraction of the juice of flesh to be considered important; and lactic acid, the presence of which was long since asserted by Berzelius, and which Liebig has now proved by ultimate analysis. In another part of the work, page 101, he enters into some speculations on the probable use of lactic acid in the system. He has found that it does not exist in healthy human urine, and that even, after the administration of large quantities of lactate of potass, none can be detected in that secretion. He infers from these facts, that lactic acid is one of the respiratory elements; and this may be in some instances the case, but cannot be so universally, for Boussingault has found lactate of potass, to the extent of 17.6 parts in the 1000, in the healthy urine

of the cow. If these observations of Boussingault be confirmed, then it is clear that the function of the lactic acid cannot be completely established; and we must look to further researches for the full elucidation of both its use and origin, which may not improbably throw an important light on some of the most obscure phenomena of the living body.

Professor Liebig then enters into a variety of minute details regarding the inorganic constituents of the flesh and blood, from which he draws conclusions which appear to us to be in some respects unwarranted by the facts, and are certainly totally at variance with the views contained in his former works. They are even, in some respects, at variance with known facts; as, for instance, in the case of his explanation of the use of common salt as a condiment. The cattle of inland districts consume fodder which contains only salts of potass, principally in the state of phosphate, while phosphate of soda is found in the blood. Hence, says Liebig, common salt being added to the food, double decomposition ensues, phosphate of soda and chloride of potassium being formed. Now, the whole of this explanation is founded on the loose statements common regarding the advantage of supplying cattle with a considerable quantity of common salt, statements which have been recently put to the test of experiment by Boussingault, and found to be totally erroneous; his experiments having shown, that when two lots of cattle were fed on the same fodder, the one with and the other without salt, no difference could be discovered between their weights after the lapse of a certain period, or rather the minute difference observed was to the advantage of those *which got no salt*.

Into the third section of the work, which is designated "Practical Application of the Result of the foregoing Investigation," we do not intend to follow our author—it contains his views of the art of cookery, which we strongly suspect will not meet with the approbation of the legitimate professors of the culinary art.

It will be observed from the *resumé* we have given of Professor Liebig's work, that it contains much interesting matter, and is, in fact, the opening up of an almost entirely new field of chemical investigation; it is, however, a very incomplete work, containing views which we feel assured will not stand the test of further investigation, and we cannot help thinking that the author would have acted more wisely had he published it in the pages of a scientific periodical instead of presenting it to the public in its present form.

We have entered into so full details regarding this work, that we have little space to do more than recommend to our readers the important work of Professor Mulder, which we believe is destined to form the standard of our present knowledge of Physiological Chemistry. We trust at a future period to recur to the consideration of it when it is more complete, and when we are in the condition to lay before our readers the results of the investigations regarding proteine with which Mulder is at present occupied. These investigations, a short account of which will be found in the form of two

letters from Mulder, in the two last numbers of the *Edinburgh Philosophical Journal*, are likely to modify in some respects the nature of the formula for proteine, though they leave untouched the great fundamental principle of the persistence of one and the same group of atoms, throughout the principal constituents of organised beings.

On the publication of the researches of Mulder and Donders, on the chemistry of the animal tissues, we shall again allude to the work of the former in its relation to structural anatomy and pathology. In the mean time we may observe, that the plan pursued by Mulder of analysing tissues under the microscope, is the only method which can satisfy the physiologist, and advance our knowledge of the true chemistry of the tissues. He is a histological chemist, and in this capacity, without subscribing to the accuracy of all his statements, we recommend his work with the utmost confidence to the consideration of our readers. The value and correctness of such a method of research will be better understood from the following excellent observations by Professor Johnston, appended to the third part of the work:—

“Every new branch of natural knowledge passes through a succession of stages—the line of inquiry becoming more difficult and refined as researches are multiplied. In the chemical analyses of parts of animals which have hitherto been undertaken, we have in general assumed substances to be simple, or single, which the microscope now tells us are mixtures of different parts, substances, or structures. A new course, therefore, is to be begun. We must first separate the parts more accurately, and then analyse them again more rigorously.

“An amusing English chemist lately dried and burned an entire mouse, and from the results of his combustion, drew grave results in regard to points which lie at the very confines of our existing knowledge. He might as well have put a whole man into his crucible or his combustion tube, and reasoned upon the nature of the ash, or the proportions of the gases he had collected.

“But chemical histology will introduce new refinements. The useful, though introductory, and as it were tentative results of our past analyses, will give place to more patient and minute researches, such as those to which the present work points the way. It is a somewhat discouraging thought indeed to one who now occupies himself with the unappreciated labours of chemical analysis, that in a few years all his results will be superseded, and his toils forgotten. There is a pleasure, however, in contributing to the advance of knowledge, which he experiences day by day, and which is independent of external things. And though his contemporaries may not reward him with the esteem which is due to the humblest contributor to that stock of knowledge which is the common good of all, yet the love of truth will sustain him under many discouragements, the perception of new wonders in the works of the Deity will lift him over his difficulties and depressions, and he will persevere as a matter of duty in those labours by which the welfare of his race and the glory of God are to be alike promoted.”

A Practical Treatise on Percussion, or an Exposition of the Applications of this Method of Exploration to the States of Health and Disease. By L. MAILLIOT, M.D., &c. Translated, with Notes, by GEORGE SMITH, M.D. Edin., Madras Medical Service, &c. Madras, 12mo. pp. 220. 1847.

ALTHOUGH much has been said and written upon percussion, its advantages are very little appreciated by British practitioners. Even in the clinical wards of our hospitals, we have seldom seen an attempt made to limit the exact size of the heart, lungs, or spleen, not to speak of the kidney, ovary, bladder, or uterus. The great practical advantages to be derived from this mode of exploration, however, are better understood on the continent, and as the knowledge of these extends, the great merits of M. Piorry will become more evident. In the work before us, indeed, we are told that percussion has been more or less practised by several individuals from the days of Jean Tagault, who wrote in 1580, up to those of Auenbrugger, who first published in 1761. But neither Auenbrugger, nor Corvisart and Forbes, his translators and commentators, can be said to have evolved more than the principle. The art with all its details as it now exists—its value in the diagnosis of numerous diseases—the merit of having defended it in modern times against the attacks of the sceptic and the ignorant, and of having established it as one of the most precious means of arriving at an exact knowledge of the nature and seat of disease—is undoubtedly owing to the talents, labour, and perseverance of M. Piorry.

Two reasons may be assigned for the superficial knowledge of percussion in this country. The first is, that it has never been taught clinically in the manner Piorry and his followers practise it, so that its advantages are unknown. True, every one finds it necessary now-a-days to tap the chest, for the same reason that they apply a stethoscope—not because they themselves can thereby derive any real benefit, but because it is the fashion, and gives an appearance of accuracy and profundity. Such persons have never been taught the art, and must necessarily be altogether incredulous as to the possibility of ascertaining with precision the exact size of the heart, spleen, gall-bladder, kidney, or any other organ, by means of mediate percussion. They have never seen it done, and find, on visiting the hospitals where they were educated, that their former teachers are as sceptical and ignorant of the matter as themselves. In the schools of Paris, Vienna, and Prague, the value of percussion is daily exhibited; and if seeing, in this case, be necessary to belief, we can only advise a visit to the wards of Piorry in the first-named city, or to those of Skoda and his pupils in the two latter.

The second reason is, that percussion, like every thing really useful in the exploration of disease, requires time, patience, trouble, and constant practice for its acquisition. It can only be learnt at the

bed-side, or on the dead body, under the eye of a teacher; and so long as clinical medicine and morbid anatomy are taught in lecture rooms, it is not to be expected that percussion, or any other useful art in diagnosis, can be communicated to the student. It unfortunately happens, that the appointments to our hospitals are seldom determined by the medical knowledge or skill of the successful candidate, and clinical teaching in this country is too much considered as an unimportant appanage of some other chair. We need not feel surprised, therefore, that such of our youth who desire distinction, should seek that knowledge in foreign schools which they cannot acquire at home, or consider their education with us as only preparatory to the great clinical schools of Paris or Vienna. This subject, however, is far too important to be fully discussed at present, although we feel satisfied that the errors of those who nominate medical officers to our hospitals, and who, therefore, are the directors of practical medicine, in this country, is the real cause of that deficiency in diagnosis of which we complain generally, and of the want of dexterity in percussion especially.

The present translation of M. Mailliot's excellent work, is well calculated to assist the student in the important art (improperly called science) of which it treats. After a short historical notice, the author describes the different methods of performing percussion. We quote his observations on the superiority of the pleximeter to the fingers.

"If, as we shall see presently, percussion is so unsatisfactory when practised over thick muscles in the way recommended by Auenbrugger, how can we reasonably make use of one or several fingers as instruments of mediation, when we reflect that they are composed of soft and thick parts? what arguments have been adduced to render this reasoning logical? People are contented with stating that the finger is infinitely more convenient, without putting themselves to the trouble of inquiring whether it is also equally useful. The proofs to the contrary will be evident if they reflect for a moment upon the principal inconveniences attending the use of the finger as a medium of percussion. Its employment is, generally speaking, more complained of by the patient, because it yields to the impulse of the percussing hand, becomes buried in the tissues, and spreads over a large surface those unpleasant jarring sensations, whose extent can be limited so readily by employing the pleximeter, and retaining it firmly applied by the two auricles. This sensation, which disagreeably affects even those in good health, explains how painful digital percussion must be when practised upon parts of the animal frame, whose sensibility is increased by a recent vesicatory, moxa, abscess," &c.

"For this reason Piorry's pleximeter has in such cases been justly preferred. Another circumstance tending to increase the pain when the finger is employed, arises from the small extent of the phalangeal surface, concentrating those disagreeable feelings which the pleximeter, by diffusing over a larger surface, renders more supportable. It is true that the advocates of digital percussion have insisted upon its more easy application to the intercostal spaces, for example; but on the one hand the pleximeter with a little practice may under these very circumstances be applied with equal facility, and on the other, those who have started this objection have alluded to exceptional cases, observed only among patients emaciated by age or disease.

"It is often necessary to percuss over the edge of the pleximeter when we

wish to limit an organ as exactly as possible ; but how, in such a case, can the finger, which can only be percussed over the centre of the phalanges, act as a substitute for that instrument ?

“ When the fingers percussing strike the finger percussed, they slide upon the skin and render the tactual sensation indistinct ; there is also the frequent disadvantage of a change in the angle of percussion ; hence erroneous results are obtained when digital percussion is made through any covering sufficiently loose to be displaced by the finger. The pleximeter on the other hand is exempt from these inconveniences, because the finger does not slip upon it.

“ When we percuss patients whose tissues are œdematous, or when, in searching for parts deeply situated in the abdomen, we require to depress the parietes, the skin imbeds the finger which is to receive the shock ; but no such disadvantage attends the use of the pleximeter.

“ If, notwithstanding the comparison I have thus attempted to draw, some of my readers should still remain in doubt regarding the preferable medium of percussion, I would advise them alternately to explore the same organ upon the finger, and upon the pleximeter, so that they may draw their conclusions, not from my opinions, but from the comparative results thus obtained ; I would recommend them moreover to experiment, not so much upon the living as upon the dead subject, in order that their researches may be confirmed on the spot ; and would request them to suspend their judgment until they have devoted their time and attention to this practical study ; mean time I must not conceal the fact, that all eminent authors prefer and recommend the pleximeter in examination of the abdomen, and in percussing parts rendered painful by various causes. In conclusion, I cannot understand how the pleximeter is deemed valuable in those cases where the fingers give little or no information ; how that instrument is always had recourse to when the sensibility of the patients must be spared ; and yet, how persons can deny its superiority in the majority of instances, whether the patient happens to suffer or not.”—pp. 17-19.

In a paper inserted in this Journal for February 1842, Dr Hughes Bennett described at some length the general and special rules to be attended to in practising mediate percussion. We think it unnecessary, therefore, to dwell on this portion of the work. In speaking of particular organs, we consider the author's anatomical description of the exact situation and seat of each likely to be very useful. The following is the mode in which he introduces the consideration of the HEART.

“ *Anatomical Position.*—Situated in the left side of the chest between the lungs and above the diaphragm, the heart is protected posteriorly by the vertebral column, anteriorly by the last pieces of the sternum and by the cartilages of the last true ribs upon the left side.

“ The left edge of the heart is received into a considerable excavation of the corresponding lung, the right or inferior margin rests upon the muscular partition of the diaphragm, and the base, gently sloped from above downwards, and from left to right, is separated from the eighth dorsal vertebra by the aorta and œsophagus. It is impossible to determine à priori at what portion of the skeleton of the thorax this base is to be found ; sometimes we observe it corresponding to the right edge of the sternum, but more frequently to the sterno-cartilaginous articulation of the third rib ; at other times it is found occupying an intermediate point between the articulations of the sternum with the costal cartilages ; the apex beats between the fifth and sixth rib in the one case, and between the sixth and seventh in the other ; it is enough to show how variable the size of the heart is, and consequently how much it behoves us to ascertain its exact position in every individual instance.

“ The axis of the organ is directed obliquely from above downwards, from right to left, and from behind forwards.

"*What is the size of the Normal Heart?*—Corvisart, Laennec, Cruveilhier, &c., think it impossible to determine with mathematical precision the dimensions of the heart in the healthy subject. The first of these authors, indeed, goes so far as to despair of ever possessing a rigorously exact standard with which to compare the size of a dilated heart. (Mal. du Cœur, p. 55—1818.) M. Bouillaud, however, maintains, that 'though the solution of the problem be difficult, we ought not to consider it impossible, or suppose that we cannot under certain circumstances ascertain the mean of the heart's size.' (Mal. du Cœur Prolég. p. 3, et suiv.)

"I may now mention what I conceive to be the proper method of arriving at an estimate which shall be of value at the bed-side of the patient. The subjects ought first to be divided into several classes, and a certain number of individuals presenting the same pathological and physiological conditions should be selected from one given age to another, and the mean of the different diameters of the heart ascertained upon a large scale. These experiments should be repeated as often as there are different physiological or pathological states. Such would certainly be the surest way of arriving at the truth, because in the case of any patient we could with confidence make use of the mean which best suited his age, sex, constitution, or disease; at the same time, however, it should not be forgotten that we ought never to pin our faith too implicitly to the data of statistics.

"But how are we to ascertain the volume of the organ?

"Some map out that part alone which is left uncovered by the lungs; others act more judiciously by measuring the whole circumference. 'Mediate percussion, employed by Piorry with such dexterity,' says Cruveilhier, 'enables us by tracing the dullness of sound to map out the volume of the entire heart, even of those portions covered by the lung; moreover, it enables us, though with some difficulty, to circumscribe that organ inferiorly from the margin of the liver, and to distinguish the dullness belonging to each.'

"Since M. Piorry has shown the possibility of mapping out the heart, he has continued to prove by facts the truth of this proposition; and although I myself have frequently demonstrated the accuracy of these statements upon the dead body, in presence of all who took an interest in such researches, yet in most of the Parisian hospitals the physicians restrict their investigations to that portion of the heart which impinges upon the thoracic parietes, as if there existed an invariable relation between the uncovered portion of the heart and that masked by the lungs."

Minute directions are given for the percussion of every organ, as well as for that of the collections of fluid in the serous cavities. We select the rules given for percussion of the KIDNEY, a feat which we believe has only yet been performed by Piorry and his pupils.

"*Manipulation.*—Place the patient upon the edge of his bed, with his face downwards, and his abdomen supported by pillows, so that the intestines may be pressed as high as possible towards the inferior surface of the liver.

"Carefully map out the situation of the spleen, tracing its outline upon the surface either with ink, or with the nitrate of silver.

"The same should be done with the liver, the posterior limits of which will be detected about two and a half, or three inches external to the vertebral column.

"We should next ascertain how far the thin lamina of lung, formerly mentioned as situated behind the kidney, extends downwards.

"Having placed ourselves upon the right or left of the patient, according as we wish to explore one or other kidney, we first mark out the points corresponding to the spinous processes of the lumbar and of the three or four last dorsal vertebræ; we then ascertain the inferior margin of the last rib, the iliac margin of each side of the spine, and finally the breadth of the vertebral

column above the renal region, where, at the eighth or ninth rib, the bodies of the vertebræ will generally extend about eight lines on each side of the spinous processes.

"The spine being measured, we trace upon each side an oblique line, extending from the eleventh dorsal vertebra to the superior border of the ilium, to represent the direction of the external border of the psoas magnus.

"We thus take advantage of the connexions existing between the spine and the kidneys, and determine approximatively the direction of the internal border of these organs. The oblique line now mentioned will serve as the basis of further researches, and we already know that dullness alone will result from percussing the space between it and the vertebral column, and that external to the line, we shall both above and below the kidneys obtain a more or less distinct resonance.

"It is scarcely necessary to observe, that in order to render the transitions of sound more distinct, the pleximeter ought to depress the tissues forcibly, particularly towards the lower part, where the thickness of the parietes is very considerable.

"How ought percussion of the renal glands to be practised? It is evident that in the case of the left kidney we may employ considerable force; but when we would limit the position of the right, the impulse of percussion must be gentle, in order to avoid confounding the dullness of the liver with that of the kidney, and to enable us to detect the presence of the lung over the second and third false ribs. About this spot, a moderate impulse communicated to the pleximeter assists us in determining the superior border of the kidney, by the deficiency of sound, and by the resistance, which is greater than that observed in the case of the liver.

"The superior limit of the renal gland being ascertained, we carry the pleximeter gradually downwards, and a gentle percussion enables us to observe the descent of the lung behind the kidney; at the spot where the lung ceases, we find the dullness peculiar to the kidney alone, until the clear, often tympanitic or humoral, sound again warns us that the inferior limit of the organ has been attained.

"A right line uniting the superior and inferior limits of the kidney points out the great diameter of that organ; this alone however is not sufficient, because the limits of the convexity must also be ascertained if we would obtain an accurate idea of the form and dimensions of the gland. For this purpose we percuss from within outwards. 1. Along a line falling perpendicular to the middle of the great diameter of the kidney; 2, along two other lines which start internally from the same point as the preceding, and divide the space comprised between the two right angles now spoken of into four equal parts. Upon each of these three lines we detect points corresponding to the exterior circumference of the kidney. In this manner the organ is completely mapped out."

We warmly recommend the study of this little work to the profession. It will be found interesting from the numerous cases and practical remarks it contains, and must be considered an important contribution to a knowledge of the seat and nature of disease. The translator has performed his task in an able manner, and added to the value of the work by many judicious observations and quotations in the form of notes.

Observations on Aneurism, and its Treatment by Compression. By
O'BRIEN BELLINGHAM, M.D., Edinburgh.

THE revival of the treatment of aneurism, by compression of the
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artery between the aneurismal dilatation and the heart, has of late attracted much attention among surgeons. The practice of compression, as formerly applied to the tumour itself, as well as to the artery above it, is of ancient date; but the compression of the vessel on the cardiac side of the aneurism alone, appears first to have been successfully practised by Pelletan, and other French surgeons, not long after the beginning of the present century. The practice, however, does not seem to have gained the confidence of surgeons, as, from 1810 till within the last four years, we hear only of occasional cases in which compression had been resorted to.

In 1824, the first case successfully treated by compression is recorded by Mr M'Coy of Dublin; and shortly afterwards Mr Todd succeeded in the cure of a popliteal aneurism by the same method. A third case is recorded by Mr White, of the Westminster Hospital, where the femoral artery was compressed for the cure of a popliteal aneurism. The pressure maintained in this case, however, was much more severe than that now employed; the object to be attained being adhesion of the coats of the artery at the point compressed,—a condition which is now found quite unnecessary for accomplishing the cure of aneurism. In the history of this case it is stated, “that the woman bore the pain heroically for five days, but the parts compressed sloughed deeply. The cure was completed, but the pain, danger, and risk incurred, were infinitely greater than any which could have been sustained from the usual operation.”

Since 1843, however, when compression was successfully employed in Dublin by Mr Hutton in a case of popliteal aneurism, the number of successful cases has rapidly multiplied. The instruments employed have been modified and improved; the degree of pressure necessary for the solidification and obliteration of the aneurismal tumour has been found to be much more moderate than that formerly used, and the practice has found an able and zealous advocate in our author.

The cases on which the observations are founded, are 27 in number, 23 of which were cases of popliteal aneurism, and 4, aneurism of the femoral artery. The results of these cases may be briefly stated as follows:—Of the 27 cases, 19 appear to have undergone a cure by simple pressure on the artery in the groin and upper part of the thigh, varying in duration from two days to three months. In 2, cures were effected by the same means, combined with pressure on the aneurismal tumour. In 2, the pressure was abandoned, and pulsation ceased at a subsequent period. In one of these the pulsation ceased after much exercise; in the other it ceased suddenly, after a violent exertion, which was followed by severe pain in the limb. In 1, a cure was effected by means of compression on the tumour alone. Case 13 died suddenly from disease of the heart, two days after the pulsation in the tumour had ceased. Case 24 died from erysipelas, induced by the pressure of the pad in the groin. And the last case mentioned is that of a drover, who, after

a fortnight's compression, preferred ligature of the vessel to a further continuance of the pressure.

These facts show that, in the great majority of cases of popliteal and femoral aneurism, moderate and carefully regulated pressure on the artery alone may be relied on as an effectual method of obtaining a cure of this disease. That the process, however, entails much suffering, restlessness, and fatigue on the patient, is undeniable. That this is a practical objection to compression has been proved by many cases recorded and unrecorded, where the patient, after the continuance of pressure for some time, has insisted on its being abandoned, and giving place to the application of the ligature. That it is not altogether unattended by danger is proved by at least two cases¹ which have been placed on record, in which death was directly caused by pressure on the femoral artery (case 24 quoted by the author, and a case related by Mr Busk in the *Monthly Journal of Medical Science*, February 1847.)

We must notice one other objection of a less important nature to the compression of the artery. Consolidation and adhesion of the parts in the neighbourhood of the artery is produced by the pressure of the pad of the compressor, which, in at least one instance, has been found materially to increase the difficulty of the dissection necessary for the deligation of the vessel, in the event of this operation being found necessary. That these objections render surgeons, who have had reason to place perfect confidence in the treatment by ligature, unwilling to relinquish this practice, is not to be wondered at; and, till experience shows the operation to be more dangerous than it has been found of late years to be in this country, we cannot expect to see it abandoned for the more tedious and irksome method of compression.

Secondary hemorrhage and mortification of the limb are the two untoward events which are said frequently to follow the application of the ligature. Statistics have been drawn up by Lisfranc and others to show the frequency of the former of these occurrences, but we place little or no reliance on such statistics. We have seen in Continental hospitals, on more than one occasion, the femoral artery cut down upon and handled as an anatomist would handle the artery of a limb which he was about to inject for dissection, and when such cases prove fatal from secondary hemorrhage, as they frequently do, they are added to the statistical account to counterbalance the number of successful cases, where the operation has been cautiously and well performed.

Of the surgeons who have written on the subject of late, we find Mr Syme stating that he had tied the femoral artery *sixteen* times, and had in no case experienced any bad effects from the operation. Mr Busk writes that he has tied the femoral artery *nine* times on

¹ A third will be found in No. 78 of this Journal, where pressure on the tumour, combined with pressure on the artery in the groin, proved fatal to the limb from gangrene, necessitating amputation.

account of aneurism with the same good result. Dr Toogood, of Torquay, states, "I have tied the artery, and assisted others many times in popliteal aneurism, without a single instance of failure." Mortification of the limb has been proved by experience to be a very rare sequel of the ligature of the main artery of a limb, and we do not hesitate to say, that it is more to be dreaded in the treatment by compression than in that by ligature.

Two other points remain to be noticed in the effects of compression. That moderate compression on the artery acts favourably by promoting and giving time for the establishment of the collateral circulation, is undoubted: but we think that this is likely to be more than counterbalanced by the injurious effects caused by the pressure, which must be at the same time, as the author allows, exerted on the femoral vein. From the experience, however, in compression, the interruption to the venous circulation does not seem to have proved injurious in the reported cases.

We agree with the author in his explanation of the solidification of the contents of the aneurismal tumour, viz.—that the interior of the sac is gradually filled by the deposition of concentric laminæ of fibrine: that the fibrine in such cases is deposited in regular concentric laminæ, the oldest or first formed next the sac, those most recently formed nearest the centre; and that any obstruction to the current, by which its velocity and amount are diminished, will accelerate the deposition of fibrine in the aneurismal sac. That such is the case when the ligature has been applied, as well as when the force of the current has only been diminished by means of compression of the artery, we think probable from the fact, that in the generality of cases, after the application of the ligature, a feeble current and pulsation may be detected for some days after the operation, by the hand placed over the aneurismal tumour. We think this theory probably more correct than that which assigns the solidification of the contents of the aneurismal tumour to the sudden coagulation of the blood contained in the sac, and absorption of its fluid contents: and we agree with the author in the proofs which he has adduced, "that the ligature at a distance from the sac effects the cure of aneurism in the same way as compression."

The result, then, in either case is the same, the consolidation of the aneurismal tumour by means of the deposition of concentric laminæ of fibrine within its walls. The manner in which this is effected, would in both cases appear to be precisely the same; and the question remains to be decided, by which means, compression or the ligature, this object can be obtained with the greatest safety and celerity, and the least amount of suffering and inconvenience to the patient.

A more extended experience, particularly of the results of compression, will be required, before those surgeons who have been so successful in the application of the ligature, are likely to be induced to abandon an operation which they have been led by experience to look upon as unattended by danger, and so satisfactory in its results.

Part Third.

PERISCOPE.

SURGERY.

CASE OF SUBMAMMARY ABSCESS DISCHARGED THROUGH THE TRACHEA. By S. C. SEWELL, M.D., &c., Montreal.

MME. A—— D——, aged twenty-nine, ten years married, barren, consulted me early in March 1840, for a painful affection of one breast, which I treated as a case of irritable breast, by frictions of iodide of potassium ointment, ioduretted solution of iodide of potassium, &c. In three weeks she expressed herself much relieved, and dispensed with further attendance. On the 20th April, Mr. D. called on me, and stated, that some uneasiness still remaining, and the breast having increased in size, Mrs D. had consulted Dr ——, who had declared it to be cancer, and advised its removal. I immediately went to see her, and, on examining the breast, I found that it was somewhat larger than the other, with fluctuation, indicating matter or other fluid behind the mammary gland. I repudiated the idea of cancer, expressing my conviction that it was a chronic abscess between the ribs and gland, and requesting that further advice might be called to decide the controversy. They suggested Dr Robertson, who came on the following day and confirmed my diagnosis. Owing to the depth of the abscess, we declined making any incision, and the ioduretted frictions were continued. On the 10th of May, I was hastily summoned to see Mrs D., who, I found, had commenced about an hour before to expectorate pus in considerable quantities. The expectoration was now less abundant; every few minutes a tracheal rale was heard, followed by cough and expectoration of pus. The breast was now the size of the other, and the gland moved loosely under the hand, as though detached. Pressure on the breast caused increased expectoration of matter. I applied a compress and bandage over the breast. The expectoration gradually ceased in four or five days; and Mrs D. has remained in good health to the present time. In this case, adhesion of the pleuræ, perforation of the intercostal muscle, pleuræ, and substance of the lung, until a large bronchus was reached, must have taken place. I should have mentioned that there was no effusion of pus into the pleura or emphysema of the cellular tissue; but there was loud mucous rale à grosses boules under the abscess.—*The British American Journal of Medical and Physical Science*, 1st July 1847.

SEPARATION OF THE ENTIRE HAIRY SCALP FROM ERYSIPELAS, FOLLOWING AN OPERATION IN THE PRACTICE OF M. VELPEAU.

A MAN was received into La Charité in April, under the care of M. Velpeau, for a malignant tumour in the parotid region; he was one of the first who had been etherized, and the operation for the removal of the tumour gave him no pain, although he said, at the time, he had *heard* every stroke of the scalpel. For some days after the operation he continued in the most satisfactory state, but, from some imprudence on his part, the side of his face opposite to the wound became affected with erysipelas, which soon extended to the temple and the whole of the scalp, and an abscess formed which covered the entire roof of the cranium; his tongue was dry—the swelling was considerable, and the pain extremely severe; he became delirious, but strange to say, the pulse remained

tranquil in the midst of all, and he seemed to be quite free from any thing that could be called fever. Several incisions were made in different points of the scalp, which gave issue to a large quantity of pus, and the entire of the scalp was found to be unadherent to the bone; it speedily fell into mortification through its whole extent; it separated at the anterior part, and, by sloughing and contraction, exposed the bone to a great extent, leaving the top of the head one wound. One would have expected a fearful exfoliation of the cranium, but there was only one little spot in which this took place; granulations soon covered over the bone, and the enormous ulcer quickly and kindly healed. The restored scalp now has a wrinkled appearance, as if it had been too large, and was plaited in to fit it to the head. After suffering a little from diarrhœa, night sweats, &c., he was discharged cured.—*Jour. de Med. et Chir.*

JÄGER'S STATISTICAL RESULTS OF THE OPERATION FOR CATARACT.

BETWEEN the years 1827 and 1844, Professor Jäger operated on 1011 cases of cataract, of which 764 were lenticular, 207 capsulo-lenticular, and 40 capsular. The kinds of operations to which he had recourse were as follows:—

Extraction by the superior section in	-	Cases. 728
„ by the inferior „ „	-	9
Partial extraction „ „	-	58
Depression „ „	-	129
Breaking downs the lens „ „	-	87

1011

Of the above number 63 lost their sight; and it will be seen by the sub-joined table, what were the processes employed which gave the worst results:—

Of the 58 operated on by partial extraction,	-	3
„ 737 „ „ by extraction	-	33
„ 87 „ „ by breaking down the lens	-	6
„ 129 „ „ by depression,	-	21

63

The proportion of those who lost their sight being, in the operation of extracting, $4\frac{1}{2}$ per cent., in depression, 16 per cent., and in breaking down the lens, 8 per cent.—*Behandlung des. gr. Staares. Vienna, 1845.*

CASE OF POPLITEAL ANEURISM CURED BY COMPRESSION IN FOUR DAYS.

THE following is one of the most encouraging cases which has been yet recorded, for the treatment of popliteal aneurism by means of compression.

A man of thirty years of age, of stout make, but not very healthy aspect, was admitted into Steevens' hospital on the 14th April last, under the care of Mr Cusack. An aneurism existed in the right ham, about three inches in length, and its breadth limited on each side by the hamstring tendons. The disease had originally been perceived two months previously, at which time, while walking, he suddenly felt "something give way" in the situation of the right popliteal artery, and on examination a pulsating tumour was found to exist of the size of a pigeon's egg. On his admission into the hospital he complained of little more than an uneasy stiffness about the knee. The collateral branches around the knee-joint were very large, particularly one which crossed the internal condyle, fully equal in size to the radial artery.

After a few days' rest in the horizontal posture, during which time the patient took ten drops of tincture of digitalis thrice a-day, pressure was made by a suitable instrument on the femoral artery as it passed over the pubes. A degree of pressure was exercised, sufficient only to diminish, without entirely interrupting, the current of blood through the vessel. When the pressure became

painful, the compressing pad was shifted an inch and a half lower down on the artery, and by alternating the pad upon these two points, uninterrupted compression of the artery was maintained.

The compression was commenced on the 22d of April. On the 24th, the tumour had increased greatly in solidity, and the pulsation was scarcely perceptible. Compression was now augmented, so as to remove altogether pulsation from the tumour; and on the 26th, on taking off the instrument, pulsation was found to have ceased entirely.

He was kept in hospital for a month afterwards, when he could walk very well, and flex the knee perfectly. The tumour in the ham was still to be felt, but hard and greatly diminished in size.—*Dublin Quarterly Journal of Med. Science*, August 1847.

PATHOLOGY AND PRACTICE OF PHYSIC.

ON THE PATHOLOGY OF INSANITY. By DR B. H. BERGMANN of Siegwart.

WE have been for some time hesitating as to the propriety of placing the observations of Dr Bergmann before our readers. Doubtless many will consider them as mere fantastic speculations. The position, however, which our author occupies as the chief physician of one of the most celebrated continental asylums, the great labour he has devoted for the period of thirty years to these researches, the accuracy of his former investigations into the anatomy of the brain (as witnessed by Valentin. See Soëmmering, tom. iv.), and lastly, the scanty amount of information to be found elsewhere on this important subject, have determined us to crave indulgence for the present attempt at placing before the English reader the substance of his communication.

No true mental derangement exists without organic disease. During its course the organs primarily affected, and from which, in the great majority of cases, the reflex action upon the brain is derived, become almost passive, so as to be overlooked by most physicians. Further, the mind in itself can be in no way affected, its manifestations only become anormal in the same manner that the most skilful musician is unable to play on an instrument which is defective or out of tune. It is to organic disease that paroxysmal returns must be attributed. Season, weather, and moral influences affect the part previously disordered in the same manner as in the rheumatic, arthritic, and other similar diseases.

As regards the organic localization of different mental affections, unwearied investigation in men and animals have induced our author to arrive at the following conclusions. Before these can be understood, however, it will be necessary to refer to Dr Bergmann's previous discoveries in the anatomy of the brain.

According to the author, between the crura fornicis and the inner prominence of the thalami optici, there are to be observed in the foveola triangularis (situated between the corpora striata and thalami optici on the passage into the middle cavity) some striæ, scala minima or triangularis. Further, between the foveola triangularis, the prominent inner border of the thalamus, and the beginning of a prominence running along its upper border, and constituting the commissure of the glandula pinealis, are to be seen the chordulæ contortæ, which unite themselves with the scala minima by means of a propangula proceeding from the latter. Again, on the inner side of each thalamus, before the white commissure and above the entrance in the infundibulum, there exists the fascis spicarum, composed of from seven to eight medullary filaments. Between this fascis and the anterior commissure, modulating striæ are seen running from above downwards, and in close connexion with the fascis, the so-called flucticuli undæ. Lastly, on the inner surface of the tha-

lami, behind and below the point of insertion of the soft commissure, there exists a third bundle of fibres, the *fasciculus radorum*. Certain filamentous radiations running on the anterior border of the lamina cornea, upwards, against the cornea, are called *flabella*. Those running from the lower end of the same lamina, towards the middle lobe of the brain, are designated *penicilli*. On both sides of the anterior angle of the fovea rhomboidalis are situated the *chordæ volubiles* or *tortuosæ*, which, on the under portion, pass into *chordæ verticillatæ*. One of these, which extends more inferiorly, and runs on each side inwards, is called *conductor sonorus*. In the middle of the fovea rhomboidalis are to be seen the *striæ transversæ* or *fossiculi acoustici*, running on each side from the inner and middle space outwards. Longitudinal *striæ* are intermingled with them about their centre, whereby they acquire a plexus-like aspect, and receive the name of *scala rythmica*. Before and on both sides of the lingula, there is a very delicate medullary filamentous tissue, the so-called *tela filipendulina*. The base or bottom of the fossa Sylvii is formed by different organs, named by Bergmann, *Conus*, *Polychord*, and *Sistrum*. The *lyra veli anterioris* is situated behind the velum, and shows a very delicate filamentous ramification, resembling in shape the lyra in the foveola of the lamina cornea. —See “*Neue Untersuchungen über die innere Organization des Gehirns*. Hanover, 1831.”

In congenital imbecility there is to be observed a peculiar shortening of the occiput, indicating atrophy of the posterior cerebral lobes, the full development of which must be considered from numerous observations as a differential sign of the human body from the rest of the animal kingdom. It is by this higher organic degree of the human organism also, that the erect position of man is obtained. The *canalis Sylvii*, the *fistula sacra pneumatica* of the ancients, is the centre of all cerebral functions, and there is no true chronic mental disease to be met with without dynamic and material affection of this cerebral focus. On the face of, or anterior to this centrum is situated the phase of intelligence, the physical vehicle of which is the system of the fornix, or of optic life (*lichtleben*), comprising the *cornua ammonis*, the *quadrigenina*, the great trigonon, with the delta and its lines. Posteriorly to the *canalis Sylvii* there is the phase of the mind, the physical vehicle of which is to be found in the musical life (*tonleben*). Of the utmost importance and necessity to the life of the soul are also the wonderfully fine and remarkable chordal systems (the *striæ* of different parts of the brain), each of which possesses its determinate dignity, as is ascertained by an attentive comparison of them in the animal kingdom, and a careful investigation of their pathological conditions in man.

Amentia and dementia, if incurable, are always occasioned by organic disease of the central organ below and on the side of the pineal gland, and of the fornix and posterior cornua. Organic alterations of the fornix and cornua ammonis are constantly followed by alteration of physical motion and mental activity. Organic disease of the posterior horns (*cornua copiae*) and *colliculi*, are indicated by disturbed intelligence; the same happens when the trigonum magnum, or true fornix (an organ peculiar to the human species), is affected. Deficiency or absence of the lamina cornua, flabella, and penicilli, produce alteration of the conception of objects and of general memory. Atrophy of the flabella is constantly attended with a diminution of the memory, especially of names; atrophy of the *scala triangularis* with that of the sense of place, and atrophy of the *fascis spicarium* with more or less loss of visual perception. Disappearance of the acoustic fibrils is not followed by a loss of hearing, but by that of proper understanding and comprehension. Defect of the *tela filipendulinæ*, produces an absence of passion and strong desire. Degeneration of the trigonum pendulum and molle, and disappearance of the *conus*, *polychord*, *gnomon*, or *sistrum*, is followed by complete nullity and vacuity of mental life.

In epileptic patients congestion is found around the pineal gland, the sylvian fossa, plexus choroides, base of the brain, and region around the tuber cinereum.

At a later period, and after repeated attacks, the congestion becomes diffused also over the plexus medius of the fornix, the glandula pituitaria, quadrigemina, and velum anterius. Hence why aberrations of the mind following epilepsy are almost always incurable.

As regards causes, the most important moral ones are fright, offended honour, and, above all, unfortunate love, which act on the centre of conscience, namely, the trigonum pendulum. Amongst the organic predisposing and occasional causes must be numbered many disorders. Tuberculization, hepatization, and other diseases of the lungs, induce stases, congestions, adhesions, and hypertrophy of the cerebral membranes, especially in the plexuses, the Sylvian cavity, foveolæ, around the chiasma, on the velum anterius and posterius, in the foveola rhomboidalis, and especially around the pineal gland. At the same time the medullary parts, especially the epithelium and chordal system, become atrophied. Diseases of the heart, more particularly hypertrophy, may give rise to mental affections by excessive determination of blood to the vascular plexuses of the brain. Scabies improperly treated, and gout, often produce cerebral metastasis, the latter especially of metastasis to the lamina cornea. Scarlet fever, continued headache, congestion with giddiness, hypochondria, spasmodic affections, and hysteria, strongly predispose to mental alienation. In some cases the cause of catalepsy was found in the fornix. Menstrual and puerperal disorders frequently lead to mental disorder. Syphilis and onanism are also frequent causes, although the author does not think that there is any especial connexion between the genitals and cerebellum.

All the causes now mentioned are metastatic. Previous to these organic changes in different organs passing into a mental one, there is always observed, according to the author, the appearance of a moral passion. Hence the striking self-will and obstinacy, foolish pride, hate, envy, and similar manifestations, must be regarded rather as consequences than as causes of the chief disease.—(*Hannoverische Annalen*, H. 6, 1846.)

DR CHARLTON ON THE TREATMENT OF DROPSY AFTER SCARLATINA.

“We have described the dropsy as coming on in two different ways. In the one its invasion was intense and sudden, in twenty-four hours the body was distended with fluid, there was high fever, and a tolerably full, but very rapid pulse, while the urinary secretion was almost entirely suppressed. In such cases free general venesection was found to be the most efficacious remedy, and it could the more easily be employed, as these intense attacks of dropsy usually supervened after the mildest forms of the exanthema, where the patient's strength was quite equal to bear the loss of blood. In illustration of this mode of treatment, we may subjoin a brief extract from the letter of one of our most valued correspondents. ‘The treatment depended much on the nature of the case. If the anasarca was great, with much oppression of the breathing, high fever, and rapid but tolerably firm pulse, I found one good general bleeding to be followed by the most beneficial effects. This was succeeded by the administration of a mixture of the nitrate of potass, liq. ammon. acet. and tart. antimon., in moderate doses every two hours, with calomel and compound jalap powder at night, and the latter repeated in large doses in the morning.’

We ourselves occasionally had recourse to the treatment recommended by Dr Golding Bird. The patient was kept in bed, wrapped in a large flannel bed-gown, hot baths were given every night, and he took the following mixture every four hours:—

R. Vin. antimon. potass. tartr. m. x., julep. ammon. acetat. oz. iii., syrup papav. m. x.; with pulv. ipecac. comp. gr. iiii and pulv. hydrarg. c cretâ gr. v. every evening.

Elaterium and other drastic purgatives were likewise employed with success, as was also Croton oil.

In the other form of dropsy, where the swelling came on gradually with but little fever, we had recourse to nearly similar treatment, only that venesection was not required. But sometimes most dangerous head symptoms unexpectedly supervened, or the respiratory organs were severely affected. Nothing was then found more efficacious than the free employment of drastic purgatives. It was surprising to see how well they were borne, and how little debility was induced by the copious evacuations they occasioned. The favourite remedies of this kind were croton oil and elaterium, especially the latter. Whether the fluid had accumulated in the pericardium, the pleura, or the peritoneum, or whether it had invaded the cellular tissue throughout the body, elaterium, in doses of from one-twelfth to one-sixth of a grain every three or four hours, often brought about the most rapid and unlooked for amendment. We saw one child who had been in the most dangerous condition from dropsical effusion and pneumonia, and who regained his health in three days from taking small and frequently repeated doses of elaterium, squills, and calomel. He was in the most debilitated condition before these remedies were given; but stimulants, chiefly gin and water, were freely administered during the operation of the drastic purgatives, and with the happiest effects.

A correspondent in the neighbourhood of Newcastle informs us, that one child of five years of age, whose death was hourly expected from effusion of fluid into the thorax and abdomen, was *emptied* in a single night by taking one-twelfth of a grain of elaterium every three hours, and from that time rapidly recovered.

Some of our friends used the iodide of potassium to facilitate convalescence from dropsy, after diuretics, &c., had been freely given. They consider their patients to have derived much benefit from this preparation. Often when patients escaped the perils of the anasarca, they remained pale and leucophlegmatic, and seemed as though about to succumb to scrofulous disease. Many did indeed suffer in this way, swellings formed in the joints, or abscesses burst in the neck, and discharged the matter so characteristic of scrofula. Here the preparations of iron were of especial advantage, particularly the citrate and the iodide of that mineral. Lastly, change of air was, as it ever is, after the exanthemata, peculiarly beneficial in restoring the patient to florid health.

We are not quite sure that diuretics were always useful in the dropsy; they seemed sometimes to increase the already existing congestion and irritation of the kidneys. Leeches applied over the situation of these organs seemed to succeed better.

Where small but numerous abscesses threatened the life of the patient after scarlatina, we ever made it our principal object to support the strength by wine and nourishing diet, and to correct the tendency to scrofulous disease by the administration of the iodide or citrate of iron. It is needless to observe, that, to secure ultimate success, the utmost attention to personal cleanliness, and to due ventilation of the apartment, was required."—*An Account of the late Epidemic of Scarlatina in Newcastle and its Neighbourhood*. By Edward Charlton, M.D., Edin. Newcastle, 1847.—A pamphlet that we recommend to the perusal of every practitioner in our large towns.

DR Baly ON THE TREATMENT OF ACUTE SIMPLE DYSENTERY.

"MILD aperients have been found most useful in perhaps the majority of the cases, but especially in those where the lower part of the large intestines was the seat of the disease, and where the fæces were retained, so that the evacuations consisted almost entirely of mucus tinged or mixed with blood. In these cases the administration of one, two, or three drachms of castor oil has produced free feculent evacuations, and has afforded great relief to the symptoms. The beneficial effect produced by the action of this mild aperient medicine has often been very remarkable, greater than I know how to explain; while evil seemed always to result from allowing more than twelve hours to pass without

obtaining a free, feculent evacuation, if the frequent discharge of bloody mucus and other signs still indicated the persistence of inflammation. In many cases, however, no aperients whatever were required, free discharge of the feculent contents of the bowels taking place spontaneously. Indeed, where the cæcum and ascending colon were the seat of the disease, the active stage of the inflammation was almost always attended with copious discharges of liquid fæces, and the subsidence of the inflammation was in great measure evidenced by the less frequent occurrence of these discharges, and their less liquid character.

Very rarely indeed was a more active purgative than castor oil found requisite in the acute stage of dysentery. In some instances, owing to the irritable state of the stomach, small doses of saline purgatives have been substituted for the castor oil; but whenever they have acted with much violence they have seemed to do harm. I have, indeed, seen nothing to encourage me to a trial of the plan of treating dysentery by active purgatives, recommended by some writers who have practised in India.

At one time I thought the use of even local blood-letting might in a great measure be dispensed with, and that the inflammation in the large intestine might be wholly subdued by means of calomel, administered in doses of five, ten, or fifteen grains, combined with more or less of opium, according as there was a disposition to the retention of the fæces, or to the discharge of frequent, liquid, feculent evacuations. And assuredly the disease was often very speedily arrested by this plan of treatment; the system being quickly brought under the mercurial influence, and the general as well as the local symptoms of inflammation disappearing as soon as the dark-green discharges produced by the calomel took the place of the bloody and mucous, or of the very liquid, though feculent, evacuations. I have subsequently, however, seen reason to prefer the more moderate administration of calomel, aided by free local depletion, as a safer and indeed more sure method of reducing the inflammatory action. In the most severe cases of acute inflammatory dysentery occurring in the Millbank prison, one or two large doses of calomel are still given at the commencement; but afterwards the administration of this remedy is continued in smaller doses. I have relinquished the use of large doses of calomel as the general mode of treatment, not only because it was uncertain in its effects on the disease, and because the ptyalism produced was sometimes very troublesome, but also because the violent mercurial action, in some cases, seemed to cause serious injury to the system, and to lead to the development of tubercular disease. I should here remark, too, that calomel, even in small doses and combined with opium, has in some cases increased the irritation of the large intestine; and that great advantage has then resulted from substituting for the calomel the grey mercury with chalk, of the *Pharmacopœia*, in combination with Dover's powder.

The local blood-letting, the calomel and opium, and the mild aperients, were the remedies to which the physician looked for the cure of the disease. But there were other remedial means from which the patients in many cases derived more sensible relief from their sufferings. To those means, however, of which the principal were opiate enemata to relieve the tenesmus, large warm poultices to the whole abdomen, and medicines to allay symptomatic vomiting, I can merely thus briefly allude. I must however remark, that whenever, from the age or debility of the patient, it appeared desirable to give moderate quantities of a stimulant, such as brandy, no harm ever resulted from its use. On the contrary, a beneficial effect was produced, however active the local inflammation might be. It may be thought that this was owing to the prisoners having been weak from long-continued confinement. But there was no strongly marked sign of such weakness, and, when general blood-letting appeared to be called for, it was borne well. Indeed, the principal reason for preferring local blood-letting, even in severe cases, was, that it seemed to be more efficacious, and not that any symptoms contra-indicated general depletion.

I need scarcely say, that the diet allowed in the earlier stage of dysentery

consisted merely of milk and farinaceous articles of food.”—*Dr Baly's Gulstonian Lectures on the Pathology and Treatment of Dysentery.*

DR J. F. DUNCAN ON THE TREATMENT OF HOOPING-COUGH.

DR DUNCAN is of opinion that whooping-cough ought to be classified among the exanthemata, and ought to be treated upon principles analogous to those which are found to answer in other members of the same group. Thus he says:—

“The patient should be confined to his room, if not to bed, and put upon low diet; gentle aperients and diaphoretics administered until the affection has had time to run its course; after which, if the disease do not subside of itself, as it probably would in most cases, the remedies of a tonic and antispasmodic nature, which are found so beneficial in chronic cases, ought to be employed. Of course I speak here only of the simple form of whooping-cough. Instead of this, how common is the practice to allow the patient freely out into the open air, especially if the symptoms begin moderately, and the weather is mild, thereby interfering with the proper course of the affection, and running the risk of complicating the case by some of the more dangerous forms of pulmonary inflammation. This habit is unfortunately encouraged by the popular opinion, that nothing is so beneficial in whooping-cough as change of air; which leads some persons from the first, and many others at an early period of the complaint, to resort to this expedient; whereas it is well known to competent practitioners, that, however beneficial change of air may be in protracted cases, the premature adoption of the practice is calculated to retard rather than promote recovery.

In the same way false notions have led to similar errors, in reference to diet and regimen, during the early stage, that would have been avoided had the opinion here advocated been adopted. Because the child has a good appetite, and is not sick, he is allowed by many persons his usual food, and those evacuations which would have the effect of lowering the fever, and making the disease both milder and shorter, are generally neglected. Antispasmodics, too, are frequently administered before the cessation of the fever, and, consequently, do more harm than good. Finally, we can understand how it was that so much benefit used formerly to be obtained by stimulating embrocations to the back, the efficacy of which I can readily attest from several instances in which I have used them, but which, I believe, have fallen into general disuse from the abandonment or overthrow of the theories upon which their adoption was recommended. Their utility depended, when judiciously applied, not upon any effect they produced upon the spinal or other nerves, for these were not diseased, but upon the tendency they had to diminish the congestion of the bronchial glands, in the same manner in which similar frictions are used in mumps to reduce the tension and uneasiness of the parotids.”—*Dublin Quarterly Journal of Medical Science*, August 1847.

PROFESSOR PAGET ON FATTY DEGENERATION OF THE HEART.

“THE most common form is that in which you find, on opening the heart, that its tissue is in some degree paler and softer than in the natural state, and lacks that robust firmness which belongs to the vigorous heart. But what is most characteristic is, that you may see, especially just under the endocardium, spots, small blotches, or lines, like undulating transverse bands, of pale, tawny, buff, or ochre-yellow hue;—thick set, so as to give at a distant view a mottled appearance. These manifestly depend, not on any deposit among the fasciculi, but on some change of their tissue. For, at their borders, you find these spots are gradually shaded off, and merging into the healthy colour of the heart; and when you examine portions of such spots with the microscope, you never fail to find the fatty degeneration of the fibre. In the least degree of the degeneration, you may see only minute black dots within the sarcolemma,

partially obscuring the proper substance of the fibre; but as these dots grow larger, so their centres grow bright, and they manifest themselves as particles of oil, which, still increasing both in size and number, may at length almost completely fill the sarcolemma, or even may, in some degree, distend it. They may be irregularly scattered, or may lie in longitudinal transverse rows; and various appearances, such as it would be tedious to describe, may be produced by the various sizes of the oil particles, and by their lying in different focal distances, so that some may appear like black dots, and others may present the many aspects which, according to their distance, we find in any specimen of minute drops of oil. The main fact is, that in portions—even, taking them altogether, considerable portions—of the heart, the proper muscular tissue is gone, and in its place you have tubes full of fatty matter, therefore powerless, wholly incapable of exercising the proper function of the muscular fibre.

In regard to the pathology of the disease, it is important to remember that it is, from the first, an affection of the fibres themselves; it is not the result of an encroachment of the natural fat of the heart upon them, although it sometimes, yet I think seldom, exists together with increase of the external fat of the heart. Its chief seat—near the inner walls of the heart's cavities, and on the fleshy columns, where is little or no fat in the natural state—would prove this; and the microscopic examination leaves no doubt, for you may not find more than the usual quantity of fat outside the fibres. The fat is nearly all within them, and the paleness, or complete absence of the nucleus, declares the nature of the case. Neither is it at all necessarily connected with general fatness of the body.

But, besides this yellow spotting, Rokitansky also mentions, but too briefly, another appearance produced by fatty degeneration of hypertrophied and dilated hearts, which has for some time past engaged the attention of both Dr Ormerod and myself as an exceedingly important affection in hearts which appear not previously or otherwise diseased. It is to this form, since it appears desirable to distinguish it by some name, that I would apply the term of "granular degeneration" of the heart,—indicating thereby both a general granular aspect seen with the naked eye, and the granular appearance which the fibres, when examined with the microscope, derive from the crowds of little oil particles within the sarcolemma.

In hearts thus diseased, there is no increase in the quantity of fat on the exterior, and the whole organ has its natural size, shape, and general external appearance; but it feels soft, doughy, inelastic, unresisting, and may be moulded and doubled-up like a heart beginning to decompose long after death: it seems never to have been in the state of *rigor mortis*. These appearances are more manifest when a section is made through the wall of the left ventricle. Then, if the wall be only partly cut through, the rest of it may be very easily torn, as if with separation of fibres that only stick together; and the cut surface of the wall looks, as it were, lobulated and granular, almost like a piece of soft conglomerate gland—an appearance which is yet more striking when observed with a simple lens of about half an inch focus. In colour, it has not on its surface, much less on its section, the full ruddy brown of healthy heart,—a colour approaching that of the strong voluntary muscle,—but is, for the most part, of a duller, dirtier, lighter brown; in some parts gradually blending with irregular marks, or blotches, of a paler fawn colour.

When a portion of the muscular wall (let it be cut however it may) is dissected in the ordinary way, or even very gently, with needles, for the microscope, it is found that all the fibres are broken into short pieces, some twice, some five or six times, as long as they are broad. The broken ends of these short pieces are usually squared; but some are round, or irregular, or cloven, and broken off lower down. The pieces are almost always completely separated, having no appearance of even cohering at their sides, and they lie scattered disorderly.

In some pieces, the transverse striæ are still well seen and undisturbed, ap-

pearing quite as in health. In more, they are interrupted or obscured by dark particles, or glistening particles with shady black margins, like minute oil particles scattered without order in the fibre. Where such particles are few, they appear to lie especially, or only, in contact with the interior of the sarcolemma; but, where more numerous, they appear to occupy every part of the fibre, leaving the transverse striæ discernible only at its margins, or even completely obscuring or replacing them, and making the fibre look like a gland-tube filled with dark granules and larger glistening dark-edged fat particles. Where these particles are very numerous in a fibre, they appear also generally larger, and more generally glistening and black-edged, like larger oil particles.

There are no oil-drops floating about, no fat cells, scarcely even any of the minute particles which are seen in the fibres, appear out of them,—the field of the microscope is perfectly clean. No morbid product, cyto-blasts, or others, can be seen as a deposit between the fibres, nor is there any apparent increase of cellular tissue.

As a general rule, the palest parts are most advanced in the disease; but even in microscopic portions some pieces of fibres appear hardly changed, while those all round them are completely granular.

Besides the characters assigned above, these diseased fibres differ from healthy ones, in that there are no nuclei lying among them as among healthy heart fibres, and that they rarely split in filaments, though often obscurely marked longitudinally.

Such is the fatty degeneration of the heart not otherwise diseased—an affection which seems of importance to us all, because it may fatally interfere with the ordinary progress of a case, whether surgical or medical.—*Medical Gazette*, August 6, 1847.

CONTRIBUTIONS TO THE PATHOLOGICAL ANATOMY OF THE INTESTINAL MUCOUS MEMBRANE IN THE INFANT AT THE BREAST. By FRIEDLEBEN & FLESCH, of Frankfort.

HITHERTO softening of the mucous membrane of the stomach and enlargement of the mesenteric glands have been usually regarded as the only causes of the acute or chronic diarrhœa, with atrophy, to which so great a mortality is due within the first year of life. This view our authors think they can disprove by exact post-mortem investigations. Softening of the mucous membrane of the stomach they regard as exceedingly rare, having met with it only in two instances, in one of which it proved fatal within twenty-four hours, while, in the other, there existed a fatal complication of intestinal disease. Diminished consistence at the fundus of the stomach was more frequently observed, but its origin was at all times secondary. They found enlargement of the mesenteric glands also to be uniformly secondary, of limited extent, and rarely of distinct character, except in the glands on the root of the mesentery. The enlargement of these glands could not have been felt through the abdominal walls, and hence it seems certain that hardened feculent masses had been mistaken by many for enlarged glands. Our authors indeed affirm, that even in the dissecting-room, after opening the abdominal cavity, the mesenteric glands cannot be felt enlarged until the intestines have been removed.

Before entering on the pathological appearances, our authors give a sketch of the normal appearances of the parts concerned in infants at the breast. The colour of the mucous layer, as well as of the subjacent mucous membrane itself, is either greyish-white or yellow, from the colouring principle of the bile, or more or less of a darkish livid colour, from imbibition of blood. It is to be remarked, however, that in this last case the mucous membrane is free from any arborization or punctuation. The mucous membrane adheres very closely to the subjacent tissue, so that, after being cut through, it can be detached only to a small extent. The mucous membrane of the colon, however, is less firmly adherent, owing probably to the greater amount of subjacent

cellular tissue—its consistence is always great enough to prevent it from being rubbed off by the finger or with the back of a knife—where the adhesion appears lessened without simultaneous diminution of its consistence, large pieces of the mucous membrane may be separated from the subjacent cellular tissue. The solitary glands, neither of the small intestine nor of the colon, are visible to the naked eye. As to those of the colon, however, we discover occasionally delicate roundish spots, presenting a contrast to the rest of the membrane by their opaque colour, but without any elevation. The villous layer, and the glands of Lieberkuhn, are also invisible. The Peyerian patches, on the contrary, may be discovered at the earliest period of life, when the intestine is held up to the light, owing to their greater opacity as compared with the rest of the membrane. Whenever these patches are recognised at first sight, a pathological state may be suspected. The largest of the patches are seated near the ilio-colic valve, where they may be discovered with a slight examination—the smallest are in the jejunum—those of the ileum are always distinguished by their marked limits. The number of the patches varies as much as from sixteen to thirty-six. The cases in which they are found to be only from four to six, are regarded by our authors as referable to atrophy of the mucous membrane.

In regard to the pathological states of the parts, we must restrict ourselves to the principal characters laid down by our authors.

I. CONGESTION.—1. *Of the mucous membrane itself.* A full red colour, varying from the rose to the cherry red, and excluding the violet and livid tints, is to be seen in little isolated spots, in patches of 1" 2", or even in arborescent trunks. Bifurcations of this colouring extend even over the normal patches. At the same time the adhesion of the membrane is not diminished—its consistence is. This congestive condition was seen alone, and also complicated with inflammation of the mucous membrane and its glands.—2. *Congestion of Peyer's glands.* Some of them are more readily visible than in the healthy condition, partly by their circumscribed redness, partly by the abnormal development of their areolar structure, the septa becoming more prominent; at the same time the solitary glands, both of the small intestine and of the colon, are also more developed, and more easily recognizable, without, however, projecting above the level of the mucous membrane. This derangement is without doubt one of the most frequent causes of temporary diarrhoea, occurring, in particular, at the time of teething.

II. ACUTE INFLAMMATION OF PEYER'S GLANDS.—1. *Primitive form.* About ten or twelve of these patches, as well elsewhere as near the ileocolic valve, exhibit at first sight a saturated cherry-red colour throughout, and a considerable elevation above the mucous membrane. The redness is sometimes limited to isolated spots, while, in other cases, it affects the border or the whole surface of the patches. Their consistence appears also to be lessened. Those of the lower parts of the canal are for the most part less affected, the progress of the alteration being from above downwards. The mesenteric glands at the same time appear elevated in most cases, and somewhat injected, but of normal consistence. Our authors in such cases found the mucous membrane itself and the solitary glands either quite healthy, or, if morbidly affected, certainly not in connexion with the alteration of the Peyerian patches. This morbid change was observed also by Rilliet and Barthez, but only in exanthematic fevers, and hence of secondary character, and somewhat analogous to the pathological states of typhoid fever. Our authors, on the contrary, found it in a primitive form, quite independent of any other disease, without any alliance to typhoid fever, but rather of true croupous character.—2. *Secondary Form.* Deep redness of Peyer's patches, with elevation, and in some spots even ulceration. At the same time, ulcerative inflammation of the solitary follicles of the small intestine.

III. CHRONIC INFLAMMATION OF PEYER'S PATCHES.—Several patches show a

decided grey-blue, or rather a slate-grey colour, strongly contrasting with that of the mucous membrane, the colour of which is usually pale, and in rare instances, owing to chronic inflammation, livid-grey. The areolar structure of the patches is not unfrequently rather opaque, their borders, however, being distinct, and studded with black punctuations. According to the stage at which death takes place, the tissue appears atrophied or elevated. In some cases, traces of inflammation of the mucous membrane and solitary glands were also observed; the mesenteric glands, on the contrary, were never essentially altered. Complicated tuberculization was never seen, but great tendency to complications in other organs, to lobular pneumonia, purulent effusion in the brain, &c., the cause of the fatal event. On this affection is founded Barri r's account of his "Diacrise Folliculeuse."

IV. ULCERATION OF THE SOLITARY GLANDS.—1. *Primary Ulceration*. A greater or less extent of the ileum presents roundish ulcerations of a size varying from that of a millet seed to that of a lentil, corresponding to the solitary glands; the borders are firm, prominent, and sharply defined; the base considerably excavated, and filled with a yellow exudation. These occur isolated, near one another, or even confluent. Tuberculous deposit was found neither on the ulcerative surface nor elsewhere. The solitary glands of the colon, Peyer's, and the mesenteric glands, in the cases observed by our authors, were normal or but accidentally diseased.—2. *Secondary Ulceration*. The difference between this and the primary form lies merely in the more intensely inflamed halo around the ulcerated glands. At the same time Peyer's glands seem to be constantly diseased, and both affections to be attended with tubercular deposits.

V. SOFTENING OF THE INTESTINAL MUCOUS MEMBRANE.—1. *Red Softening*. There is here lessened consistence of the mucous membrane, more frequently in that of the small intestine, less frequently in that of the colon. The adhesion of the mucous membrane to the subjacent tissue is at the same time diminished, this defect of adhesion occurring at points where the consistence is still unchanged. Along with these appearances the mucous membrane sometimes exhibited a species of œdematous infiltration, or a homogeneous pulpy and readily lacerable mass. The simultaneous alterations of the solitary glands, Peyer's patches, and the mesenteric glands, are of subordinate importance.—2. *White Softening*. This differs from the former only by the paleness of the membrane, and the greater extent of the affection, owing to its longer duration. The patches of Peyer are, in such cases, most readily visible, not in consequence of any change on themselves, but merely of the unusual paleness of the rest of the membrane. On the whole, this form of disease appears to be merely the termination of an inflammatory state of the mucous membrane, or the gradual change of the red softening into the white—in favour of which view there are analogies in the case of other organs, witness the brain—and this is the side of the question taken by Rilliet and Barth  z.

It is surprising that alterations so frequent and of such importance should have been till now so imperfectly understood. The French authorities have thrown them together, under the name of Entero-Colitis. But it is one of the most beneficial results of the investigation with which we have been engaged, that, with the exception of secondary acute inflammation of Peyer's glands, and secondary ulceration of the solitary glands, all the intestinal alterations in infants at the breast appear in most cases as idiopathic, and altogether independent diseases, without any simultaneous alteration of the other component parts of the mucous membrane. It is also very deserving of attention, that some of the alterations frequent in more advanced childhood, were never met with during the first year after birth—for example, membranous or aphthous formations, and independent ulcerations of the mucous membrane.

CONCLUSIONS.—1. Morbid alterations of the intestinal mucous membrane are a very frequent, probably the most frequent, *post-mortem* appearance in the infant at the breast. 2. When of a chronic character, these alterations lay the

foundation of atrophy; when acute, their most prominent effect is exhausting diarrhœa, which, from the cerebral symptoms often attendant on it, was mistaken by many observers for softening of the stomach (gastromalacia). 3. The frequency of the alterations above described, is as remarkable as the rarity of those which were once believed to be of common occurrence; for example, enlargement of the mesenteric glands, aphthous formations, gastromalacia, &c. 4. It is also to be remarked, that the diseases of the intestinal tract are in general far more frequent than those of the stomach, which, with the exception of softening of the fundus, are proportionally rare at this period of life. 5. The ulcerations of the mucous membrane, unless when secondary, are in most cases isolated. 6. The most frequent of the alterations under examination, is chronic inflammation of the Peyerian patches; and this is the only anatomical base of atrophy. 7. Next to this, but far more rarely, the red and white softening are essential causes of atrophy. 8. Red and white softening are but different stages of the same morbid process; the simple and gelatinous softening give but a difference of form. 9. In atrophy of infants at the breast, chronic ulceration of the solitary glands of the small intestines is among the rare occurrences. 10. The acute inflammation of the Peyerian patches, though hitherto little attended to, is among the most fatal of morbid alterations. 11. This is a true inflammation, as is attested by the attendant affections: for example, croupous lobular pneumonia. 12. This affection is generally unknown; and the few who are acquainted with it, commonly refer it to Dothineritis,—a disease which does not occur in infants at the breast. 13. Secondary acute inflammation of Peyer's glands, as well as inflammation of the solitary glands, belongs to tuberculization; in all such cases, tubercles of the spleen were met with simultaneously, but never in the intestinal tract. 14. The colitis of the French observers was often met with, but uniformly of small extent, and of subordinate importance, compared with the simultaneous, but far more serious, alterations of the mucous membrane of the small intestines. The mesenteric glands, with the exception of slight redness and elevation, in a few cases were, for the most part, normal; the alteration of these was never either considerable or prominent; in general tuberculization, it is true, they were infiltrated with tuberculous matter; but even then their enlargement could not be felt through the abdominal walls. 16. The peculiarity and frequency of the above described morbid alterations, and the absence of others, known to occur in the next year of life, as typhoid fever and abdominal tubercles, present features of great interest in the pathology of the alimentary canal during the first year of life.—*Zeitschrift f. Rationelle Medizin.* Bd. V. H. iii. Pp. 321, 367.

Part Fourth.

MEDICAL NEWS.

EDINBURGH OBSTETRICAL SOCIETY.

SESSION VI.

Proceedings of the Fifth Meeting, continued from p. 140.

CASE OF PHLEBITIC OR PUERPERAL OPHTHALMIA, read by *Dr Graham Weir*.—Mrs Cameron, æt. 37, (Henderson Row,) was delivered on the 5th November 1843, of her eleventh child, after a protracted labour and severe hemorrhage (placental presentation)—on the 8th, symptoms of uterine inflammation appeared, ushered in by severe rigors. The local pain was relieved by hot turpentine.

tine fomentations, calomel and Dover's powder, and tartar emetic. The constitutional symptoms, however, continued unabated. On the afternoon of the 14th, being the 9th day from her confinement, she had severe shiverings, and, shortly afterwards, was suddenly seized with a peculiar sensation, which she described as a rush of blood to her head, especially to the left side of it. The sensation was so distinct as to make her call her husband's attention to it. Soon afterwards, violent pain commenced in her head, particularly round the left orbit. She then remarked that she could hardly see with the left eye, or, as she expressed it, "there was a great dimness over it;" there was intolerance of light with both eyes, but more with the left, and scalding lachrymation. The integuments of the left orbit and cheek were considerably swollen and red—the conjunctival vessels were slightly congested—the iris was active, but from the great tenderness of the eyelids and the intolerance of light, it was found impossible to make a minute examination of the eye. The pulse was 120, and tolerably firm, and there was no abatement of the other feverish symptoms. Warm fomentations and poultices were applied to the eye. On the 16th, the pain in the head was slightly relieved, but the dimness of vision with the left eye was increased, and there was great tenderness in and around it. A dozen leeches were applied around the eye that evening, with considerable relief to the pain in both the eye and the head; and a mixture of hyoseyamus and camphor given internally. On the 17th, there was less redness of the conjunctiva, but several portions of it appeared to be stained of a pale yellow colour; there was copious lachrymation; and total loss of sight with this eye. The warm fomentations to the eye were continued, (while, for the uterine affection, she was ordered a mixture of camphor and nitrous ether, and a morphia draught at night).

On the 19th, the pain in the left orbit continued severe. There was an evident fulness of the parts immediately around the eye, especially at the upper and outer angle of the orbit, and the eye itself appeared to be slightly enlarged. The yellow colour of the conjunctiva had disappeared. The constitutional symptoms were much the same.

November 21st. (On making a careful examination of the eye, the following appearances presented themselves.)

The upper lid of the left eye very dark and discoloured from the application of the leeches, and both lids very much swollen. The conjunctiva covering the lower part of the globe, projected considerably between the lids, and hung out, as it were, in a sort of bag, filled with a transparent, whitish-coloured fluid, which seemed to be a mixture of serum and fibrin. The conjunctiva behind the upper lid was also in the same state, but a good view of it could not be obtained. At the inner canthus, the conjunctiva was covered with numerous red vessels. The globe appeared to be both larger than the other, and to be considerably pushed forwards. Great pain was complained of below the supra-orbital ridge. The iris, which in the right eye was of a bluish-gray colour, was, in the left, changed to a greenish hue, as if from the effusion of fibrin into its substance—it was fixed and immoveable, and the pupil was slightly irregular. Behind the pupil, a grayish opacity could be observed. Vision was completely gone.

The abdomen was very tympanitic—pulse 120, and feeble. Tongue clean, and bowels open. The projecting conjunctiva was punctured in several places, and a quantity of a transparent slightly yellow-coloured fluid evacuated. Warm fomentations were applied to the eye, and a small quantity of cusparia and wine given internally.

On the 22d the conjunctiva was again punctured, and some of the fluid examined by the microscope, but no pus globules could be detected in it. To take a pill of quinine and aloes, āā gr. ij. three times a-day.

Nov. 23d. The chemosis much diminished; both the sclerotic and the conjunctival vessels now very distinct, as was also the opacity behind the pupil. During the night she complained of a pain in the *right knee*, which was found

to be a little swollen and painful to the touch; and there seemed to be fluid effused around the joint.

Nov. 25th. The eye better, there being less vascularity and pain; but the globe still hard, tense, and projecting. (The eye improved as the knee became worse.) Knee more swollen, and fluctuation very distinct; veins of the leg not affected. Twelve leeches were applied to the knee, and the fomentations continued.

Nov. 28. The pupil more irregular and ragged looking, and, as it were, dragged from side to side. The globe had returned to nearly its natural size. Had been observed to squint frequently for the last two days, and had been frequently in almost a state of stupor. The knee was rather diminished in size, but was still painful, and the fluctuation distinct both above and below the patella. The abdomen continued tympanitic—pulse 100, feeble. Tongue clean, bowels open; knee again leeches.

Nov. 30. Chemosis returned, with dense effusion of serum below the whole conjunctiva, causing great projection of the globe, with much pain in and around it; the upper eyelid could not be raised. Less tension of the abdomen. The chemosis was again punctured with relief. A blister was applied to the knee, and another to the left temple—the quinine and aloes pills, and the wine, continued.

Dec. 2d. Much less tension of the abdomen, but the knee rather increased in size. The eye appeared to be nearly double its natural size, with considerable inflammation of both conjunctiva and sclerotic. Pupil very much contracted, with numerous adhesions to capsule of lens; lens itself of a whitish colour, and opaque. Iris pushed forwards—pulse 90, firmer. To give up quinine, and take pill Hydrarg. gr. ij. e. P. Opii. gr. $\frac{1}{4}$, three times a-day; to have a small quantity of steak and wine, and continue fomentations to eye and knee.

Dec. 7th. Better. Less inflammation and projection of the eye; iritis nearly gone, but pupil very irregular, with many adhesions; the iris and opaque lens are pushed forwards towards the cornea at its upper and inner part; no chemosis; knee rather diminished in size, but still painful; mouth slightly sore. To give up pil. hydrarg. and take quinine, gr. ij. three times a-day.

Dec. 10. Much better. There only remains slight scleratitis, but no perception of light; knee also better, since twelve leeches were applied to it two days ago; and there is less pain in it on pressure—pulse 80, firm.

Dec. 15. Two days ago the eye became worse, with considerable pain in the globe, extending to the side of the head and cheek, but with very little inflammation in any of the tissues of the eye itself; the knee continued to improve, but she had had shiverings the previous night. Twelve leeches were applied to the temple, and hot fomentations, and she was ordered a blue pill night and morning.

Dec. 18th. There is now almost no inflammation in the eye, and, on passing a lighted candle before it, she can tell when the hand is interposed between the eye and the flame. The knee is also diminished in size, and is less painful, and she can stretch it out a little better. After this date, her recovery was steady and uninterrupted.

Dr Burns had attended, two months ago, a woman who was delivered on the third or fourth day after being seized with fever. Uterine phlebitis came on, which proved fatal. A few days before her death the left eye became severely inflamed. The mother of this patient was seized with erysipelas of the face, and died in four days.

SIXTH MEETING.—*June 15th, 1847.* Dr SIMPSON in the Chair.

Dr Ebenezer Skae and Dr Buchanan were elected ordinary members.

CASE OF FATAL PUERPERAL ARTERITIS; PHLEGMASIA DOLENS OF THE LEFT UPPER EXTREMITY, &c.—*Dr Simpson* detailed the following case of partial placental presentation, which *Dr Beilby* attended along with him several years ago, and

where secondary inflammatory affections of a very unusual kind supervened, and carried off the patient five weeks after delivery. The lady, about a year before becoming pregnant, laboured under a very severe attack of rheumatic endocarditis. During the latter period of utero-gestation, she suffered greatly from attacks of difficult breathing, which amounted sometimes to orthopnoea. About the eighth month hemorrhage suddenly supervened, and Dr Simpson immediately ascertained, that, in addition to her other complications, she had the placenta projecting over the posterior lip of the uterus. After the os uteri was nearly dilated, the membranes having been ruptured some hours before, without suppressing the very severe and exhausting hemorrhage that was present, Dr S. extracted a child, who is still living, with the long forceps. The mother seemed for some days to be making a most perfect and satisfactory recovery. Some symptoms of irritation, however, supervened, and, during the second week after her confinement, Dr S. found, on making his morning visit, that there was no pulse to be felt in the right arm lower than the elbow, whilst it was distinct and strong down to that point. This forearm felt, at the same time, coldish, stiff, and benumbed. In the course of a few days, the pulsation in the right radial artery gradually but feebly returned, whilst the circulation, in the one and the other leg, seemed to be similarly affected. At last, unequivocal symptoms of erratic phlebitis began to show themselves, and, five weeks after delivery, ended in a fatal attack of phlegmasia dolens in the left arm and left side of the face. On opening the body, the *vena innominata* on the left side, and its large affluent trunks, were found entirely obstructed by coagulable lymph. The humeral artery, at the bend of the arm, was shut up by a coagulum; but the inner coat of the vessel had no appearance whatever of laceration, such as was seen in all the cases of spontaneous obliteration of arteries, so well described by the late Professor Turner, in the *Edinburgh Medico-chirurgical Transactions*, vol. iii. p. 105. The uterus was nearly of its natural dimensions, and did not present any traces of diseased action. The valves of the left side of the heart were profusely covered over with small wart-like excrescences. Was, Dr S. inquired, the obstruction of the artery, or arteries, in this case, produced by any mechanical cause, (as one of the vegetations separated from the cardiac valves), carried along, in the case of the arm for example, to the bifurcation of the humeral artery, and impacted there? Was it not rather the result of an original *puerperal arteritis*? Or it might be the effect of an effusion of coagulable lymph from phlebitic inflammation in the coats of the artery, a secondary phlebitic deposit upon the lining arterial membrane. Dr Parry, in his work on the arterial pulse, (p. 139), mentions an analogous case of local stoppage of the pulse in the arm of a puerperal patient. It took place two or three days after parturition, and was attended with coldness of the arm; but the power of motion remained. The other arm had lost all power of voluntary motion, but the pulse was distinct in it. The patient soon died, but a dissection was not obtained.

PUERPERAL CONVULSIONS CONNECTED WITH INFLAMMATION OF THE KIDNEY.—Dr Simpson pointed out the connexion of puerperal convulsions with derangement of the kidney as a very striking fact in Obstetric Pathology. He had seen *post-mortem* appearances of nephritis in some fatal cases of convulsions.

CASE I.—In this case, the patient, a delicate female, was exhausted by the pains of labour, and complaining of severe headache when the convulsions supervened. Dr Niven promptly and easily delivered the child, which was dead, by turning. The convulsions gradually subsided, but re-appeared several times. In the intervals she was profoundly comatose; and, in this state, she died about forty hours after the first attack.

Post-mortem Appearances.—When the lateral ventricle of the right side was opened, fluid blood escaped. The corpus striatum and outer part of the optic thalamus were broken up, and mixed with a large quantity of coagulated blood, forming a clot of large size. The fluid blood was found in the opposite

lateral ventricle, also in the third and fourth ventricles. The right kidney was converted into numerous cysts, of about the size of a walnut, containing unhealthy pus, which passed along the ureter and filled the bladder. The left kidney exhibited an advanced stage of Bright's disease.

CASE II. Dr S. lately saw with Dr Carmichael. The lady had so perfectly recovered after a labour which was quite natural, as to have been out at church, &c. Seven weeks, however, after delivery, after some sudden anomalous affections of sight and hearing for thirty or forty hours previously, she was seized with the most severe convulsions. Despite free evacuations, &c. &c., they continued to recur from time to time, and proved fatal in three hours; the patient during that time never being perfectly sensible. The pelvis of each kidney was filled with a whitish purulent-like matter, and its mucous lining membrane coated with large patches of adherent coagulable lymph, or false membrane. The ventricles of the brain were distended with serous fluid. The urine, when tested, presented no sign of albumen.

CASE III.—In a third case, one fit of convulsions came on a month before delivery, and recurred again in a severe and fatal form fourteen days after confinement. During the intervening six weeks she was free from any symptoms, and the labour was natural. The last attack came on suddenly in the evening, about nine o'clock; the convulsions were again and again repeated, and she died comatose in eight hours.

Dr MacLagan, Dr Handyside, and Dr Simpson had examined the urine during this last attack, but found in it no traces of albumen. On inspecting the body, some whitish turbid fluid was found in the renal pelvis, and could be pressed out abundantly from the renal papillæ. It looked like pus. On microscopic examination, it seemed to contain merely a very large quantity of epithelial cells, and no pus globules. Was this inflammatory? There was no effused fibrin or coagulable lymph.

PUERPERAL NEURITIS IN THE LOWER EXTREMITIES.—Dr Simpson directed the attention of the society to this as another not unfrequent, but neglected form of puerperal disease. He had seen several cases of it, and had found it mistaken for phlebitis and other forms of phlegmasia dolens. It was characterized often by numbness and tingling of the affected limb, and pain, fixed or remittent, passing along the crural or sciatic nerve, down to the knee, calf, or even the foot—increased by pressure along the course of the nerve, and by stretching of the limb, sometimes relieved by strong pressure on the highest portion of the nerves. Sometimes there was no coexistent œdema, or if it were present the pain was in a degree greatly disproportionate to the œdema. It was often very protracted in its course. After local leeching, an elevated position of the limb, the application of belladonna, aconite, &c., greatly relieved the patients.

Various members alluded to cases of this disease which they had seen.

ETHERIZATION IN LABOUR.—In addition to the inferences drawn from a former communication (see meeting of Feb. 20), Dr Simpson now added the following:—1. The state of etherization had little or no influence upon the fœtus; none at least of a deleterious kind,—the fœtal heart increasing only a few beats, if at all, when the mother was kept long and fully etherized, either during pregnancy or labour. 2. The mother, during labour, may be kept etherized, if required, for one, two, three, or more hours. Dr Simpson described two cases, in one of which the mother was about six, and in the other about four hours etherized, before the children were born. In both cases, the duration of the intervals, and of the pains before and during the etherization, was noted (as in the experiments which Dr S. had published on galvanism), and the etherization seemed to have no effect either on their frequency or strength. But, 3. In two or three cases, Dr S. had seen a very deep state of etherization modify apparently the full strength of uterine contractions, but they recurred immediately in full force when the patient was allowed to fall back into a state of

slighter etherization. 4. Dr S. had hitherto seen no traceable injury to either mother or child, from its employment, but the reverse. 5. The inhaler he used was either a concave sponge saturated interiorly with ether, and held over the face, or a simple portable flask, such as is represented in the accompanying woodcuts. The ether flask is a flattened ovoid metallic bottle, exactly resembling the common nursery bottle; perforated, like it, at the side with an aperture (2) to admit the ingress and egress of air, and with a moveable mouthpiece (*m*) attached to one end. The instrument is without valves. A plug fitted to a screw-socket fills accurately the lateral aperture (*a*) when the instrument is shut; when it is open and in use, the plug is unscrewed so that the aperture is freely opened. The following letters point to corresponding parts in each of the following figures; *a*, brass plug described above, having holes cut in its sides for the admission of air when unscrewed; *b*, body of inhaler; *n*, neck; *m*, mouthpiece; *t*, tapering tube $1\frac{5}{8}$ inches long, reaching into the cavity of the inhaler, and attached by a screw to the neck, *n*. This tube prevents the regurgitation of fluid from the cavity of the inhaler. Length of inhaler from 1 to 2 (see Fig. 1), $7\frac{1}{4}$ inches; breadth from 3 to 4 (Fig. 1), $3\frac{1}{4}$ inches; thickness from 5 to 6 (Fig. 3), $1\frac{7}{8}$ inches.

Figure 1 shows the inhaler shut up, and as carried in the pocket, leaving the mouthpiece (*m*) turned down on the body (*b*); the air-hole (*a*) closed, and a brass cap (2) screwed on the orifice of the neck. Ether may thus be carried in the interior of the instrument.

FIG. 1.

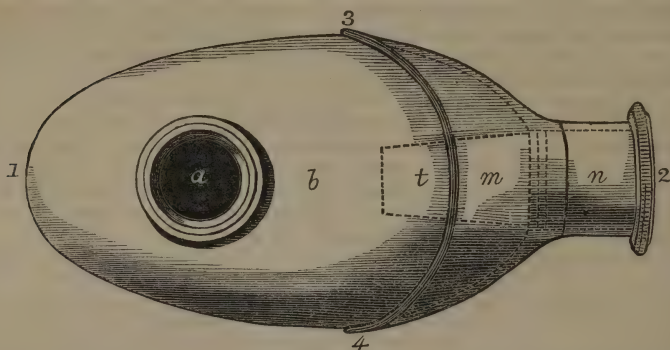


Figure 2 shows the inhaler, having the mouthpiece in its proper position, and the neck and the lateral air-hole (*a*) open.

FIG. 2.

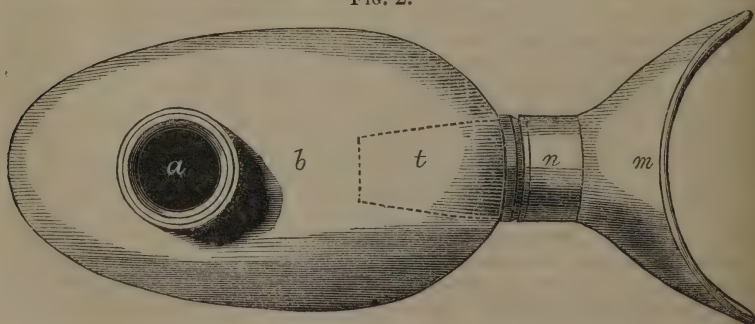
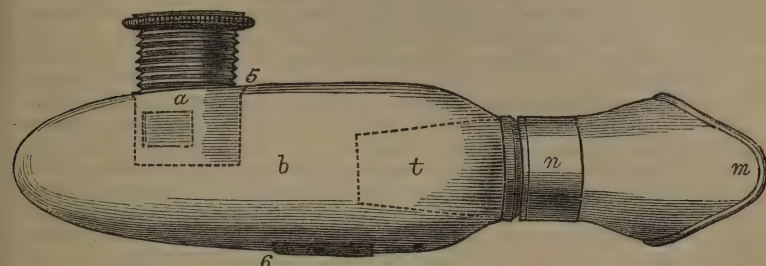


Figure 3 shows a side view of the inhaler when ready for use. The mechanism of the screwed ring and socket for admission of air (*a*) is distinctly seen.

FIG. 3.



CASES OF ANENCEPHALOUS BIRTHS.—Dr Keiller read a short paper on the subject of *anencephalous monsters*, and particularly referred to cases that have occurred in the practice of Dr Lyell of Dundee. Dr Keiller also produced a dissection which he had made of a similar case reported to the Society by Professor Simpson, and exhibited the sketches which accompanied Dr Lyell's report.

The cases were extremely analogous in kind, and differed only in the degree of deficiency in the brain and its osseous coverings. Spinal fissure existed in all the cases, and two of them (Dr Lyell's cases) presented, in addition to the cranial and vertebral malformations, good examples of *exomphalus*, the abdominal parietes being in both extremely defective.

Dr Simpson stated, that in his opinion, in anencephalous monsters, the malformation arises from intra-uterine disease, viz. from the bursting of the head when hydrocephalic. The brain is opened up and distended by fluid, so that it becomes gradually absorbed; and at length the enclosing membranes give way. The two small tubercles, always seen in anencephalous cases, lying on the base of the cranium, seem to be nothing else than the remains of the membranes, shrunk up, and almost obliterated.

SCURVY IN FRANCE.—DISCUSSION ON THE SUBJECT IN THE FRENCH ACADEMY OF MEDICINE.

At the meeting of the French Academy of Medicine, July 13, M. Scoutteten read an account of an epidemic scurvy which has recently attacked the garrison of Givet, to inquire into which he had been sent there by the minister of war. It was first observed on the 20th of May in the military hospital. It had attacked the soldiers of two regiments—the one infantry, the other cavalry, the latter being less severely affected. This scurvy presented itself under all the several forms of the disease, from the slightest lenticular rose-coloured spots to the most extensive ecchymoses. It was preceded by a typhoid epidemic, which had been very severe, and which, according to our author, appeared to be dependent on the same causes as the scurvy. M. Scoutteten made the following observations on the causes of this scurvy:—"The hospital is badly situated, being placed on the banks of the Meuse, surrounded with lofty trees, and so is low, moist, and shaded. We caused the patients to be conveyed to a fort on a neighbouring eminence, and the change for the better in every respect was immediate. Besides the insalubrity of the site of the hospital, it appears that there had been previously a diminution of the diet of the patients in general; it had been diminished more than a third, and in some companies to the extent even of one-half. M. S. considered this diminution of diet, to-

gether with the insalubrity of the hospital, to have been equally the source of the typhoid epidemic."

In the discussion which arose in the academy on M. S.'s communication, much was said of the causes of scurvy, and also of those of typhoid fever and typhus. Hardly sufficient weight seems to have been given in the discussion to the effect of insufficient diet, and M. Scoutteten appears to have had some restriction imposed on him as to the disclosure of the circumstances tending to the diminution of the diet in the regiments affected. M. Rouchoux insisted on the all-powerful effect of humidity in the production of scurvy. M. J. Cloquet held that scurvy is the effect either of humidity or of defective alimentation, and that either cause is of itself sufficient. This he illustrated by reference to the occurrence of scurvy among the French troops in Africa, his conclusion being that humidity is more efficient than defect of diet. M. Ferrus remarked that scurvy was far from unfrequent at the present day, being often met with in prisons and establishments for the insane; he had often met with it in the course of inspecting establishments of the latter description. He did not deny the influence of humidity and defective diet; he regarded the latter as an important condition of its appearance, but insisted that the disease depended on a multitude of causes. After insisting on the influence of debilitating causes of all kinds in the production of typhoid fever, scurvy, and erysipelas, he went on to say, that the great exciting causes of scurvy were humidity, defect of alimentation, and overcrowding in a narrow space. In mad-houses, he affirmed, there was too frequently a train of causes combined fit to produce scurvy,—overcrowding, a defective and too uniform diet, sedentary occupations often in persons before accustomed to employments in the open air, demoralisation, and the depressing influences attendant on confinement. He added his belief in the intimate connexion between the causes of typhus and scorbutus, more particular in armies. M. Bouillaud denied the identity of the causes of scurvy with those of typhoid fever; he thought there was not even an analogy between them; typhoid fever was to be seen at all times in the Parisian hospitals, while he had never seen a case of scurvy, though he had himself seen in twenty years more than 700 cases of typhoid fever.

BOOKS RECEIVED.

1. The Physiological Anatomy and Physiology of Man. By R. B. Todd, M.D., and W. Bowman, F.R.S., &c. Part III. London. 8vo. 1847.

2. The Preservation of Infants in Delivery, being an exposition of the chief cause of mortality in still-born children. By Richard King, M.D., &c. London. 8vo. 1847.

3. An Experimental Inquiry into the Functions of the Ophthalmic Ganglion. By C. Radclyffe Hall, M.D., &c. Part I. (From the Ed. Med. and Surg. Journal.)

4. On the Causes and Treatment of Abortion and Sterility; being the result of an extended practical inquiry into the physiological and morbid conditions of the uterus, with reference especially to leucorrhœal affections and the diseases of menstruation. By James Whitehead, F.R.C.S., &c. London. 8vo. 1847.

5. A Practical Treatise on Percussion,

or an exposition of the applications of this method of exploration to the states of health and disease. By L. Mailliot, M.D., &c. Translated with Notes by George Smith, M.D., Edinburgh. Madras. 12mo. 1847.

6. Hassall's Microscopic Anatomy. Part X.

7. Unhealthiness of London, and the Necessity of Remedial Measures. A Lecture, &c. By Hector Gavin, M.D., F.R.C.S.E., &c. London. 8vo. 1847.

8. The Chemistry of Vegetable and Animal Physiology. By Dr G. T. Mulder. Translated from the Dutch by Dr P. F. H. Fromberg. With an Introduction and Notes, by James F. W. Johnston, F.R.S.S.L. and E. Parts I., II., and III. Edinburgh. 8vo. 1847.

9. A few Remarks on the Expectant Treatment of Diseases. By ΑΚΕΣΤΗΣ. London. 8vo. 1847.

TO CORRESPONDENTS.

Communications have been received from Dr Peacock and Mr Lyon.

THE
MONTHLY JOURNAL
OF
MEDICAL SCIENCE.

No. LXXXII.

OCTOBER, 1847.

No. 16. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Case of Axillary Aneurism, for which the Subclavian Artery was Tied with Success.* By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

HAVING already placed upon record two instances of life being preserved, under very peculiar circumstances of Axillary Aneurism, by amputation at the shoulder joint, I have now the more pleasing duty of relating a case of the same disease, remedied by ligature of the artery without removal of the limb.

A gentleman, thirty-four years of age, from the north of Scotland, recommended by Dr Ross of Tain, applied to me on the 25th of July, on account of an axillary aneurism of the right side. It was of a large size, filling the axilla, and pressing forward the pectoral muscle, so as to be distinctly perceptible through the clothes. The patient stated, that about sixteen years ago he had fallen down a stair, and, by an involuntary effort to save himself, had seized the railing with his right hand, and consequently sustained a very severe wrench of the limb. With exception of some pain, and the ordinary uneasiness attending such an injury, he had not afterwards suffered any noticeable inconvenience further than an occasional difference of temperature in the hands, until about ten months ago, when he began to suffer from pain in the little and ring fingers, which gradually became almost constant and extremely distressing. More lately, the axillary tumour had attracted attention; and on the 29th, with the assistance of my friends, Drs Duncan and Mackenzie, I tied the subclavian artery, where it emerges from the scalenus anticus, by a single silk ligature, drawn with all the tightness

in my power. No inconvenience whatever was experienced—the ligature separated on the fifteenth day, and the patient at the end of another fortnight returned home, perfectly free from pain, and with hardly any perceptible remnant of the tumour.

In performing the operation I made an incision along the clavicle, so as to extend over the edges of the sterno-mastoid and trapezius muscles, and another from the centre of this upwards, parallel with the edge of the latter muscle. The dissection was conducted entirely by the knife and forceps. The needle was passed under the artery, with its convexity upwards, and the ligature was tied by the unaided effort of the fingers. It has been advised to pass the needle with its convexity downwards, or towards the clavicle, with a view to protect the vein from injury. But this vessel is not at all in the way, while the cervical nerves are so situated in regard to the artery, as in general to render it nearly, if not quite, impossible to convey the ligature from below upwards. It has also been advised to employ the assistance of some mechanical contrivance for tightening the knot. But I feel persuaded that the thread will always be within reach of the fingers, and may be more safely tied by them simply, than with the intervention of any instrument.

I may take this opportunity of remarking, that in the last number of this Journal I have been represented as utterly regardless of the pain suffered by patients. Having constantly endeavoured to lessen the sufferings inflicted through the practice of surgery, by diminishing the frequency of operations, and simplifying their performance, I trust that any notice of a charge so unexpected, beyond an indignant denial, will be deemed superfluous. It is true that, as stated in my paper upon the use of ether, I regard the pain suffered during an operation as a secondary consideration, when compared with any defect in accomplishing the object of its performance. And I still think that the North British Reviewer did no service to the public or the surgical profession, by exaggerating the importance of pain, and misrepresenting the effects of its prevention.

ARTICLE II.—*Case of an Unusual Form of Post-Pharyngeal Abscess.*

By T. B. PEACOCK, M.D., Physician to the Royal Free Hospital and Royal General Dispensary, Aldersgate Street.

SURGEONS have long been familiar with the occurrence of abscess in the post-pharyngeal cellular tissue: the following instance, however, which exhibits an unusual form of the affection, and the difficulty which occasionally exists in distinguishing these cases during life, is conceived to be of much practical interest.

Emma Webber, aged seven months, was brought to me at the Royal General Dispensary on the 26th of March 1845. She was stated to have been stout

and healthy till three weeks before that time, when she began to fall off in appearance, became fretful and peevish, and gradually lost flesh. She had suffered from difficulty of breathing for about a week, and it had been severe for the last three days.

The dyspnœa first came in paroxysms, the child's breathing being little affected in the intervals, but it gradually increased in severity and became constant, though still subject to aggravation in paroxysms.

When first seen by myself the dyspnœa was extreme, and each inspiration was attended by a loud sound, which, while it bore considerable resemblance to the sound of croup, was not precisely like it, and gave the impression of an obstruction chiefly confined to the immediate neighbourhood of the glottis. The sound of the voice was not in any way altered. There was a short dry cough, which it was stated had been present for the last few days; it had not the croupy character. The child continued to take the breast, and had no difficulty of swallowing. The neck and throat were carefully examined, so far as the extreme difficulty of breathing and the struggles of the child would allow, but without eliciting any information as to the nature of the obstruction. The chest expanded fully on inspiration and sounded sufficiently clear, and auscultation afforded no evidence of pulmonary disease. The child had just cut the two lower incisor teeth, and the upper incisors were pressing on the gums. There was considerable emaciation, the face was pallid, and the pulse quick and feeble.

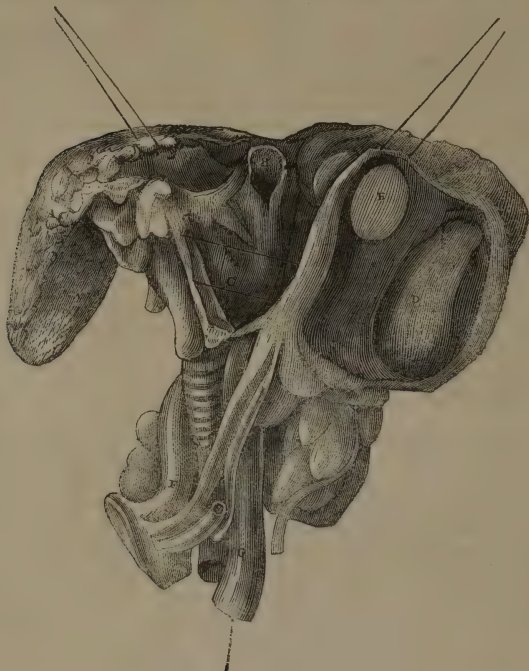
The nature of the affection was by no means clear; as, however, the obstructing cause, whatever it might be, seemed confined to the glottis, and the difficulty of breathing was so urgent in the paroxysms as to threaten suffocation, I suggested the performance of tracheotomy. The child was, however, seen by two of my colleagues, one of whom had prescribed for it the day before, and stated that the dyspnœa was less urgent than it had been, and, as neither concurred in the propriety of the operation, general treatment was prescribed. The gums were lanced, an emetic mixture was given, and a warm bath directed to be immediately employed. During the operation of the emetic a considerable quantity of mucus was hawked up from the throat, and the breathing became more easy afterwards, but no general amendment took place, and the child died exhausted the same evening. When first seen the mouth was a little drawn to one side, but there were not at any time decided convulsions.

The body was examined twelve hours after death.

The lungs were healthy, with the exception of the inferior margin of the middle lobe, and the posterior part of the upper lobe of the right lung, where portions of small extent were solid, of the deep purple colour of lobular pneumonia, and sunk in water. On section, the condensed portions displayed numerous minute white specks, from which a little milky fluid exuded on compression. The bronchial mucous membrane was natural, but the smaller tubes contained much pale mucus. The heart, liver, and kidneys were healthy. The spleen unusually large and solid. The alimentary canal was healthy, and the mesenteric glands not enlarged.

On dissecting the pharynx from the bodies of the vertebræ, an abscess was exposed, situated in the post-pharyngeal cellular tissue. It extended downwards in front of the bodies of the vertebræ, from the articulation of the atlas and occiput, for upwards of an inch, and was about one inch and a half in breadth. It was flattened in front, so as not to occasion much general protrusion of the posterior wall of the pharynx. Above the centre of the anterior wall of the abscess, however, its cavity was prolonged forwards so as to form a conical cul-de-sac, which projected into the fauces, and by its extremity entirely closed the aperture of the glottis. The main abscess was bounded by a dense sheath of cellular tissue; but the walls of the small cul-de-sac were so thin at its apex as to be quite translucent, and consisted of little else than the mucous membrane of the pharynx. The abscess contained thick yellow-coloured pus. Some of the absorbent glands in the course of the large vessels of the neck were

enlarged, but the bone was nowhere denuded. The larynx and trachea were free from appearances of disease, but contained much thin mucus.



A The Tongue.

C The Pharynx—slit open at the left side.

E The small Cul de Sac, showing its connexion with the main Abscess, and the point by which it obstructed the Glottis.

F The vessels given off from the arch of the Aorta.

B The Glottis.

D The larger Abscess.

G The Esophagus.

Remarks.—This case, though at first sight it bore a close general resemblance to one of croup, yet presented peculiarities in its history, course, and symptoms, which sufficiently distinguished it from that affection.

First, Unlike a case of croup, the symptoms had been slow in their progress, nearly three weeks having elapsed from their commencement before they had attained such severity as to induce the parents to seek medical relief.

Second, The difficulty of breathing had steadily become more urgent from the period of its first appearance; and, though throughout subject to exacerbations, the dyspnoea was never so completely absent in the intervals as it generally is in cases of croup.

Third, The voice and cough were free from the croupy character, and the sound produced on inspiration was different from that of croup on the one hand, and of crowing inspiration on the other. It gave, indeed, as before stated, the impression of an obstruction confined to the neighbourhood of the glottis.

Fourth, The degree of general febrile disturbance was less than that ordinarily attendant on cases of croup. The symptoms, indeed, were rather those of prostration than of excitement.

These considerations precluded the case being regarded as one of croup. They left, however, the true nature of the obstruction at the glottis in obscurity, and, it will be observed, that the more characteristic symptoms of suppuration in the post-pharyngeal cellular tissue were absent, or imperfectly marked.

On examination, both by inspection and with the finger, no swelling was detected, either at the back of the pharynx or at the side of the neck, nor was dysphagia present at any time during the child's illness. The editors of the *Gazette Médicale* have remarked, in reference to a case of this description related in a Belgian journal by M. Bessems,¹ that for the detection of abscess in the post-pharyngeal cellular tissue, it is sufficient to think of the possibility of its occurrence, since an accurate examination of the fauces would at once remove all doubt of the nature of the affection. From repeated experience, however, I have found that the examination of the throat of an infant labouring under severe dyspnœa, whether by inspection or with the finger, is extremely difficult, and will often, however carefully performed, elicit no satisfactory information. In the present instance, the peculiar form and situation of the main abscess, and the small size of the anterior prolongation of the sac, which by obstructing the glottis occasioned death, will readily explain the absence both of marked swelling in the fauces and of difficulty of swallowing. The absence of dysphagia must indeed be regarded as a peculiar feature in this case. M. Mondière, M. Bessems, and all writers on this form of affection, have laid great stress on the presence of this symptom, and its general appearance before the occurrence of dyspnœa, in cases of post-pharyngeal abscess, as affording the means of their deferential diagnosis from true croup. This case, however, shows that too much importance must not be attached to this symptom, since it was distinctly stated that the child continued to take food and to swallow throughout the whole course of its illness, and till within two or three hours of its death.

The case related is by no means a solitary instance of the patient's life having been lost, owing to the nature of the affection not having been ascertained, and the appropriate treatment employed. Similar cases have been recorded by Mott, Manoury, and Dariste;² Mr Carmichael³ has referred to one which occurred in his own practice, another is quoted in the *Archives Générales*, from a thesis by M. Ballot;⁴ more recently one has been related by M. Bessems;⁵ and,

¹ *Gazette Médicale*. 1846. Also Month. Journ. of Med. Science, Aug. 1846.

² *Ibid.* 1842. Vol. x. p. 394.

³ Trans. of Dublin College of Physicians. 1820. Vol. iii. p. 180.

⁴ *Archiv. Gen. de Méd., iiime. et nouvelle serie.* 1841. T. xii. p. 257.

⁵ *Gaz. Med.* 1846.

since the preparation of the above case was exhibited at the Pathological Society, a fourth has been mentioned to me by my friend Mr Milton.

When the existence of matter is detected in the post-pharyngeal cellular tissue, or deeply seated in the neck, there can be little hesitation as to the propriety of immediately effecting its evacuation by puncture, and the experience of almost every medical man shows the generally rapid recovery of such cases. In the present instance, in the absence of any information as to the cause occasioning the obstruction at the glottis, tracheotomy could alone have been had recourse to; and it is much to be regretted that its performance was not enforced, since there is little doubt that, had the child's life been prolonged, even for a very short time, the walls of the smaller cyst would have given way, and the matter have been evacuated.¹

ARTICLE III.—*Cases in Surgery.* By WILLIAM LYON, ESQ.,
Lecturer on Surgery in the Portland Medical School, Surgeon to the Royal Infirmary, Member of the Faculty of Physicians and Surgeons, Glasgow, and President of the Glasgow Medical Society.

CASE I.—*Ligature of the Common Iliac Artery for Inguinal Aneurism.*

May 25th, 1847.—I was asked by my friend Mr T. Gray, to see a patient of his, Mr D., a gentleman fifty-three years of age, rather tall, corpulent, relaxed, of active and temperate habits, and in general enjoying good health. Patient had casually, about a month since, applied to Mr Gray respecting œdema of his left leg, for which frictions, bandage, and diuretics were advised.

Not improving, and in addition feeling pain about hip and upper part of left thigh, patient again called on Mr Gray, who, examining pained parts, found a tumour, size of expanded and arched hand, extending for three inches below, and rather further above Poupart's ligament, slightly depressed in centre by ligament, nearly of equal breadth as length, and throbbing violently.

Tumour could be slightly diminished by pressure, but expanded as the pressure was relaxed; pulsated alike laterally and vertically, and, when considerable pressure was made, a strong whizzing sensation was communicated to hand, symptoms, which taken in conjunction, and with others needless to specify, made its aneurismal character evident. Circulating system appeared otherwise healthy; he had no ailments excepting those arising from tumour, and had until this time been in daily attendance on his business as a builder. He was ordered to remain at home and at rest, use plain fare, and keep bowels relaxed. He was informed of the nature of disease, and that it would likely require performance of an operation. Mr Gray and I canvassed the treatment of aneurism by electro-galvanic puncture, compression on distal side of tumour on Brasdor's plan, and the merits and risks of those and ligature. In electro-galvanic puncture we had scarcely any experience, and not, we thought, sufficient observation by others. We were doubtful if coagulation could be effected by insertion of a few needles in a tumour through which so much blood

¹ A memoir on this subject, by Mr C. Fleming, containing two very interesting cases in which the abscess in the post-pharyngeal cellular tissue was punctured, and the patients rapidly recovered, is published in the 17th vol. of the Dublin Journal, p. 42. 1840.

was passing as in this instance, and were afraid of the introduction of *many*, not feeling certain that ulceration might not occur in their tracks, especially as there was reason to believe the sac was thin. We thought compression by the method of Brasdor objectionable, because, while being employed, the aneurism might so much enlarge as to make even ligature of the primary iliac impracticable; while the tumour extended so high, that application of a ligature lower than on that vessel would be hazardous, by exposing tumour to risk of being opened. In these opinions we were supported by the able counsel of Dr John Macfarlane, and all particulars as to the different modes of treatment, the usual course of the disease, and the dangers of the operation by ligature being explained to patient and relations, he in a few days expressed his wish to submit to latter method.

To diminish as far as possible bulk of intestines, patient was restricted for a day or two to small quantities of plain diet, and was ordered night previous to operation to take 2 Pil. Col. comp., and have following morning an enema containing Sp. Terebinthinæ.

June 3d, Wednesday, eleven o'clock, A.M., in presence of Mr Gray, Drs Macfarlane, J. G. Fleming, Mackie, and W. Findlay, I proceeded to secure the Common Iliac artery.

The patient was placed on his back upon a table, with the side to be operated on exposed to a good light. Standing on right side of patient, and placing forefinger of left hand on a point nearly on a transverse line to left of umbilicus, and thumb a little way above superior margin of tumour, I made an incision through skin and cellular substance, very slightly diverging outwards superiorly, about two inches inside of anterior superior spinous process, and seven inches in length. Aponeurosis of external oblique and tendon were now divided inferiorly, and its covering and muscular portion superiorly, to extent of first incision. The internal oblique was passed through in same manner, when, forgetting for a moment that, superiorly, transversalis muscle was still to divide, its external aponeurosis was for a moment mistaken for the more dense fascia transversalis lying on the peritoneum. The difficulty of separating last divided layer from the one below, and appearance of some muscular fibres, showed we had yet the transversalis muscle and fascia to pass through, and, nipping up a small point of the latter with the forceps, at inferior part of wound, and insinuating a director, the pale, reddish-blue peritoneum was exposed, finger introduced, and transversalis covering divided to extent of external wound. Thigh of patient was now flexed on abdomen, his head and shoulders raised, and peritoneum detached, first from transversalis fascia, and posteriorly from iliac fossa, until, without much difficulty or delay, I felt the pulsation of the common Iliac, and found, that on compressing it, pulsation in tumour ceased.

By copper and thin wooden spatulæ, parietes of wound externally, and peritoneal sac internally, were attempted to be drawn aside, that we might see artery; but this, from protrusion of the peritoneum, was found impracticable, and I therefore introduced an armed aneurism needle upon my hand, slipped it along finger, partly with nail and partly with point of instrument, without much difficulty separated the artery from the vein and its other connexions, and passed the needle under it. But ligature could not be taken hold of with fingers, so as to draw its extremity through the eye of the instrument; and it was with some difficulty, that, by soaking up the pool of blood in which the point of the needle lay, by means of a piece of sponge attached to a slip of wood, which we had prepared on purpose, that point of instrument was exposed, and artery lying over it. Ligature was now seized with forceps, but, having become entangled around instrument, could not be withdrawn. Still keeping artery over needle, another needle and ligature were conveyed along first; and this time ligature being easily led through, and it being found that pulsation in tumour entirely ceased when artery was compressed between point of finger and thread, latter was tied. Only two small arterial branches

threw out blood per saltum, and it ceased without ligature. Tumour now lost motion, tension, and size; and lips of wound were put in contact by a few sutures passing through muscles and skin; were supported by strips of plaster, and these, again, by a compress and broad bandage. Limb was laid upon, and covered with carded cotton, and patient ordered to be kept quiet. He suffered operation most heroically, though it was a little delayed by the entanglement and consequent difficulty of drawing through ligature. He scarcely appeared distressed, and pulse was only slightly, if at all changed, from its healthy condition.

Before leaving room, say fifteen minutes after dressing wound, I laid my hand on tumour, and was sorry, but not surprised, to feel it feebly pulsating. The other gentlemen, all of whom had satisfied themselves of cessation of pulsation after application of the ligature, were greatly surprised, and doubted if it was not permitted by the ligature not having been properly knotted, and having therefore become loose. I had felt tumour almost in expectation of finding pulsation, which I thought could not be long absent, from the large number of branches directly or indirectly throwing blood into the sac, and enlarged as they likely would be by free anastomoses. Directly, I contended considerable currents would be carried by the epigastric, circumflexa ilii, and by the numerous and large inoscultations of the internal iliac, with the same vessel of the opposite side; and with freedom also retrogradely, by the profunda supplied from the obturator, and ramifications of internal and external circumflex arteries, &c.

At eight o'clock evening, patient felt comfortable, pulse was eighty-four; pulsation very feebly continued, tumour was diminished one half in bulk, and slightly painful, and left thigh, leg, and foot, were considerably lower in temperature than those of other side.

To take two grains of opium at bed-time, and limb to be gently and frequently rubbed by hand of attendant.

June 3d. Thursday, eleven forenoon.—Has slept several hours, and taken a very small plain breakfast. Pulse 120, of moderate strength; not hard or wiry. Skin of general surface, of healthy temperature, abdomen free of uneasiness and soft, urine passed repeatedly. Tumour painful on pressure; pulsation just perceptible; size, one-third of what it was previous to operation. Left limb cool; if not rubbed, cold; feels benumbed since morning.

To continue rubbing occasionally, and in intervals cover limb with carded cotton.

Eight evening.—Present, Mr Gray, Drs Macfarlane, Fleming, and self. General state nearly as above mentioned, but pulse smaller. Pain on site of wound upon moving or coughing. Limb, especially foot and lower part of leg, almost cold, and of mottled blue colour.

Almost constant friction of limb to be employed, and to take $1\frac{1}{2}$ grain opium, with two grains calomel, every three hours, if not asleep.

Ten, night.—Frictions have been constantly practised during the two hours since last visit, and limb is now nearly of healthy temperature. To flex thigh and leg slightly, so as to diminish pressure by tumour over artery in groin, and thereby permit more fully passage of blood. If limb continues warm to omit friction after a few hours, substituting wrapping in cotton covered with flannel, but watching temperature carefully, and rubbing again if necessary.

June 4th, Friday.—Has slept some, and is free of pain. Countenance haggard and cold; perspires profusely; abdomen not tympanitic: considerable discharge of bloody serum from wound; pulse 130, and very weak. Limb very cool, if not cold; but has only been so since rubbing was omitted a few hours ago. Bowels not moved since operation. To have half a table-spoonful of brandy every two hours, and friction of limb to be renewed.

Eleven o'clock, A. M.—Is sinking, brandy to be increased.

Died at a quarter to six P. M., fifty-four hours after operation.

Examination of the body.—Monday, June 7th. Decomposition remarkably

advanced the short time since decease considered. No vestiges of inflammation *at any part* of peritoneum, unless a small quantity of milky serum in track of wound be so reckoned. Tallow injection was now thrown into superior part of abdominal aorta, and being allowed to cool, vessels examined. Arteria media sacra and right iliac vessels well filled. A small quantity of injection mixed with blood, in part of left common iliac above ligature. Ligature situated about half an inch above origin of internal iliac. Latter vessel slightly permeated by injection, and small portions felt amidst blood in external iliac as far as aneurismal sac. Sac not more than a third of the size it was before operation, so that a considerable extent of external iliac remained uncovered by it. Contents of sac, a few particles of injection mixed with soft coagulated blood. Circumflexa ilii of left side well filled, and considerably larger than that of right. Left epigastric, as might be anticipated, from point where injection was thrown into aorta, and from the circuitous route of its other and small anastomoses, not filled, but vessel also above usual size; both circumflexa ilii and epigastric given off from sac. Upon artery at ligatured point being slit up, a pretty large coagulum was found above, and a very small one below, strictured part. At point where ligature applied, internal coat lacerated and puckered; internal and middle coats red, but no, or at least the slightest, appearance of lymph or adhesion. Left ureter attached to peritoneum in track of wound, and uninjured. Anterior crural nerve in normal situation. Exposed *psœ* muscles of healthy appearance, &c. Sac situated beneath, and so pressed on by Poupart's ligament as to appear, externally, divided into two portions, of which the larger was superior. Sac was so matted to surrounding parts, and so soft and thin posteriorly and superiorly, that, in a very careful attempt to remove it and attached vessels for preservation, it gave way. And two hours having now been expended on the examination, body being inconveniently situated in the coffin, relations becoming impatient, and my own engagements urgent, I did not try to ascertain the particular species to which the aneurism belonged, or the state of the artery at upper part of thigh, or the anastomoses of its branches with those of the internal iliac, through obturator foramen, sacro-sciatic notch, &c. Similar reasons prevented examination of pectoral organs, &c., though absence of symptoms indicating their implication, made their inspection almost superfluous, we would suppose.

I have thus detailed this unfavourable instance of surgical interference, a duty imperative, though disagreeable,—omission of which, if not criminal, is at least cowardly, and betrays either conscious culpability, a morbid and indefensible dread to avow truth, or the want of candour to acknowledge error. And, although disagreeable, if the detail shall prove useful, by adding a single fact to those collected regarding the operation,—by showing its difficulties and dangers, shall incite to trial of less severe or less hazardous procedure,—or supply data, by which to compare its consequences with those of other methods,—I will feel the consciousness of having made my experience as useful as possible to the profession, and of having at least acted ingenuously, if not successfully.

There are, however, men who hunt down reputations with as much ardour as the jackal does his prey, to whom this case will afford satiety of their much enjoyed propensity. These persons, with egotistical vanity, confound their malicious dispositions with the knowledge and ability to criticise;—interpret success, as *chance*;—the want of it, *cruelty*; and the will and capability to interfere, when

required, is in their vocabulary synonymous with temerity. It is consolatory that the power of such individuals is in the inverse ratio to their wills; they are like stingless wasps, desirous but unable to wound;—and, besides, have an antidote to their intended poison in the good sense and feeling of their brethren in the profession, to whose appreciation I willingly leave them.

The operation was undertaken with a full knowledge of the responsibility incurred. It was not lost sight of, that, without the operation, the patient was likely to live a considerable time; while, if performed, he incurred the risk of quickly sinking. But there was nearly the certainty he would have soon died of the disease, and, for aught known to contrary, death might have occurred soon and suddenly; while, by operation, there was a chance, and a considerable one too, of early and complete recovery. The latter clause may be doubted, when the extensive wound is reflected on, and not only extensive, but largely implicating, by detachment and exposure, the easily excitable serous tissue, uncovering nerves and muscles, and thereby first inducing the risks of shock; while, should they be passed, the secondary effects are still to be dreaded,—the more so, as just stated, from the size of the wound, the nature of the tissues involved, and added to by its depth and position, from which the discharges have a tendency to gravitate towards, and implicate, the pelvic contents.

From such considerations, the greatest doubts of success would present; but when we appeal to facts, to the statistics of the operation, our first impressions are not supported. In the lectures of Mr Guthrie, so conspicuous for interesting, instructive, zealously collected, and very able opinions, facts, and cases, on wounds and injuries of arteries,¹ we find it stated, that “the operation of placing a ligature on the common iliac artery has been performed certainly fourteen times, and has succeeded in six instances;—6 American, 5 British, 1 French, and 2 by Russian or German surgeons.”

This is certainly still a large mortality; but, from the nature of the operation, not as large as *à priori* would, I think, have been anticipated; and, considering the fatal conditions for which the operation is undertaken, it surely affords a just defence for subjecting a patient to its chances; and yet Mr Guthrie appears justly to suppose that the statistics above quoted do not correctly indicate the dangers of the operation which are absolutely inevitable, as in two of the cases the peritoneum was necessarily opened—an occurrence undoubtedly adding to the usual risks; so that, if the number of operations, fourteen, be sufficiently large for correct inferences, the future success may be higher than that stated.

The comparative severity of the operation, however, the importance of correct diagnosis, and the consequent preference which

¹ Med. Times, 1846. Vol. xiv. p. 190, § 1.

should always, when practicable, be given to ligature of the external iliac—an operation in which, Mr Guthrie informs us,¹ “the mortality, as deduced from eighty-six instances, is not greater than in those from all sorts of cases on the femoral artery at the ordinary place of election,” is thus very strongly shown. Whatever objections may be offered to the statistics, it is not with this as some other operations reported as practised successfully in distant regions, and by unknown operators; for the names of the distinguished French, American, and British practitioners, afford at least a guarantee that the circumstances have been truly reported.

I have not been able to find the details of all the cases in which the common iliac has been ligatured, and cannot therefore state the causes of the unfavourable results in the fatal instances. I think, however, that shock, inflammation, suppuration, and hemorrhage, on separation of the ligature, comprehend the principal risks, and that the chance of their occurrence is likely to be as I have placed them.

I cannot refer the result in the case detailed by me to aught else than the first of these—shock, or sinking of the constitutional powers; for the appearances on inspection did not indicate *the slightest* effects of reaction; and yet the symptoms of that condition did not manifest themselves immediately after infliction of the injury, as they usually do. For the first twenty-four hours he was in every respect favourable; when sinking commenced, and without pain or any appreciable lesion, he rapidly lost strength, retaining his mental powers in perfection to the last. The different capabilities for resistance to effects of injuries dependent on temperament, sex, age, and race, are well known; and the influence on the results, occasioned by the parts injured, the extent of the injury, and in an operation, the time occupied in its performance, are too trite for comment. He bore the operation most coolly and courageously, while nothing injurious was done unnecessarily, and what is not absolutely unavoidable; and, with exception of a very brief delay in passing the ligature, nothing occurred which should have acted in disappointing our eager anticipations. We did not think there was any thing especial in the constitution, condition, or habits of our patient, making him an unfavourable subject for operation. But I have since learned we acted on incorrect information; for although seldom deeply inebriated, it now appears he tippled continually, and, to the impairment of his constitutional powers so occasioned, can I alone refer the early approach and rapid progress of the fatal asthenic symptoms.

Looking to the success with which of late years the abdomen has been largely laid open, and the intestines exposed and handled in the operation for removal of the ovary; and recollecting how

¹ Med. Times, 1846. Vol. xiv. p. 190, § 1.

often, in operations for hernia, the same parts, at least large portions of them, after subjection to severe and long-continued injury too, are exposed to the influence of the air for long periods, and manipulated and pressed to effect their reduction: it occurs to the mind whether ligature of the common iliac would not be more successfully, as it would most assuredly be more easily practised, by cutting into the abdomen in front, over the point where the vessel is wished to be secured, passing through the peritoneum over it, and thus quickly and with facility accomplishing the purpose in view, than by our present troublesome, more protracted, and in some respects more coarse and severe operation. This would be a return to the practice of Sir A. Cooper on the aorta: is thrown out interrogatively, rather as announced preferable, and will probably be latterly determined by the comparative statistics of the operations referred to, and the present mode of securing the common iliac, after a larger accumulation of facts has been obtained.

I have mentioned, that in order to avoid the evidently serious dangers of the operation, we casually discussed the merits of electro-galvanic puncture, a practice, so far as I know, not yet tried in this country for aneurism. It certainly appears a suggestion, or rather practice, exceedingly feasible, effecting, as it is said to do, coagulation of blood in the sac, as after operation by the ligature, quickly and safely. It was discarded by us from want of sufficient information and observation, and from a dread that, if not successful, it might be hurtful by ulceration occurring in the tracks of the needles required to be employed in the method—an occurrence the more to have been feared in our instance, that the size of the tumour would have demanded numerous needles; and attended with the greater hazard, that the softness and diminution of the tumour by pressure, rendered probable, thinness of the sac, and absence of fibrinous layers. I have tried the practice on purpose to effect coagulation, and thereby render impervious, varicose veins on the lower extremities. The patients presented in the Dispensary of the Royal Infirmary here; but, though urgently requested, did not return so as to enable me to judge of the value of the treatment. In the *Medical Times*¹ several cases are related of aneurisms cured by this method: one by M. Ciniselli of Cremona, another by M. Petrequin at the Hotel Dieu, Lyons. In Braithwaite's *Retrospect*, too,² details are amply referred to of a similar kind: and based as this treatment is on rational views, and producing curative conditions analagous to those sometimes arising from natural causes, it is applicable, from being performed *on the tumour*, to many cases where ligature of the artery nearer the heart than the tumour, is impracticable, and likewise to *nævi*, treatment of the various forms of which is by the present methods often found troublesome. It is easy of execution, too, and we hope soon

¹ 1846, Vol. xiv. Pp. 234, 422.

² 1847, Vol. xv. P. 209, & sequelæ.

to see it subjected to such trials as will better enable us to judge of its merits than we are at present prepared to do.

As to employment of Brasdor's method, by interrupting the flow of blood through the artery distad to the sac, there were objections additional to the one mentioned, of such enlargement of the aneurism during its trial, as might have rendered ligature of the primitive iliac impracticable. For the ligature could not have been applied between the sac and the origin of the profunda; and therefore an undiminished, or from arrestment to the flow through the femoral, possibly an increased current would have been sent through the sac, and thence into the profunda, so as to render expectation of retardation, or rest of blood in the sac almost visionary. But, apart from this, free circulation through the sac would still have been kept up by the epigastric and circumflexa ilii, both of which entered the tumour.

Visionary and faint though the hope might have been, however, we had determined, if not to ligature, at least to try compression of the femoral on Brasdor's plan, had the patient not agreed to the operation performed. We were the more incited to this, from observation of the following case.

CASE II.—*Treatment of Aneurism of the Arteria Innominata, by compression, on Brasdor's principle.*

November 28, 1843.—I was consulted by J. S., forty-three years of age, a man of very intemperate habits. For last six weeks he has suffered from pain in back of neck and right shoulder, and two weeks since observed some swelling at lower and anterior part of neck. A strongly pulsating tumour, size of a small fist, diminished by pressure, but expanding as pressure is removed, of regular form on surface, narrow above, and broad below, occupies neck from right side of thyroid cartilage to sternum, and extends laterally under inferior portions of sterno-clavicular muscles. Parietes firm at inferior, but soft and thin at superior part, where the tumour overlaps the carotid to within half an inch of superior border of thyroid cartilage. Pulsation of carotids normal, that of right subclavian and radial scarcely to be felt. No bruit at any part of tumour; action of heart healthy; no dullness on percussion, or absence of respiration over any part of chest, anteriorly or posteriorly. General health tolerably good. Within three or four days he was twice bled, each time to twelve ounces; cold water was applied to tumour; he took Tr. Digitalis with A. Plumb. frequently, and was kept on spare diet, and at perfect rest.

December 2.—Drs Macfarlane, J. Watson, A. D. Anderson, and the late Mr Douglas, saw him with me. After careful examination, all agreed there was every reason to believe the tumour depended on dilatation of the arteria innominata, and a majority advised, that, considering the dubious effects of Brasdor's operation, which alone was practicable here, and from the extension of the tumour to very nearly the division of the carotid, and consequent difficulty or increased danger of applying a ligature, that the usual course of the disease, and the chances from the required operation, with its attendant dangers, should be fairly stated to patient, and that, if he chose to submit, the common carotid, if possible, should in first place be secured, and if any benefit followed, that the subclavian of same side should afterwards be operated on.

He refused, and I then determined to try the forlorn hope of compression on the carotid beyond the tumour. The pressure was applied by a half circle of steel spring, with a button and screw over the vessel, and a plate for ob-

taining counter pressure behind, and sufficiently large to avoid lateral part of neck. He was to use the apparatus as often as possible, and firmly enough to interrupt pulsation in the temporal of right side.

December 21.—Has worn the apparatus frequently; no change on tumour; pain in neck and shoulder less severe. Proceed.

December 27.—Has been adhering closely to treatment, viz. rest, low diet, purgatives, Tr. Digitalis with A. Plumb., and use of the collar generally for an hour and a half at a time, and in all for eight or nine hours in the twenty-four. I think the tumour is to-day as a whole decidedly less, particularly on left over part covered by sterno-mastoid. There is unequivocally far less pulsation, that is to say, it is much less forcible, especially over portion of tumour overlapping carotid; and it is not only much less at this part, but this portion of the tumour, formerly soft and easily compressible, with its parietes apparently very thin, is *now, firm, undiminished* by pressure, and scarcely pulsates. Besides, the tumour is altogether firmer, pains less troublesome, voice not so hoarse, and swallowing easier. Right subclavian pulse smaller than at former examination.

To continue treatment *most assiduously*.

1844. *January 4.*—Since last report has adhered rigidly to treatment. Health good—pains gone—voice and deglutition normal—pulse small, soft, and eighty. On measuring tumour with a cast taken on 21st December, it is found diminished in its transverse and longitudinal directions three-fourths of an inch, and vertically, the patient being prone, nearly to same extent. Other conditions of tumour continue favourable, pulsation scarcely being observable, though formerly evident at several yards' distance, and slight pressure is now required to make it distinct. Right carotid patent throughout all exposed course—radial likewise so, but pulsation exceedingly weak. Has suffered no inconvenience by use of instrument, which, from impossibility of confining it entirely to artery, was feared, from effects on internal jugular and par vagum. To keep closely to present method.

February 9.—No change.

February 24.—In former state.

November.—Patient has been seen by Drs Macfarlane and Watson, who are much surprised at his greatly improved condition. He now lounges occasionally out of doors.

December 24.—To-day took a third cast, from which I am glad to observe that, on careful measurement by compasses, the tumour is less by one-half than when former cast was taken; or by two-thirds in all directions, as shown on the first one.

1845. *April.*—Appearances as formerly, but feels pains as before, and has slight cough.

July.—His wife informs me he has returned to his intemperate habits, being almost constantly drunk; strikes her, and is so violent she wishes him confined. Of course, treatment was now neglected.

July 26.—Died suddenly and unexpectedly; and death was found to have been occasioned by copious hemorrhage into right pleural cavity from a large rent in an aneurismal sac.

The aneurismal tumour extends from cricoid cartilage to arch of aorta, is of ovoid shape, and about the size of a moderately large cocoa-nut. It is firm above where closely attached to right sterno-clavicular articulation, and at that part contains a large fibrinous deposit; at other parts it is filled with soft coagulated blood. On right side, parietes are as thin as writing-paper, where a rent an inch in length exists. Below, it is fused into commencement of arch of aorta for extent of two or three inches, and left carotid arises from its lower and anterior border. The right carotid and subclavian arteries, which are both pervious, arise from its upper and posterior part. It thus appears to be principally formed by the innominate, largely and irregularly dilated, the aorta being but very slightly involved.

I am well aware different conclusions will be drawn respecting the effects of the treatment in the case just detailed; by some it will be viewed as having been decidedly beneficial; the diminution, decrease of pulsation, solidity, betokening deposition of fibrinous layers, abatement of pain, and all other symptoms, with prolongation of life for twenty months after disease had made great progress, indicating marked advantage from the practice. Others will refer the whole to the rest, bleeding, regulated diet, &c. While a third party may contend the treatment was the reverse of beneficial, and will insist that the absence of respiratory murmur, or dullness on percussion, at all points when the case was first seen, proved the aorta to be then unimplicated; and that, by interruption to the flow through the right carotid, a greater impetus was thrown on the innominate, which gradually enlarged, and afterwards expanded into, and carried with it, the aorta.

There is surely considerable reason to believe, however, that as the tumour pressed on the right subclavian, from its position, likely before giving off the thyroid axis, as denoted by the nearly absent radial pulse; and flow through the right carotid, was likewise often prevented by the ring tourniquet used; that the blood, in the expanded innominate which formed the tumour in the neck would be frequently almost at rest; that coagulation, deposition of fibrin, loss of the heart's impetus, and contraction of the aneurismal sac, were thus procured: but that the aorta, though not indicated by any symptoms, being from the first diseased, proceeded to expand, which, had the innominate alone been involved, the practice followed as it retarded, would probably have permitted to contract.

Explain the improvements as we may, succeeding as they did immediately on institution of the treatment, regularly advancing until the patient returned to his intemperate and violent habits; they were sufficiently beneficial and remarkable to entitle a similar plan to trial, when more certain measures are impracticable, or will not be submitted to.

It must be acknowledged the aneurism in the case where I ligatured the common iliac, was less favourably situated for treatment on Brasdor's plan than the one of the innominate; for, unless we could have applied pressure so as to command the current by the profunda as well as the femoral, the quantity of blood passing through the tumour would have been only slightly diminished: and still though we had, free entrance or exit of blood would have been permitted into the sac by the epigastric and circumflexa ilii: nevertheless, such diminution of the quantity, partial rest, and decrease of the heart's impetus might have been obtained, as would have retarded, if not cured the aneurism.

In ligaturing the iliac we did not etherize the patient, fearing lest the occasional involuntary motions under its use might seriously impede the procedure; especially, as it has yet to be shewn it lessens, although equally so, that it increases danger.

In operating I adhered more to the method of Liston and others, than to that of Mr Guthrie. In making the external incision, Mr Guthrie recommends its upper part to be carried "diagonally inwards towards the edge of the rectus muscle, above the umbilicus."¹

It is true you are thus more nearly over the track of the vessel, and thereby may be supposed to have a little less difficulty in the necessary manipulations; but by being nearer the middle of the abdomen, is there not a greater extent and bulk of peritoneum and contents, from protrusion of which the greatest difficulty in the operation is encountered, to be turned inwards?—and is there not likewise risk from division of the lateral branches of the epigastric, which, though in other respects unimportant, would annoy? If I may presume to speak, the length of the wound, not forgetting the proper situation, is a part of more importance than the direction, as recommended by Mr Guthrie. My incision was seven inches, and evidently not too long, the hand requiring to be freely admitted before the finger could be placed on the artery; nay, the greater part of both hands before the ligature could be knotted, &c.

With exception of keeping asunder edges of wound through parietes, I did not find the copper or wooden spatulæ of any service. When used to support peritoneal sac, it was protruded so much, especially when pain caused patient to wince, that we feared to make as much pressure by them as was required, lest the peritoneum should have given way; while the hands holding the spatulæ occupied so much space, and incommoded so greatly, that the articles were thrown aside, my own fingers, and those of the assistants far more safely and effectually drawing aside, and supporting, the distended and bulging peritoneal sac.

It may not be superfluous to direct attention to the little difficulty and delay I experienced from the small matter of twisting and entanglement of the ligature around the aneurismal needle, a circumstance trivial foresight will always prevent. I might have withdrawn the needle, and have easily re-introduced it; but to this there was the objection, that I might, and very likely would, have insinuated the needle at another than the first point where it passed, and thus, unnecessarily and injuriously, detached the artery from its connections, and therefore chose rather to pass a second needle, by running it along the first. I was provided with an aneurism needle, from which the point containing the ligature can be drawn off after the instrument has been passed under the artery. This I did not employ; but, so deep is the wound in this operation, so tightly is the wetted ligature drawn in the eye of the instrument, while the latter is being passed in, that it cannot be felt, far less taken hold of by the fingers: only with great care can the point be *seen*, so that the ligature can be seized with long small forceps; and, if there be con-

¹ Med. Times, 1846. Vol. xiv. p. 267. § 1.

siderable oozing, exposure of the point will, I feel assured, be quite impracticable. The artery is so far from the surface, that a longer aneurism needle than usual should be provided: if it has a longer curve, too, some of the difficulties will be obviated; but I would say, the instrument of Mr Weiss should always be at hand, if not solely employed.

Return of pulsation in the tumour, after operation for aneurism, is not very uncommon; but days usually elapse before it occurs, as it generally requires and depends on establishment of a free *collateral* circulation. The remarkable circumstance in my instance was, the manifestation of pulsation so soon after the operation. We will see, however, there are other facts on record proving existence of nearly similar conditions as permitted its return in my patient.

In Mr Guthrie's Lectures,¹ a case is related, where "Dr Horner of Pennsylvania, placed a ligature on the *external iliac* artery, just above the origin of the epigastric, in consequence of an aneurism of the femoral in the groin." "This operation having been *completed* with great precision, as was proved after death, Dr Horner thought it right to open the aneurismal sac, which, after the laminated fibres and coagula had been removed, poured out arterial blood in the *most formidable and urgent manner*, so that the patient lost more than twenty ounces in a few minutes."

And yet, in Dr Horner's case, there were not the same facilities for immediate passage of blood into the sac as in my one. He tied the *external iliac*, and therefore all current by the free anastomoses of the *internal iliac* with branches of the same artery of the opposite side, and with the media sacra and lateral sacral of the same side, was interrupted; but still, blood was carried so freely into the sac by the epigastric and circumflexa ilii, and retrogradely by the profunda, supplied by its anastomes behind and on each side, as to afford the *formidable and urgent "hemorrhage"* described, and fully explaining the immediate return of pulsation in my patient.

Again, in the same work,² Mr Guthrie quotes another case, proving that blood may be largely, and *immediately*, carried by collateral vessels into an artery below the point ligatured, thereby also accounting for the pulsation which recurred feebly so soon after the operation in my case. It was as follows:—M. Lutens ligatured the *external iliac* artery for a wound of the femoral, immediately below Poupart's ligament, bleeding from the wound having recurred several times. "The ligature having been applied during hemorrhage, the compress on the bleeding point was taken off as soon as it was tightened, when the hemorrhage returned as *furiously as before*, on which a second ligature was placed on the external iliac, higher up, which failed *equally in arresting the bleeding*. It was now manifest that the *epigastric* and *circumflexa ilii* arteries poured their blood

¹ Med. Times, 1846. Vol. xiv. p. 212.
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² Ibid. p. 192, column 3d.
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into the open femoral artery by regurgitation, and that the extremity of this vessel must be secured. M. Lutens therefore tied the femoral artery below the origin of the profunda, *but still the bleeding continued*, and, mortification of the limb ensuing, the man died."

"On dissection, it was found that the first and second ligatures had been placed *above* the origin of the epigastric or circumflexa ilii arteries. The last was placed *below* the profunda, so that three vessels entered into the artery between the ligatures, and brought blood collaterally into it, two above and one below, the opening made by the point of the sword."

Considering these particulars, we cannot feel any surprise at the immediate return of the pulsation in my case, seeing the blood could not only find its way by the vessels mentioned in M. Lutens' instance, but also by one larger, having equally, if not freer anastomoses, a more direct course, and entering the artery above the aneurismal sac, but below the ligature, viz., the *internal iliac* artery. Case 102 in the work already quoted, from the practice of M. Dupuytren, and others, more or less directly establish the freedom and celerity with which blood may be conveyed from one part of an artery to another, a ligature intervening. But it is not to be supposed this form of secondary hemorrhage or return of pulsation after ligature of a vessel above the seat of aneurism, will occur with equal frequency at every part of the vascular system.

In popliteal aneurism the collateral branches do not communicate very directly, or with very large arteries, from or near to the sac.

In the artery of the superior extremity, on the contrary, the collateral vessels are so numerous and large from the point where the artery has just passed between the scaleni, to where it travels in the axilla, that early return of pulsation,¹ or occurrence of hemorrhage below the ligatured point when the subclavian is operated on, is much more likely to happen, as in the patients of our townsman Dr M. Buchanan and others, so graphically detailed and pointedly censured by Mr Guthrie.² After ligature of the *external iliac*, pulsation or hemorrhage is at least as likely to happen as in the subclavian, &c.; and the probability is, we think, still greater if the operation has been performed on the *common iliac* than in other situations, in consequence of the current poured in below the ligatured point by the free anastomoses of the *large internal iliac*, and likewise the considerable sized epigastric and circumflexa ilii, while copious flow retrogradely may be afforded by the profunda.

The early return of pulsation after operation for aneurism, is a much less surprising fact than occurrence of bleeding from a wound in a large artery above which a ligature has been applied: in the one instance, pressure by the tumour often obstructs the ordinary course of the blood, and it is forced to find its way by the collateral vessels, which consequently dilate: in the other case, no such favour-

¹ Med. Times, 1846. Vol. xiv. case 73. P. 147.

² Ibid. Pp. 147, 148, 149.

able circumstances assist, and yet, even in such, as the patient of M. Lutens,¹ with wounded artery, formerly quoted, bleeding below the ligatured point takes place soon after reception of the injury for which the operation has been demanded.

It cannot fail to be observed how strongly the particulars in my patient go to support the extended observation, large collection of facts, able and valuable practical deductions and axioms founded on them, which Mr Guthrie has, with so much talent, zeal, and labour inculcated in his lectures on some of the more important points in surgery.²

Supposing my patient had been *wounded* where aneurism was *situated*; and, as has so frequently in parallel cases been done, the common iliac had been secured, in place of the wounded artery, above and below the injured point: there can be little doubt, if hemorrhage had not immediately recurred from the wounded part of the artery, that long time would not have passed until it showed itself seriously, quickly effecting that destruction which the severe and unnecessary operation had left undone.

And so likewise if the operation had been undertaken for recent traumatic aneurism (see case by Dupuytren³), as in that species of the disease, suppuration in the sac, ulceration of the original wound, and hemorrhage have been often met with.

Seeing the early return of the pulsation, the question arises, if the operation had not destroyed, would the aneurism have been cured. Analogy and reasoning incline us to answer in the affirmative. Pulsation after operation for aneurism, in general, soon ceases; in this case the tumour had lost two-thirds of its size, and motion in it was very feeble within twenty-four hours subsequent to the operation. Those conditions, arising from diminished circulation and loss of the heart's systolic impetus, would have increased as more indirect collateral circulation by the branches of the profunda with the obturator, pudic, ischiatic, and gluteal enlarged; and thus the blood, being diverted from its course through the sac, would there have been more at rest, would have deposited its fibrine, the sac have further contracted, and a cure been accomplished.

Though blood was carried into the sac, the cold and benumbed state of the limb indicated that only a small quantity passed beyond the tumour, and gave reason to fear impending mortification. This, in part, might arise from depression consequent on the operation, and in part from the unenlarged collateral vessels not permitting free circulation, and would probably have been exchanged for a better, or opposite state, had patient lived sufficient time for the ordinary changes to have been effected.

At all events, sufficient proof was afforded of the great efficacy of friction, so much recommended by Mr Guthrie for aiding the circulation, and consequently the temperature and vitality of the limb.

¹ Med. Times, 1846. Vol. xiv. p. 192.

² Ibid.

³ Ibid. p. 76.

For we repeatedly observed, that while cotton and flannel failed to maintain the warmth, prevent numbness, or lividity, gentle continued friction soon removed those harbingers of loss of vitality—an effect attributable in some measure to the evolution of caloric, as from friction of non-vital parts, but in greater degree here to its influence in stimulating the nervous energies, pressing the blood through the arteries, and promoting its passage into, and return by, the veins.

Report of the Surgical Operations performed at the Newcastle-upon-Tyne Infirmary, during a period of seventeen years and a half.
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PART I.—*On Amputations of the Limbs.*

THE importance of collecting the results of operations is so obvious, that no apology is required for introducing this paper to the profession. The source from which the facts relative to the Newcastle Infirmary have been drawn, is a register of the various operations performed in the hospital. In each case, the name, age, and trade of the patient are noted, together with the nature of the accident or disease for which the operation was required, the time of its performance, the date of his death or dismissal, and, in the older books, the duration of his illness. Although I have carefully searched through the other registers of the hospital, which have been preserved for nearly a century, I have been unable to find any other notice of operations, excepting a note on the number of cases of lithotomy in the year 1757, and a record of a few amputations about 1812; but, as in these last, neither the time of their performance, nor other circumstances are stated, I have rejected them as useless for statistical purposes. The operation register seems to have been commenced by the late Mr Church, at that time house surgeon to the Institution. The first entries which can be employed are in the year 1823; from that time until May 1843, it has been regularly kept, excepting during intervals of time, which amount, on the whole, to two years. The period, therefore, during which the operations are registered is seventeen years and a half.

As I could only answer for their correctness during the time in which it was my own duty to record these operations, I have carefully compared the results of each, as given by the operation book, with those furnished by the general registers of the hospital, and, with a few trifling and easily explained differences, have found them to agree.

There are a few omissions in the method of drawing out the cases

of amputations in the register of the operations, which it may be as well to mention, as they have deprived them of much of the value they would otherwise have possessed. The first is, that the particular part at which the limb was removed is not usually stated; and the second and still greater omission is, that neither the period at which dangerous symptoms came on after the operation, nor the causes of death, have been regularly noted down. This last is indeed a matter of the utmost importance, as it is only from a proper knowledge of the diseases causing death, that we can anticipate any great improvements in operative surgery.

I had originally intended to publish the results of all the operations performed at this Infirmary in one paper; but finding that the many valuable contributions to this part of science, which have been made during the last few years, had not been brought together, and that many of my medical brethren were still sceptical with regard to the great mortality consequent on amputations, I determined to incorporate the facts obtained at the Newcastle Infirmary with the observations of others. I have therefore collected, as far as the circumscribed limits of a provincial library afforded me the means of doing, the statistical labours of other surgeons, hoping that, although the conclusions I have arrived at may not be in all cases satisfactory, yet that these tables may prove of some assistance to others, who, with more time and greater opportunities, may hereafter pursue the subject.

1.—*General Mortality of Amputations.*

The number of amputations performed at the Newcastle Infirmary during the seventeen and a half years, is 435, of which number 59 deaths took place, or 1 in every 7·37; 229 were amputations of the limbs, and 54 of them died, or 1 in every 4·24. Of the 229, there were three persons who suffered amputation of two limbs, and in one case it was necessary to operate a second time, on account of gangrene of the stump; so that only 225 individuals submitted to the operation. Of these, 20 were dismissed as out patients, with their wounds still unhealed at an average period of 61·2 days; the time of cure of those who left the hospital perfectly recovered was 52·13 days; and the average time of death amongst the fatal cases was 15·03 days.

Of the 206 minor amputations 5 died, or 1 in every 41·2; the time required for cure cannot be accurately ascertained, as the greater part of them became out patients immediately after the performance of the amputation, but the average time of death amongst the fatal cases was 25 days.

TABLE I.

General Mortality of Amputations of the Limbs.

Hospital or Authority.	No. of Amputations.	No. of Deaths.	Average Mortality.	Period in which Operations were performed.
CIVIL PRACTICE.				
Liverpool Infirmary,.....	43	3	1 in 14'33	1834 to 1836.
" " (Mr Halton),.....	— 11'66	22 years.
" Northern Hospital,.....	96	18	— 5'33	1834 to 1843.
Edinburgh Infirmary,	61	31	— 1'96	3½ years.
Glasgow Infirmary,	276	100	— 2'76	1794 to 1839.
" "	155	47	— 3'29	1841 to 1846.
Six Scotch Hospitals,	24	3	— 8'	1842.
Newcastle Infirmary,	229	54	— 4'24	
Royal Berkshire Hospital,	27	5	— 5'4	1838 to 1845.
Chester Infirmary,.....	21	9	— 2'33	1838 to 1841.
University College Hospital,	66	10	— 6'6	1835 to 1841.
Guy's Hospital,	36	4	— 9'	1843 to 1845.
Great Britain (Mr Phillips),	233	53	— 4'39	
Collected from various Journals (Mr Phillips)	308	76	— 4'05	
Notes of various Surgeons (Mr Phillips), ...	107	28	— 3'82	
Various Surgeons (Dr McHardy),.....	364	83	— 4'38	
Total of British Practice,	2046	524	1 in 3'9	
Massachusetts General Hospital,.....	67	15	1 in 4'46	
Pennsylvania Hospital,.....	79	22	— 3'59	
America (Mr Phillips),.....	95	24	— 3'95	
Total American Practice,	241	61	1 in 3'95	
Germany (Mr Phillips),	109	26	1 in 4'19	
France (Mr Phillips),.....	203	47	— 4'31	
Hotel Dieu,	35	17	— 2'05	1840 to 1842.
"	178	104	— 1'71	1836 to 1842.
Hospitals of Paris (Malgaigne),	552	300	— 1'84	1836 to 1841.
Paris (Gendrin),	63	23	— 2'73	1834.
Paris (Dèpuytren),.....	59	15	— 3'93	
Total of Continental Practice,.....	1199	532	1 in 2'25	
Total of Civil Practice,	3486	1117	1 in 3'12	
MILITARY PRACTICE.				
Army at Algiers, (Garrison)	63	17	1 in 3'71	1837 to 1840.
Baron Percy,	92	6	— 15'33	
New Orleans,	52	12	— 4'33	
Naval Action of June 1, 1794,.....	60	8	— 7'5	
Bombardment of Algiers,	59	24	— 2'45	
British Army in Peninsula,.....	842	289	— 2'91	
British Army at Thoulouse,	100	31	— 3'22	
Other Military Records (Alcock),	74	6	— 12'33	
British Legion,	109	55	— 1'98	
Total of Military Practice,	1451	448	1 in 3'23	
Total of Civil and Military Practice,	4937	1565	1 in 3'15	

The assertion that one person out of every three who suffers an amputation perishes, would have been repudiated a few years ago as a libel upon our professional value; and yet such is the rate of mortality observed in nearly 5000 cases.

It is a striking illustration of the necessity of accurate calculations, that we find a celebrated hospital surgeon supposing that only 1 out of 20 died after this operation; whereas the general result of British civil practice, in more than 2000 cases, shows an average mortality

of 1 in 4; and how shall we reconcile the returns of military surgery with the boasted success of many of our military authors?

Upon observing how much the average mortality differs in different infirmaries, even in the same country, as from 1 in 14·33 at Liverpool to 1 in 2 in the Edinburgh Infirmary, we might be tempted to suppose, that these returns would enable us to estimate the healthfulness of the hospitals, or the relative skill of their medical officers. Such a supposition, however, will, upon a little reflection, be seen to be incorrect.

Two surgeons may each have six cases of gangrene; one may wait until a line of demarcation has formed in them all, may operate upon three of them, and save but two; the other may amputate the limb in every case, and only three may recover. Now, it will be evident that although the latter has, by his boldness, preserved the life of one more person than his more timid colleague, yet in estimating only the success of the operations, whereas in those of the former the average mortality would be but 1 in 3, that of the latter would amount to 1 in 2. It is, I conceive, partly from this reason, that, in looking over the operations of the Newcastle Infirmary, I have so seldom found the apparent success of any of our most celebrated surgeons commensurate with his reputation; but, on the contrary, have often observed those who have enjoyed the greatest name, number the largest amount of deaths amongst their patients. Again, as it will be afterwards proved that a vast difference in mortality exists according to the disease for which the operation is performed, and that traumatic amputations are much more fatal than those required for pathological causes, the practice of any hospital may appear peculiarly successful from its situation in an agricultural district, although, if more minutely examined, the results of its operations may be really less favourable than those of another placed in the midst of a mining or manufacturing population. These considerations are of no small importance, as I have known practitioners hesitate to publish reports which exhibited a high state of mortality; and, in more than one instance, may we see very creditable papers on operative surgery, whose merit is lessened by quotations from other works, inserted to exhibit the superior success of the hospital with which the writer is connected. But, although it is highly useful to have an accurate idea of the general results of amputations, we must be careful not to employ such knowledge to purposes to which it cannot properly be applied. We should remember, that in every operation its value to the patient should be alone considered; and the above figures, whilst they show the general success consequent on the labours of the surgeon, cannot be used to estimate the chances of any particular individual who may be placed under his charge. An attempt has been made to sanction other operations, by comparing their results with those of amputations; and, were the reputation of the surgeon the only matter for consideration, no better means could be employed for that purpose; but as it is the value to

the patient which is to be estimated, the matter must be viewed in a very different light. Thus, although one out of every four cases of removal either of an ovary or limb may die, the value of the operation will be very different to the persons suffering from the complaints requiring the performance of these operations. We are here to weigh against the dangers of extirpation, a considerable chance of two or three years' existence, and a comparative freedom from pain during that time; whilst, in the other, a speedy death or great suffering is all that can be expected, unless by amputation the affected part be removed.

It is a matter of great importance to ascertain at what time after an operation there is the greatest danger to life, so that especial care may be used to defend our patients from the attacks of disease at that particular period. Many seem to imagine, that, notwithstanding the light that of late years has been thrown upon the causes of death after operations, the shock is chiefly to be dreaded; and, in conversation, usually speak of an amputation as likely to be successful, if the patient have surmounted the first depression of the operation, or of the injury for which the limb was removed.

The next table shows, however, that in the second week after an amputation, there is a very great danger of the patient being attacked by disease, and that, until he has passed the 14th or 21st day, we cannot consider his chance of recovery even tolerably good. This table has been constructed so as to exhibit the number in each period of which 1 person perished; as, for instance, at the Newcastle Infirmary, one died out of twelve in the first week, and one in fifteen in the second. To form it, the whole number of amputations were divided by the number of deaths that occurred in the first week, so as to give the average mortality in it: These deaths during this first week were then subtracted from the whole number of operations, and the remainder again divided by the deaths during the second week, so as to give its mortality. The calculations for the Glasgow Infirmary were made in the same manner, from the times at which the cases of amputation died, as given in Dr Lawrie's valuable paper in the Medical Gazette for December 1840.

TABLE II.

Showing the number of Cases of which 1 has died in each week after Amputation of the Limbs, in the Newcastle and Glasgow Infirmaries.

Hospital.	WEEKS.								
	One	Two	Three	Four	Five	Six	Seven	Nine	Fourteen.
Newcastle Infirmary, .	12.33	15.69	27.28	26.28	59.	...	174.	173.	...
Glasgow Infirmary, ...	5.79	12.63	16.	30.	38.66	37.66	55.	...	108.

As the average time required for the recovery of those persons who were dismissed cured, was found to be in the Newcastle Infirmary upwards of seven weeks, it is evident that, after an amputation, a tendency to fatal diseases is set up, which, after a certain lapse of time, declines, whether the wound produced by the knife be healed or not; for were this not the case, the amount of deaths would be more equal in the second, third, and fourth weeks, than it appears to be in Table II. It is also somewhat remarkable, that although, in the Glasgow Infirmary, 1 out of 16 died in the third week after an amputation, and only 1 in 27·28 in the Newcastle Infirmary, yet that the time of cure in the former was only forty-two days, whilst in the latter it extended to fifty-two days. It is worthy of observation, that, whilst in Glasgow, the mortality in the first week was 1 in 5·79; at Newcastle, it was only 1 in 12·33. This difference, I conceive, arises from the greater proportion of traumatic operations in the practice of the former hospital, and, in order to illustrate this opinion, I have added the average mortality which took place at both hospitals in the first seven days after the amputations.

TABLE III.

Showing the Number of Cases of which 1 has died in each day, within the First Week after the performance of Amputation of the Limbs, at the Newcastle and Glasgow Infirmaries.

Hospitals.	DAYS.						
	One.	Two.	Three.	Four.	Five.	Six.	Seven.
Newcastle Infirmary,.....	74·	31·28	106·	105·	208·	...	69·
Glasgow Infirmary,	24·	53·7	26·3	76·	30·	48·3	47·3

The difference is here exceedingly striking, more than three times the number having sunk during the first twenty-four hours in the Scotch Hospital; and although, in the following day, the amount of deaths has been greater at Newcastle, yet, in the succeeding periods, we still find a much greater fatality in the former. In order, however, to make these figures of any practical use, we must ascertain the nature of the affections producing death; and, not being able to bring forward any statistical statement from the Newcastle Infirmary on this point, I have collected the accounts of as many *post-mortem* examinations as are reported in the various records before quoted. The number of deaths amounts to 138, and the causes are as follows:—

TABLE IV.

Showing the Number of Deaths caused by different Diseases after Amputation of the Limbs, their proportion, and the number of Cases which may be expected to Die from each Class of Diseases in every 500 Amputations which may be performed in the Civil Hospitals of Great Britain.

	No. of Deaths.	Proportion to the total number of Deaths.	Proportion in 500 Amputations in British Civil Practice.
Shock, exhaustion, and delirium,.....	23	1 in 6'	} 27·82
Gangrene of stump,.....	7	" 19·71	
Secondary hemorrhage,	4	" 34·5	} 3·89
Tetanus,	4	" 34·5	
Erysipelas,	6	" 23'	} 80·62
Visceral inflammations,	21	" 6·57	
Diseased viscera,	6	" 23'	} 80·62
Purulent deposits,	29	" 4·75	
Phlebitis,	20	" 6·9	} 12·07
Phlebitis and purulent deposits,	5	" 27·6	
Diarrhœa and hectic,	12	" 11·5	} 12·07
Bed sores,	1	" 138'	
	138		

Here, then, are a number of diseases following amputation, differing both in their nature, in the organs they attack, and in the amount of death they produce. We observe some causing only one fatal result in every 34 deaths, whilst another appears to be the means of terminating life in one-fourth of the unfortunate cases of the operation. We before observed, that the operators connected with the largest Hospitals, had to lament a considerable number of their patients carried off by death after amputations; and we now see, by glancing over this table, how little manual dexterity can influence their success.

And, yet, works upon operative surgery are daily written, which detail with the greatest accuracy the formation of flaps, and lay down to a hair's breadth the extent of incisions, without mentioning the secondary affections liable to occur after the operation. Huge dictionaries issue from the press, discussing the rival merits of ancient authorities to some trifling improvement in the method of amputating, but forgetting to lay down rules for the prevention or detection of the many dangerous diseases which so often follow its performance. How many Hospital surgeons seem to imagine that the necessity for their personal attention to a case of amputation, terminates at the door of the operating theatre, and leave the after treatment to be solely directed by a house surgeon or a dresser! How many content themselves with a hurried inspection of the pulse and tongue of the patient, after an amputation, instead of exploring the chest, or watching for the first indication of phlebitis; and how many young practitioners do we not find, who speak of an amputation as though the interest connected with it ended with the operation, and as though success were certain if the patient surmounted the shock, or be unaffected with hemorrhage!

These causes of death may be readily classified: thus, 30 out of 138, or 1 in 4·6 died from the shock, in one form or another. In these cases, the death seems to result from the sympathy of the nervous

system with the part of the body which is injured, no anatomical alteration of the nervous centres being perceptible on *post-mortem* examination.

I have included under this head, delirium and gangrene of the stump. The former is usually observed as following accidents and operations in persons of intemperate habits; but occasionally I have seen it as a consequence of shock in the case of healthy and temperate individuals, and have observed it to destroy life within one or two days after the operation. Gangrene, if it be not the re-appearance of the disease which necessitated the removal of the limb, is, in most cases, produced by want of power in the system, and may, therefore, be appropriately classed as the result of exhaustion. Supposing, then, that these numbers represent the usual amount of death, we should expect that, out of every 500 amputations, upwards of 27 would perish from this class of diseases.

Secondary hemorrhage in civil practice, although it is stated to occur once in every 13 cases of amputation, is nevertheless an infrequent cause of a fatal termination; only 4 deaths out of 138 being referrible to it. In military practice it seems more frequent: thus Mr Alcock estimates its occurrence, according to his own observation, as taking place once in every five amputations, and as causing death in 5 in 109 cases. I am unable to bring forward any statistical statement of its frequency at the Newcastle Infirmary; but am inclined to imagine that the proportion of 1 in 13, quoted from Dr M'Hardy, would be about the average of its occurrence; and yet, during the seven years that I lived within the Hospital, no death was caused by it. In the Operation Register, a case is mentioned as having taken place twenty years ago, in which amputation was performed on an old man on account of ulceration of the leg; the arteries were found ossified, and gave way in the application of ligatures; a considerable amount of blood was lost; and although, finally, the femoral artery was secured higher up in the thigh, the patient sank on the third or fourth day after the operation.

In the diseases we have just mentioned, we cannot perhaps anticipate any great improvement in our means of cure; for we may always expect that a certain proportion of those undergoing amputation will be in such a state of health at the time of its performance, that death will ensue from exhaustion. Perhaps, as our science improves, the mortality from shock may rather increase, as in many cases we may be inclined to give a chance of life, where now we leave the patient to his fate. But it is widely different in the class to which we have next to direct our attention: it consists of diseases of various parts of the body, attended with alterations of their structure, appearing some days or weeks after the operation, and, when fully developed, usually proving fatal to the patient:—91 out of 138 deaths took place from them, which is in the proportion of 84 deaths in every 500 amputations. It may seem strange, that in this class I have included tetanus; but although it is frequently considered a

functional disease, yet there is, I think, considerable probability, that at some future time some structural change will be discovered in the spinal chord. Although only 1-34th of the number of deaths occurred from it, there is every reason to suppose that even this amount is above the general average in this country, and that some of the above were cases, in which removal of an injured limb had been unavailingly tried as a means of stopping the symptoms of the complaint.

It speaks well for the management of the Hospitals, from which these numbers are taken, that so small a proportion as only 1 in 23 deaths has arisen from erysipelas.

Judging from my own observation, I fear we should have had to reckon a somewhat larger number had the causes of death at the Newcastle Infirmary been added to this table, as it has frequently appeared in the wards, and, on more than one occasion, I have seen persons sink from it after submitting to operations of a trivial character.

We find that 27 had died of visceral inflammations and diseases, or in the proportion of 25 in every 500 amputations. We can more readily explain how an attack of erysipelas may take place, than by what means an inflammatory affection of the internal organs occurs after the removal of a limb. Almost every organ has, however, been found diseased after amputation, though chiefly those which contain the largest amount of blood in a state of health, as the lungs and liver. Some persons have believed, that in these visceral inflammations the organs affected had been previously unhealthy, and that the operation had only acted by rousing the latent disease into fatal activity. I have certainly observed some cases which supported this view in a most striking manner; but the large proportion of individuals so affected after operations, must at once overthrow the idea of this being the general cause of their production. Others have attributed them solely to the ordinary exciting causes of inflammation acting upon a habit enfeebled by chronic diseases; but this theory must also fall to the ground, as it will be afterwards shown, that whereas, in amputations from pathological causes at the Newcastle Infirmary, only one died out of every 5·14, one died out of every 3·11 in those performed for accidents. It has been surmised by others, that the depression of the shock is the usual cause of these complaints, by producing a typhoid condition of the system, and that, in proportion to the amount of shock, is the danger of the occurrence of secondary inflammation.

In support of this opinion may be quoted the fact, that traumatic amputations, which are most liable to death from shock, are also most subject to visceral diseases.

But this opinion appears to be disproved by the following facts:—In Table X., it will be seen that in the Glasgow Infirmary, although the deaths from shock were one in every 12·8 in the primary, and one in every 6· in the secondary amputations, the mortality in both from secondary inflammations was nearly equal; and, in Mr Alcock's practice, one in every 8·66 died from shock in the secondary, and

only one in every 57 cases of the primary amputations, and yet no instance of phlebitis or disease of the viscera was observed in the former; whilst 1 in 9·5 died from phlebitis, and 1 in every 3·56 of disease of the viscera in the latter. And again, in Table XXXVI. it will be observed, that, between 10 and 20 years of age, the mortality during the period of shock is 1 in 2, and yet no deaths had taken place out of 4 after the first week; whilst, amongst those between 20 and 30 years of age, although the mortality was only 1 in 12 in the first week, it amounted to 1 in 2·75 after that time.

The truth, I imagine, will be found to be, that the exciting causes of these secondary diseases are various; but that a predisposition is given to inflammatory diseases by the removal of any large portion of the body. For, after this operation, the heart and arteries will contain more blood than is required for the body, whilst a less proportion of venous blood will be formed, on account of its passing through a smaller extent of the capillary system; and, therefore, this fluid will be more highly stimulating than it should be in a state of health, especially when the patient has not been reduced by previous illness. We should also add to this predisposing cause those above mentioned; viz., that where previous disease has existed, it will be liable to be brought into activity by the state of the patient after the operation; and also, that after a certain length of time, if the patient be much weakened, either by fever or by discharge, pneumonia will be as likely to take place as it usually is in other persons in a similar state of health, but in whom it is produced by any other predisposing cause.

It is only within the last few years that any satisfactory explanation has been given of the occurrence of purulent deposits, in different organs of the body, after an accident or operation. The passage of pus into the circulation has, however, been proved to be the general cause of this fatal form of disease; and the further the subject is investigated, the more are we surprised at the frequency with which it takes place after operations.

More than one-third of the whole number of deaths after amputation, seem to have arisen from this cause. I have included purulent deposits and phlebitis under one head, as in the majority of cases the former result from the latter; and often, where the more obvious diseases of the viscera have alone attracted the attention of the pathologist, there is every reason to believe the primary cause of death had been overlooked, from want of due care in the examination. When, however, we reflect upon the disposition of veins to inflammatory action, we need not be surprised that it so frequently follows amputation. We see phlebitis set up idiopathically, we observe it arising from the passage of a catheter, from small scalp wounds, from an issue near a vein, and from many other equally trivial operations; and how can we expect that these vessels will remain unaffected in the inflammatory attacks of the stump, which are so often observed after the removal of a limb? We should likewise remember, that, in taking up the smaller arteries, the accompanying veins must be

frequently included in the ligatures, which, of itself, is sufficient to give rise to the disease. Besides, occasionally, ligatures are placed upon the larger veins through carelessness; thus, I have seen a surgeon of great experience tie the femoral vein on account of a little bleeding, which by other means might have been stopped, and urge as an excuse for his conduct, that, notwithstanding what had been written upon the subject of phlebitis, he had often adopted the plan without any unpleasant results.

The small proportion of deaths from secondary exhaustion, as from hectic, diarrhœa, &c., is very remarkable when contrasted with the large number caused by inflammatory affections. Only $\frac{1}{10}$ th of the whole number of deaths is referrible to this class, or of 500 amputations in British civil practice, we may expect but 12 to perish from this class of diseases. Even this proportion is perhaps a little too high, as, under the head of diarrhœa, I may have included some who have sunk rather from muco-enteritis than from exhaustion; since in the next table it will be shown, that 2 of the deaths from this cause occurred at an early period after the operation.

That the surgeon should be acquainted with the proportion in which these diseases appear after an amputation, is of obvious importance; but the value of this knowledge is greatly increased, if he is also aware of the time at which each is most liable to occur. In order to assist in attaining this, I have added the following table, drawn up from the report of Dr Lawrie.

TABLE V.

Showing the Number of Deaths which, at the Glasgow Infirmary, had taken place from various Diseases, at different periods, after Amputation—calculated from Dr Lawrie's Table.

	Within 4 days.	4 to 7 days.	2 weeks.	3 weeks.	4 weeks.	5 weeks.	6 weeks.	7 weeks.	14 wks.
Shock, exhaustion, and delirium,.....	7
Secondary hemorrhage,.....	3	1	...
Gangrene of stump,.....	4	2
Tetanus,.....	1
Erysipelas,.....	...	2	1	1
Visceral inflammations,.....	1	2	2	2	...	1	1	...	1
Rigors, phlebitis, and purulent depos.	1	4	6	4	4	2	1	1	...
Diarrhœa,.....	1	1	2	1
Bed sores,.....	1
	18	11	11	8	4	3	3	2	1

At different periods after an amputation, different diseases are liable to occur. Thus, taking the first period of 4 days for our consideration, and rejecting tetanus, which appears to have been the cause and not the consequence of the operation, we find, that out of 17 deaths, only 2 or $\frac{1}{8}$ th of the whole had died from organic diseases, 1 had perished from diarrhœa, and the remainder were cut off, either by the shock of the operation, or by hemorrhage. The first 4 days after an amputation may be therefore termed the period of shock, or that in which the danger from primary exhaustion is chiefly to be dreaded; and the number dying in it should always be stated in any report where the real causes of death cannot be ascertained.

Although after the 4th day the influence of shock is to be but little dreaded, yet 2 deaths from gangrene occurred in the next period, one on the 5th and the other on the 7th day, after which no other from the same cause is recorded. If, then, a patient have survived the 4th day, our fear of his death resulting from the exhaustion of the shock, or from delirium, may as a general rule subside, and after the 7th day we need not even dread the secondary effects of the shock in the shape of gangrene of the stump. But after the 4th day a new class of diseases attains a greater prominence than before, and in this class we first observe 2 deaths from erysipelas.

Now, although we see persons die from this affection at all periods after an amputation, yet it will, I think, be found that the general time for its attack is during the first or second week. In this report no death from erysipelas had taken place after the 17th day. We observed, that even by the 4th day cases of death from phlebitis and visceral diseases had occurred, but there were only two during the first 4 days; whereas, after the shock of the accident or operation has subsided, these affections are so rapidly produced, that between the 4th and 7th days no fewer than 6 deaths are registered as produced by them. From this time they gradually decline in frequency, the next period of 7 days showing only 8 deaths, and the third week only 6. But, if we consider the mortality produced by them in another point of view, we find it is as great in the second as in the first week, and that the surgeon has as great a chance of losing his patient from this class of diseases, after the 14th day, as before that time. It may be likewise remarked, that even in the 14th week a case of death had taken place from visceral inflammation; and as these diseases are most frequent in hospitals, it may be suggested, that as soon as the condition of the patient will admit of it, he should be removed to a less crowded residence than the ward of an infirmary.

The proportion of deaths from diarrhoea is very large in this report, and Dr Lawrie classes it among the secondary inflammations, a classification which the time at which it produced death certainly appears to favour. If I were to judge from my own experience, I should be inclined to say, that, in most hospitals, diarrhoea is both a more unfrequent cause of death than here stated, and that it usually takes place at a later period after the operation.

2.—*The success of an Amputation varies according to the part of the limb at which it is performed.*

In order that statistical researches may be applied to direct our prognosis, it is necessary that every circumstance connected with the facts should be considered. Thus to know that one out of every four who suffer amputation perishes, is interesting as giving us a definite idea of our success; but it does not enable us to declare the chance of life which any particular patient can be expected to possess. To do this, we must collect all the favourable and un-

favourable points connected with each case, and then, by ascertaining the results of a number agreeing in these particulars, we shall be able pretty accurately to define the probable chance any patient suffering amputation, when placed under the same circumstances, will have.

These remarks should be borne in mind when we look at the mortality following each amputation, lest we should draw from it erroneous conclusions.

As I was unable to give the real average of success at the Newcastle Infirmary, from it not being usually mentioned at what part of the limb the operation was performed, I have contented myself with the approximative statement contained in Table VI.

TABLE VI.

Showing the success of the Amputations at the Newcastle Infirmary, when they are classified according to the part at which the Accident or Disease existed which required their performance.

	No.	Dead	Average Mortality.	Av. time of Cure.	Av. Dismissal as O. P.	Av. time of Death.
Amput. for injuries and diseases of thigh and knee,...	58	14	1 in 4.14	51.1	67.8	12.28
Do. do. leg and ankle,.....	112	29	3.86	54.9	55.1	17.
Total of lower extremity,	170	43	1 in 3.95			
Amput. for injuries and diseases of arm and elbow,...	40	7	1 in 5.71	50.8	93.	17.42
Do. do. fore arm and wrist	15	4	3.72	42.54	...	6.25
Total of upper extremity,	55	11	1 in 5.			

TABLE VII.

Showing the Mortality after Amputations at different parts of the Lower Extremity, as observed in various Hospitals.

AMPUTATIONS OF LOWER EXTREMITY.

Hospital or Authority.	Hip Joint.			Thigh.			Leg.			Knee Joint.		
	No.	Dead.	Aver. Mort.	No.	Dead.	Aver. Mort.	No.	Dead.	Aver. Mort.	No.	Dead.	Aver. Mort.
Liverpool Northern Hospital,	29	4	* 7.25	40	9	4.44
Edinburgh Infirmary,	25	11	2.27	12	4	3.
Glasgow Infirmary (Lawrie),	1	128	46	2.78	62	30	2.06
Glasgow Infirmary,	50	18	2.77	48	14	3.42	1	1	1.
Five Scotch Hospitals,	6	2	3.	7	...	0 in 7
Royal Berks Hospital,	12	5	2.4	4
Chester Infirmary,	13	5	2.6	6	4	1.5
University College Hospital,	22	4	5.5	26	4	6.5
Dr McHardy,	202	56	3.6	55	11	5.
Liverpool Infirmary,	11.	6.
Massachusetts and Pennsylvania Hospital,	50	15	3.33	49	12	4.08
Hotel Dieu (M. Roux),	16	10	1.6	12	5	2.4
Do. (1836 to 1841),	63	43	1.46
M. Malgaigne,	1	1	1.	201	126	1.57	192	106	1.81	9	7	1.28
Mr Alcock,	43	27	1.59	11	4	2.75
Lyons Military Hospital,	4	3	1.33	5	3	1.66
Paris Military Hospital,	5	1	5	5	3	1.66
Various Authorities,	1.42
			1.42	869	376	2.31	534	209	2.55	10	8	1.25

* Whenever the term "average mortality" is employed in these tables, the figures placed under that head must be understood as showing the number of

TABLE VIII.

Showing the Mortality after Amputations at different parts of the Upper Extremity, as observed in various Hospitals.

AMPUTATIONS OF UPPER EXTREMITY.

Hospital or Authority.	Shoulder Joint.			Arm.			Fore Arm.		
	No.	Dead.	Av. Mort.	No.	Dead.	Av. Mort.	No.	Dead.	Av. Mort.
Liverpool Northern Hospital,	2	1	2	17	4	4.25	7	...	0 in 7
Edinburgh Infirmary,	3	2	1.5	1	...	0 in 1	2	1	2
Glasgow Infirmary (Lawrie),	6	3	2	53	21	2.52	22	...	0 in 22
Do. Do.	8	3	2.66	16	5	3.2	17	3	5.66
Five Scotch Hospitals,	4	...	0 in 4	5	...	0 in 5
Royal Berks Hospital,	3	...	0 in 3	4	...	0 in 4	4	...	0 in 4
Chester Infirmary,	2
Massach. & Pennsylv. Hosp.,...	2	1	2	15	2	7.5	21	1	21
Hotel Dieu (M. Roux),	3	2	1.5	4
Dr M'Hardy,	54	14	3.85	53	2	26.5
Mr Alcock,	10	2	5	38	19	2	9	4	2.25
M. Malgaigne,	14	11	1.27	91	41	2.21	23	8	3.5
University College Hospital, ...	1	8	2	4	8
Lyons Military Hospital,	2	1	2	7	6	1.16	1
Paris Do. Do.,	2	1	2	4	2	2
Newcastle Infirmary,	5	2	2.5
	58	27	2.14	317	118	2.81	181	19	9.52

It is obvious that, however the practice at various hospitals may differ, their returns all agree in displaying a greater mortality after amputations of the lower extremity than after those of the upper.

The removal of a limb at the joint seems much more dangerous than when the bone is divided: thus, we find 8 died out of 10 on whom amputation at the knee joint was performed; whilst, according to the statement of Malgaigne, in the hospitals of Paris, only 1 died out of 1.81 when the operation took place below the knee, and only 1 in 1.57 when performed above it. Again, after amputations at the shoulder joint, the mortality is greater than after those of the thigh or of the leg, and it is much more than what is observed after the removal of the arm at any other part. We have not sufficient numbers to decide whether it is safer to remove the fore-arm above the wrist, or at the joint, no death having occurred of 7 operations performed at the latter, and the average mortality of amputations above the joint being only 1 in every 9.52. The only case recorded in these hospitals of removal at the ankle joint terminated fatally.

cases of which one has died; thus, in Table VII., the average mortality following amputations of the thigh, is stated to be 7.25 in the Liverpool Northern Hospital, and 2.27 at the Edinburgh Infirmary, the meaning of which is, that one person has died of every 7.25 on whom amputations of the thigh have been performed in the former, and that 1 has perished of every 2.27 cases of the same operation at the latter hospital. The figures placed under the heads of "average time of cure," "average time of dismissal as out patient," and "average time of death," show the number of days that each person, on an average, was dismissed from the hospital, or died after the performance of the operation.

When we consider the shock produced by the removal of the whole of the lower extremity, the vast size and number of the vessels and nerves divided, their contiguity to the important viscera of the abdomen, and the general disarrangement which all the functions of the body must experience from the loss of so large a portion of the frame, we should rather feel surprised that so many had recovered, than that so large a number had perished after amputation at the hip joint, and should view the success of this operation as one of the proudest triumphs of operative surgery.

The small difference between the mortality following amputations of the leg, and that succeeding those of the thigh, is well worthy of notice; that of the former being 1 in 2·55, that of the latter 1 in 2·31. In some instances, indeed, as at the Liverpool Northern Hospital, and at the Glasgow Infirmary, the advantage has been with the thigh operations.

The conclusion would appear therefore inevitable, that if amputations, in general, greatly decrease in danger the further they are performed from the trunk, there must be some peculiarity in the structure of the parts divided in amputations below the knee, which renders this operation an exception to the general rule. But if we examine the matter more closely, we shall discover the cause of this apparent exception, in the fact that a greater number of more fatal diseases require amputation of the leg than of the thigh. Thus, if we take the results of the operations performed for diseased joints and bones alone, we find a mortality of 1 in 4·91 after amputations above the knee, and only 1 in 10·33 in those performed below it; or, if we compare those required for accidents, it is 1 in 1·55 in the former, and 1 in 1·93 in the latter. Here, then, are evident proofs that the exception is more apparent than real, and it shows us the necessity of caution in drawing conclusions from statements of a general nature.

The amputation below the knee is usually considered an exception to the general maxim, that no more of a limb should be removed than is absolutely necessary, and the part usually selected for its performance has been named the point of election. But while we consider that the mortality after an amputation decreases in proportion to the distance from the trunk at which it is performed, doubts naturally arise in our minds respecting the propriety of unnecessarily removing any portion of the body, more especially as it is not impossible, that some cheap description of artificial leg might be invented for those submitting to the operation above the ankle, which might be equally convenient with that usually worn upon the stump below the knee. But what are the results of these two operations, viz., that below the knee, and that above the ankle?

We have found the average mortality in the former to be 1 in 2·55, whereas in 9 supra-malleolar amputations only 2 of the patients died, or 1 in every 4·5. In addition to this statement, MM. Arnal

and Martin, of 97 cases of supra-malleolar amputation, state the number of deaths to have been only 1 in 10. Before, then, the surgeon amputates for a diseased foot or ankle joint, let him fancy himself to be the patient, let him coolly consider the figures just quoted, and calculate the different chances of death to which these two operations would expose himself. And then, if he can believe the greater inconvenience of the lower operation to outweigh its greater comparative safety, let him advise his patient to submit to the removal of the limb at the usual point of election. In the upper extremity, as we before remarked of the lower, the mortality decreases according to the distance at which it is performed from the trunk. Thus, 1 died in 2·14 after amputations at the shoulder joint, 1 in 2·81 after those above the elbow, and 1 in 9·52 after those of the fore-arm. Another fact is also observable in the operations on both extremities, viz. that the decrease in the number of deaths is not in any regular ratio; for had the mortality, after removal of the fore-arm, borne the same proportion to that following amputation of the arm, that the mortality of this latter does to the mortality following the shoulder joint operation, it would have been 1 in 3·68, whereas we find it only one-third of that amount, or 1 in 9·52.

It becomes us next to inquire to what causes of death each amputation is most liable, and to elucidate this point I have added the next two tables. The first is calculated from the cases contained in the register of the Newcastle Infirmary, the other is formed from the papers published by Dr Lawrie. This last table has been formed in the following manner:—He has stated the number of those who died after removal of the leg, and the causes of whose death he has been able to ascertain, to be only 20; whereas the whole number submitting to that amputation at the Glasgow Infirmary was 62, of which 30 perished. Now, according to this rate of mortality, the 20 whose immediate cause of death is registered, would represent 41 amputations. This number 41, I have therefore divided by the number of deaths caused by each disease, so as to show the mortality produced by each affection. The other divisions of the same table have been calculated in a similar manner.

TABLE IX.

Showing the number of Amputations, of which 1 died, at different periods after the performance of Amputations at different parts of the Limbs.

NEWCASTLE INFIRMARY.

	Within 4 Days.	4th to the 21st Day.	After the 21st Day.
Amputation for diseases and injuries of Knee and Thigh,	1 in 11·6	1 in 11·6	1 in 14·5
Do. do. Leg and Foot,.....	" 22·4	" 6·58	" 16·
Amputation at Shoulder Joint,.....	" 2·5
Do. for diseases and injuries of Arm and Fore-arm,	" 50·	" 6·25	...

TABLE X.

Showing the average Mortality which took place at the Glasgow Infirmary from different diseases, the Amputations being separated according to the part at which, and the cause for which they were performed.

	Thigh.	Leg.	Shoulder	Arm.	Accidents.	Diseases.	Primary Amputat.	Secondary Amputat.
Shock, exhaustion, and delirium,	9·7	20·	8·83	...	12·8	6·
Secondary hemorrhage,	24·25	53·	37·	...	21·
Gangrene of stump,	24·25	20·5	17·66	...	16·	21·
Erysipelas,	97·	13·33	26·5	...	21·33	42·
Inflammation of viscera,	16·16	8·2	...	20·	10·6	24·6	10·6	10·5
Phlebitis and purulent deposits,	19·4	3·41	2·	5·	6·23	7·4	5·81	7·
Diarrhœa,	32·33	41·	...	40	35·3	37·	32·	42·
Bed sores,	97·	106·	42·
Tetanus,	97·	106·	...	64·	...

At the Newcastle Infirmary we find, that in either extremity the nearer the amputation is performed to the trunk, the greater is the mortality during the first four days, or in that period in which we before found the results of shock were chiefly to be dreaded.

Omitting the amputations at the shoulder joint, as too few in number, we see that the danger from shock is greatest after the removal of the thigh; the average mortality in this operation being 1 in 11·6, in that of the leg 1 in 22·4, and in that of the upper extremity 1 in 50, during the first 4 days. The same conclusions may be deduced from the statements of Dr Lawrie; for, according to Table X., the average mortality from shock, was in the thigh operation 1 in 9·7; no death took place after amputation of the leg of 41 cases, and only 1 in 20 after that of the arm. Mr Alcock also gives the causes of death in 21 cases, of which 3 died from shock or exhaustion; of these 2 were amputations of the thigh, and the part at which the third suffered the operation is not mentioned.

Gangrene, which does not appear to have produced a fatal result in any amputation of the upper extremity, was the cause of death at the Glasgow Infirmary, of 1 case in every 24·25 of the thigh operation, and of 1 in every 20·5 of the leg.

We might have anticipated that secondary hemorrhage, occurring after amputations of the thigh, would prove peculiarly fatal, both on account of the greater size of the vessels, and also since the means required to restrain it when it occurs, some time after the operation, are attended with greater danger in that part of the body than in others. In accordance with this view, we find by Table X., that 1 in every 24 cases of amputation of the thigh had been lost from this cause, whilst no death had resulted from it in the other operations. And yet, though more frequently followed by an unfortunate result, secondary hemorrhage is not a much more frequent consequence of an amputation of the thigh than of the arm or leg. According to Dr M'Hardy's researches, 1 case in 11·22 is affected by it after the thigh operation; 1 in 13·77 after the removal of the

leg; 1 in 13·5 after amputation of the arm; and 1 in 26·5 after amputation of the fore-arm.

Erysipelas seems more frequently to affect the upper extremity than the lower; thus, according to Table X., the average mortality after amputations of the arm is 1 in 13·33; whilst no deaths had been produced by it after removal of the leg, and only 1 in 97 cases of operations on the thigh. In like manner, according to Dr M'Hardy, 1 in 67·3 was attacked by erysipelas of the thigh amputations; 1 in 27·5 of the leg; and 1 in 18 after removal of the arm; whilst none out of 53 had suffered from it after the removal of the forearm.

We might have reasonably supposed, that in proportion to the extent of the body removed, we should have observed the development of internal inflammations; but such does not appear to be the case, as visceral diseases are stated to have been twice as frequent, at the Glasgow Infirmary, after removal of the leg as after that of the thigh; and the loss of the arm has exposed the patient to but little less chance of being affected by them than if he had suffered amputation *of the thigh*. Whether this may have arisen from classing together cases differing in the diseases which have required the operation, we know not; but it is not more extraordinary than the comparative freedom from phlebitis, which the most dangerous amputation is here stated to possess. Only 1 in 19·4 of the thigh amputations had perished from the latter destructive malady; whilst of those performed below the knee, the average mortality was 1 in every 3·41; and in those of the arm, 1 in 5. At the same time, as giving some support to this remarkable statement, Dr M'Hardy's statistics show 1 person in 11 affected by phlebitis after amputation of the leg; 1 in 13·5 after that of the arm; 1 in 18·36 after removal of the thigh; and 1 in 53 after amputation of the fore-arm. Altogether, these important points require further examination before we can attempt to speculate upon the probable causes which produce these varieties.

In its liability to be succeeded by diarrhoea, and by the remaining causes of death, the thigh amputation is especially remarkable.

Notwithstanding the remark which was before made, that an hospital might show the greatest mortality, and, at the same time, the shortest average period of cure after amputations, we *usually* find the longest time required for cure to coexist with the greatest amount of death. Thus, in Table VI., the mortality in cases of amputation performed for diseases and accidents below the knee, is 1 in 3·86, and the time of cure 54·9 days; whilst in those amputations performed for similar causes above it, the mortality is 1 in 4·14, and the period of cure 51·1 days. Again, in Table XI., the mortality in diseases and accidents of the thigh requiring amputation, is 1 in 1·6, and the period of cure 70·33 days; in the leg cases the mortality is 1 in 2·71, and the period of cure 60·33 days; in amputations for accidents and diseases of the arm, the mortality is 1 in 6, and the time of cure 53·33 days; and in those of the fore-arm the mor-

tality is also 1 in 6, and the time of cure 42·4 days. I have said such is usually the case, for it is not universal: thus, in the class of diseased joints and bones, whilst the mortality in amputations of the thigh was 1 in 5·44, and the time required for cure 49·34 days, in those of the leg these conditions were reversed, the mortality being 1 in 16·33, and the period of cure 52·13 days, though at the same time it should be remarked, that the time of the dismissal of the out patients in the former class of cases was 67·8 days, whilst in the latter, the out patients were dismissed from the hospital, on the average, at the 42d day. As a general rule, we may say that amputations of the upper extremity heal more quickly than those of the lower, and, in either, the time required for cure depends considerably upon the thickness of the parts at which the operation is performed.

(*To be continued.*)

ARTICLE V.—*Contributions to Pathology and Rational Medicine.*

By JOHN HUGHES BENNETT, M.D., Lecturer on Pathology and the Practice of Physic, Director of the Poly-Clinic at the Royal Dispensary, Edinburgh, &c.

No. IX.—CLINICAL AND HISTOLOGICAL RESEARCHES ON CANCEROUS AND CANCROID GROWTHS.

OUR further knowledge of cancer, it seems to me, must be prosecuted by uniting the study of its structure in different organs with clinical observation. In this way only will it be possible for us to separate that malignant, spreading, and constitutional disorder from the numerous non-malignant and purely local diseases with which it is so frequently confounded. This once accomplished we shall be better enabled to enter upon the important subject of its treatment, and attempt a solution of the great question—"Is cancer curable or not?" Without further preface, therefore, I propose to record the result of my researches into the minute structure of cancerous and cancroid growths; give a representation of the appearances presented by a microscopic examination of all their different forms which have fallen under my observation, and connect this examination with the history of the case. Lebert and Sedillot have opened the way to the truly practical study of cancer, and I shall consider myself fortunate if this paper be regarded as a continuation of their valuable labours.

OBSERVATION I.—*Cancerous Tumour of the Breast—Excision.*

Susan Baillie, æt. thirty-seven, dressmaker, admitted February 22d into the Royal Infirmary, under Mr Syme. At the latter end of December she observed a swelling of the left mamma, attended with sharp shooting pain, which was thought to be an abscess, and treated by fomentations, &c. Under this treatment it decreased in size, leaving in its place a small tumour the size

of a horse-bean, in which she felt a sharp, gnawing pain. It has increased rapidly, paroxysms of pain coming on at intervals, attended by swelling of the arm and axillary glands of the same side. On admission, a circumscribed tumour in the left breast, about the size of a small orange, can be distinctly felt, separable from the true substance of the gland. *February 24.*—To-day Mr Syme removed the tumour by a single incision. She was dismissed with the wound completely healed, and to all appearance in good health, *March 18.*

Examination of the Tumour.—On examining the tumour I found it, as stated in the report, to be about the size of a small orange or billiard-ball, imbedded in the fatty tissue of the mamma, a layer of which surrounded it laterally and posteriorly. It was intimately connected with the substance of the gland laterally and posteriorly, into which it sent here and there short prolongations—anteriorly, it was adherent to the skin. It was of firm consistence, but somewhat softened at one spot. On section it grated under the knife, and presented a smooth, whitish, fibrous structure, except anteriorly at the softened part, which was about the size of a hazel nut, of a pinkish hue, with a small infiltration of blood in its centre, of a bright crimson colour. The circumference of the tumour generally was hard, firm, and resistant to the knife (scirrhus). This hardness diminished anteriorly towards the softened part, which presented all the characters of encephaloma, and yielded on pressure a fluid the colour and consistence of thick cream. The elliptical portion of skin removed with the tumour was healthy.

*Microscopic Examination.**—A thin section was removed from the centre of the tumour by means of Valentin's knife, and examined with a power of 250 diameters linear. It consisted of a mesh-work of fibrous tissue, forming waved bands, arranged here and there in circles, varying in size. Some of these were very large (one-fifth of a millimetre in diameter), enclosing other circles, each of which were surrounded by several filaments of the fibrous tissue. Some of these circles contained numerous nucleated corpuscles crowded together, mixed with granules; others contained only a few, or nothing but granules. Here and there were to be seen several compound granular corpuscles. The appearance of this section is very accurately represented—Fig. 1. On adding acetic acid to the whole structure it became more transparent. Many of the fibrous filaments became invisible, and such as remained were studded here and there with elongated nuclei. The walls of the corpuscles were partially dissolved, and rendered very transparent; whilst their nuclei were unaffected. A portion of the section, as seen after the addition of acetic acid, is represented—Fig. 2. The cream-like fluid, which could be squeezed from the surface of the tumour, contained, 1st, nucleated cells; 2dly, compound granular corpuscles; 3dly, numerous granules. The nucleated cells were of a round or oval form, varying in size from the 1-100th to the 1-50th, or even 1-40th of a millimetre in diameter. Some contained one nucleus, others two, of an oval form, varying in the longest diameter from 1-100th to 1-75th of a millimetre. Some of these nuclei contained one nucleolus, and others two. On the addition of acetic acid the cell-wall was rendered more transparent; the nuclei were unaffected, and appeared in consequence very distinct. The appearance of these cells, with and without acetic acid, is exhibited in the two lower figures of the woodcut. The compound granular

* The microscopic examinations were all made with Oberhaeuser's small instrument, which I cannot too highly recommend, for cheapness, portability, convenience, and excellency of definition to the pathologist. The power employed was in all cases the same, namely 250 diameters linear. The demonstrations were for the most part exhibited to the gentlemen attending my class of morbid anatomy last winter. The drawings, made from these demonstrations by myself at the time, I have since copied on wood, in which they were cut by Mr Dudley. Although on this account perhaps they want that finish and beauty which the labour of a professional artist would have given to them, I feel satisfied that they will be considered by every histologist a faithful representation of the structures described.

corpuscles and granules do not require minute description here. They were the same as have been already spoken of.—(See Contributions II. and IV.)

Fig. 1.

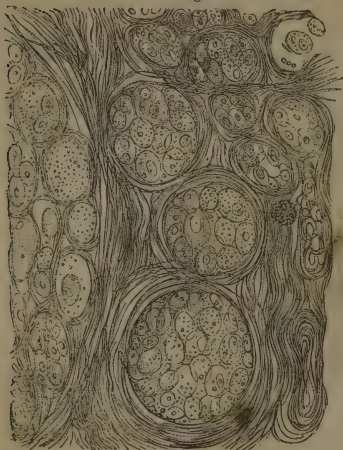


Fig. 2.

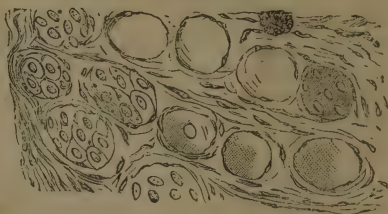


Fig. 4.



Fig. 3.

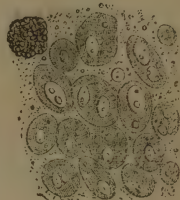


Fig. 1. Portion of the section from tumour described Obs. I., consisting of fibrous tissue and cysts, enclosing cancer cells and granules. A compound granular corpuscle is also visible. *Fig. 2.* Another portion of the same section treated with acetic acid. The fibrous tissue is rendered more transparent, and elongated nuclei are visible scattered throughout it. The nuclei of the cancer cells are unchanged, while their walls are very transparent. A compound granular corpuscle is seen at the upper part of the figure. *Fig. 3.* Cancer cells from the cream-like juice squeezed from the tumour. Numerous granules and a compound granular cell are seen. *Fig. 4.* The same after the addition of acetic acid.

Remarks.—This case was a very characteristic one of the so-called scirrhus of the breast. I therefore have selected it as a standard of comparison, presuming that if any growth deserves the name of cancerous or malignant, it will not be denied to this one. I spent three entire days in its microscopic examination, and making numerous drawings, a selection from which has been cut on wood. Sections with Valentin's knife were made of it in all directions, and the fluid it contained examined at all points. In short, I spared no possible pains to make out every anatomical fact in connexion with the tumour. The result of the examination was, that it was composed of a fibrous matrix, arranged so as to form numerous minute cysts, within which, as well as between the meshes formed by the fibrous tissue, numerous nucleated cells had developed themselves. These cells were characterised by the presence of a double or divided nucleus, within which might be observed a double or divided nucleolus. This structure may be denominated the cancer cell. On the addition of acetic acid, the cell-wall was partially dissolved and rendered very transparent, whilst the nucleus was unaffected. The fibrous tissue at the same time exhibited numerous elongated nuclei, which were seen in the waved filaments as well as those which formed the concentric circles constituting the walls of each cyst.

I may further notice that no distinct line of demarcation could be discovered between the morbid growth and the healthy tissue of

the mamma, the fibrous tissue of the one being continuous with that of the other, and the adipose tissue of the gland encroaching here and there upon, and portions of it being occasionally embedded in, the substance of the tumour.

Several other tumours of a like appearance to the naked eye which I have examined, presented the same structural arrangement. In a growth about the size of a pigeon's egg, excised from the breast by Mr Spence, which he kindly allowed me to examine, its section was so exactly similar under the microscope, that the same drawings might serve for both specimens.

OBSERVATION II.—*Tumour apparently Fibrous, but really Cancerous, of the Neck—Excision.*

May 8th, 1847.—I received for examination the half of a tumour, which was of several years growth, and had been excised from the region of the parotid gland. It was of a globular form, about three inches in diameter, with a small mamellated projection at one point. It had been carefully dissected out from the neck, its surface being smooth, and distinctly defined. On section it was resistant, and crunched under the knife. It presented a tolerably even dull-white surface, uniform throughout, of tolerably hard consistence, and was pronounced, after the most careful inspection, to be fibrous. At the same time it did not present any distinct fibrous appearance, and, on being broken up, it was observed to break across abruptly. One of the clerks in the Infirmary (Dr Gairdner) who examined it, likened its general appearance and texture, with considerable truth, to that observed in a hard pudding made of ground rice. No cream-like fluid could be obtained from it on pressure, although a pulpy substance could be obtained from its surface easily, on scraping with a knife.

Microscopic Examination.—On making a thin section with Valentin's knife, it was seen that this tumour, like that described Obs. I., was composed of fibrous tissue, arranged here and there in concentric circles or cysts, differing in size, but generally measuring from the 1-50th to 1-20th of a millimetre in diameter—Fig. 5. The fibrous filaments in many places were exceedingly fine, and mingled with numerous fusiform corpuscles. Within the cysts nucleated corpuscles were contained, the walls of which were rendered very transparent by acetic acid, while the nuclei were unaffected—Fig. 6. The fibrous tissue did not present the same wavy character, nor did it seem to be so well developed as in Obs. I. On examining the pulpy substance removed by scraping the surface of the tumour, it was seen to contain numerous nucleated corpuscles, with some fusiform cells—Fig. 7. They were mostly of round or oval form, but some were distinctly caudate, heart-shaped, or somewhat square. Several contained two nuclei, each with one nucleolus, others a single nucleus with two nucleoli.

Fig. 5.

Fig. 6.

Fig. 7.

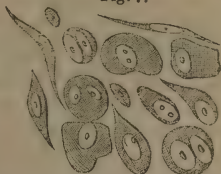
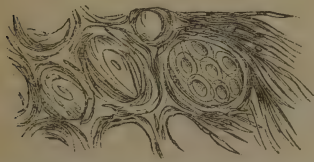


Fig. 5. Portion of a section from tumour described, Obs. II., consisting of very fine fibrous tissue, mixed with fusiform cells, and cysts inclosing cancer cells. Fig. 6. Another portion of the same after the addition of acetic acid. Fig. 7. Cancer and fusiform cells, scraped from the surface of the tumour.

Remarks.—It is necessary to remark, that whilst the corpuscular element and the minute structure of this tumour were almost iden-

tical with that observed in Obs. I., its appearance to the naked eye was very different. It was considered by all who examined it to be simply fibrous; it was so considered before and after removal by the surgeon who operated; the morbid growth was distinctly limited and defined, and admitted of being carefully dissected out. Yet its minute structure, as shown by the microscope, was seen to consist of the same elements, similarly arranged as in the former tumour, which presented to the naked eye very opposite characters, and was at once recognised by every one who saw it to be malignant. I consider this to be a very important fact, and one which will go far to explain many of the anomalies and contradictions which have prevailed among practical men concerning this subject.

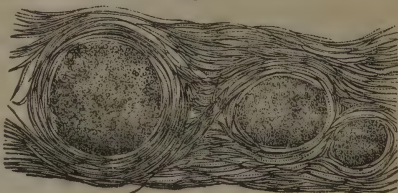
OBSERVATION III.—*Fibro-cystic Tumour of Breast (Cystic Sarcoma)*—*Excision.*

A married woman, æt. thirty-six, without children, had observed a tumour in the right mamma eighteen months ago, the size of a hazel-nut. When she applied to Dr R. Mackenzie, she stated that it had regularly progressed in size, and was now as large as an adult human hand. At one point the skin is slightly ulcerated from pressure, and bleeding had taken place from this point more than once. She complained of pain only from its weight and size, and had none of the lancinating severe pains so common in cancerous tumours. It was smooth on its surface, round, and moveable. A cyst was punctured in it, and a quantity of glairy fluid removed. The mamma was excised in the beginning of May by Dr Mackenzie, by a double elliptical incision. The wound healed readily, and the cicatrix is now (September) sound.

Description of the Tumour.—The tumour consisted of a dense mass, of globular form, weighing 7lbs. The portion of skin on its anterior surface was healthy, with exception of the ulcer from pressure formerly noticed, and a layer of adipose tissue was attached to it here and there, posteriorly and laterally. On section it presented a smooth surface, of a yellowish fawn colour, of a faintly fibrous texture, with here and there distinct cysts, varying in size from a pea to that of a pigeon's egg. These cysts were lined with a smooth serous membrane, and filled with a glairy, glutinous, transparent fluid, of a pale straw colour. The tumour presented the characters of so-called cystic-sarcoma, and a glairy transparent fluid could be squeezed from it, or removed from its surface by scraping with the knife.

Microscopic Examination.—Numerous sections were made of this tumour in different directions with Valentin's knife. It was found to be composed of a fibrous texture, the filaments of which were interlaced together in some places, forming waved bands of filamentous tissue, occupying the entire field of the microscope. In other places cysts were formed, as in the two former Observations, the walls of which were composed of fibrous filaments and fusiform corpuscles, as in Obs. II. The contents of those cysts varied considerably. Some were occupied by a coagulated exudation, composed of minute molecules and granules, as in Fig. 8. Others contained numerous corpuscles of round or oval form, varying in size from the 1-100 to the 1-50 of a millimetre in their longest diameter, generally of the latter size. They each contained a single oval nucleus, generally 1-100 of a millimetre in diameter. These cells occurred in groups, underwent a slight change in form from contiguous pressure, and were only slightly affected by

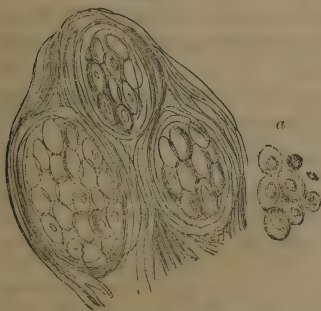
Fig. 8.



Fibro-cystic structure in a portion of the Tumour described Obs. III. The cysts containing granular exudation.

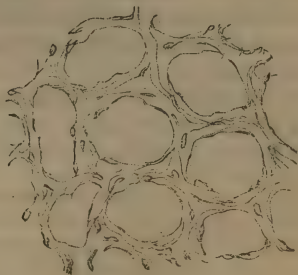
acetic acid.—(See Fig. 9.) Occasionally the fibrous tissue was arranged so as to form groups of loculi, generally about the 1-25th of a millimetre in diameter, containing a structureless, gelatinous fluid, as in Fig. 10. Acetic acid everywhere rendered the fibrous tissue more transparent, and rendered the elongated nuclei very apparent.—(Fig. 10.) The glairy, gelatinous fluid was for the most part structureless, but sometimes contained some of the cells above described, or fusiform corpuscles.

Fig. 9.



Fibro-cystic structure, in another portion of tumour described Obs. III. The cyst contained fibro-plastic cells; (a) a few of the latter after the addition of acetic acid.

Fig. 10.



Fibrous structure with loculi, in another portion of tumour described Obs. III., after the addition of acetic acid.

Remarks.—The fibrous element in this tumour exactly resembled that which existed in Observations I. and II., and was arranged in the same manner. The corpuscular element, however, was very different. In no case did the cells contain a double nucleus or nucleoli. They in every respect corresponded to those denominated *Fibro-plastic* by Lebert.¹ Their general size was much smaller than that of cancer cells, and they were not accompanied by numerous granular, or compound granular corpuscles. The action of acetic acid upon them was also very different. The division of the nucleus being nowhere perceptible, we may presume that these cells do not possess the power of reproduction. Hence a marked difference between them and those described in the two former observations. The coagulated granular exudation, occupying some of the cysts, is an occurrence which I have only met with in this tumour.

A fibro-cystic tumour removed from the breast of a lady, June 17th, by Mr Syme, composed of numerous minute cysts, containing a gelatinous yellow fluid, and embedded in fibrous tissue, presented the same structure represented Figs. 9 and 10.

(To be continued.)

¹ Physiologie Pathologique. Vol. ii. p. 123.

ARTICLE V.—*On the Changes which the Albuminous Substances undergo in the Stomach during the Process of Digestion.* By G. J. MULDER, Professor in the University of Utrecht. Translated from the Dutch by Dr AUGUST VOELCKER.

I HAD shown last year (Scheikundige Onderzoekingen, Deel III., p. 470), that the fibrin of the blood undergoes no change of composition by solution in muriatic acid and precipitation by carbonate of ammonia.

The results of my analysis, employing in the present instance my last experiments on the amount of sulphur in these substances, were as follows (Scheik. Onderz. Deel IV., p. 202):—

Undissolved Fibrin.		Dissolved and thrown down with carbonate of ammonia.	
C	- - 52·7		52·7
H	- - 6·9		6·9
N	- - 15·4		15·8
O	- - 23·5		23·5
S	- - 1·2		1·1
Ph	- - 0·3		

The phosphorus was not determined in the dissolved portion, but as vitellin loses phosphamide under the influence of acetic acid and ammonia,* it is probable that fibrin will have been deprived of the phosphamide under the influence of the muriatic acid and carbonate of ammonia.

The same experiments were repeated on casein and albumen, and the following results obtained.

A small quantity of muriatic acid was added to milk; a precipitate fell, which was washed for a long time with water. At last the mass began to be gelatinous; in this state it was mixed with water, and laid aside at a temperature of about 104° F. After some hours the whole was dissolved, and the butter rose to the top. The watery solution was decanted, thrown down by carbonate of ammonia, and the precipitate washed completely with water, alcohol, and ether, and dried at a temperature of 262° F.

0,5568 grammes produced 0,0033 ash.

- I. 1586 grms. gave, after being burned with caustic soda and nitre, 0,07 Ba O, S O³.
- II. 1,914 grms. produced 0,101, Ba O, S O³.
- I. 0,6512 of the substance without ash, gave 83 C C of moist N, at 16°, 5 C, and 766 m.m.
- II. 0,6542 of the substance without ash, 85 C C of moist N, at 16°, 5 C, and 766 m.m.
- I. 0,5652 of the same substance, 1,1065 C O² and 0,3564, H² O.

* Von Baumhauer, in Scheik. Onderz. Deel III. p. 284.

Undissolved Casein.*

C 53·8
H 7·1
N 15·6
O 22·6
S 0·9

Dissolved and thrown down.

I.

53·44
7·01
15·01
23·93
0·61

II.

53·08
6·93
15·30
23·96
0·73

Casein, as I have shown,† as well as Schlossberger,‡ is a complex body; it consists of different protein compounds, of which the body that I have now studied constitutes the chief element; it is characterised by a somewhat smaller amount of sulphur, and is distinguished besides from the mixture hitherto called casein, by the circumstance that it contains more oxygen. It shows the reaction of sulphamide-protein.

Without S N² H⁴.

C 53·5
H 7·0
N 15·0
O 23·9
S 0·6

53·4
6·9
14·5
23·9

54·1
7·0
14·7
24·2

100·0

98·7

100·0

Albumen of eggs, coagulated by heat, was mixed with diluted muriatic acid, and, after the addition of a small piece of rennet, set aside at a temperature of 104° F. After some days the albumen was completely dissolved. To the filtered liquid carbonate of ammonia was added, and the precipitate washed with water, alcohol, and ether, and dried at 262° F.

0,745 produced 0,004 ash.

I. 0,645 without ash, 87 C C of moist N, at 17°, 5 C, and 766 m.m.

II. 0,640 free of ash, 85 CC of moist N, at 17°, 5 C, and 756 m.m.

I. 0,573 free of ash, 1,107 C O², and 0,358 H² O.

II. 0,513 ditto, ditto, 0,998 C O² and 0,317 H² O.

I. 1,103 produced 0,1465 of sulphate of barytes, by means of caustic soda and nitre.

0,938 produced 0,118 of sulphate of barytes, by means of caustic soda and nitre.

Undissolved albumin.§

C . . 53·5
H . . 7·0
N . . 15·5
O . . 22·0
S . . 1·6
Ph . . 0·4

Dissolved and thrown down.

I.

52·74
6·93
15·97
22·53
1·83

II.

53·11
6·93
15·60
22·63
1·73

Mean.

53·0
6·9
15·8
22·5
1·8

* Scheik. Onderz. Deel IV. p. 278.

† Ibid. Deel III. p. 453.

‡ Annalen der Chemie und Pharmacie, April 1846, p. 92.

§ Scheik Onderz. Deel IV. p. 223.

The phosphorus, probably not contained in the precipitated albumin, was not determined. The quantity of the sulphur exceeds that in the albumin by $0,2\frac{0}{0}$; however, I do not doubt that this increase must only be ascribed to an error of experiment. It showed at any rate the reaction of sulphamide.

Without $S N^2 H^2$.		
C . .	53·0	54·9
H . .	6·9	7·0
N . .	15·8	14·7
O . .	22·5	23·4
S . .	1·8	
	<hr/> 100·0	<hr/> 100·0

Thus, albumen has not been changed in composition as regards the C, N, H, O, and $S N^2 H^4$; and it appears that no new combination of albumen is produced during the digestion in the stomach. Is this the case also with casein? No other conclusion can be drawn from the above analysis, than that either casein contains already a substance richer in oxygen, or that the production of such a combination is caused during the solution in the stomach. Let us now compare the organic group which remains, after deducting the elements of sulphamide from fibrin, hair, and this dissolved casein:

	Fibrin.	Hair.	Dissolved casein.
C . .	54·4	53·6	54·1
H . .	7·0	7·1	7·0
N . .	14·4	14·6	14·7
O . .	24·2	24·7	24·2

And let us place next to it the group, occurring originally in casein and albumin, (Scheik. Onderz. Deel. IV. p. 278 en 225):

	Casein.	Albumin from eggs.	Albumin from blood.
C . .	54·8	55·6	55·0
H . .	7·1	7·1	7·2
N . .	15·1	14·4	14·5
O . .	23·0	22·9	23·3

It will now easily be seen that there exists a considerable difference. In the three first groups is contained evidently less C and more O.

They are expressed by—

	Atoms.	Calculated.
C . .	36	53·6
H . .	54	6·7
N . .	8	13·9
O . .	13	25·8

The first change of the albumen in the stomach is therefore only solution, that of casein may also be oxydation. I say *may be*, for it is possible that this group already pre-exists in casein. This can only

be determined with certainty when the other constituents of casein shall have been further examined.

At all events, in casein, after its solution in the stomach, the same organic group exists as in fibrin, and there is so far an intimate relation between fibrin and casein. Casein must easily produce fibrin, while albumin remains still albumin in the stomach, and probably undergoes no other change than in the proportion of sulphamide it contains.

The question now is, how far the use of milk will be advisable in inflammatory diseases. I address this question to medical experience; but I am far from establishing any opinion upon the numerical results of experiments. Medical experience has its own rights, as well as chemistry; the one must not dictate to the other, but to propose such questions is both the right and the duty of chemistry.

Part Second.

REVIEWS.

Curabilité de la Phthisie et des Scrofules, appuyée sur des Preuves Authentiques. Par A. M. BUREAUD RIOFREY, M.D. &c. Paris, 8vo. 1847. Pp. 216.—(*Curability of Phthisis and of Scrofula, founded on Authentic Proofs.* By A. M. BUREAUD RIOFREY, M.D. Paris, 8vo. 1847. Pp. 216.)

On Pulmonary Consumption and on Bronchial and Laryngeal Disease: with Remarks on the Places of Residence chiefly resorted to by the Consumptive Invalid. By Sir CHARLES SCUDAMORE, M.D., F.R.S., &c. London, 8vo. 1847. Pp. 259.

IN no circumstance, perhaps, is the influence which pathology has exercised on the modern practice of physic better exhibited than in the growing conviction among the profession of the curability of phthisis pulmonalis. A few years ago a respectable practitioner scarcely ventured to ask himself whether consumption was a disease capable of undergoing a successful termination. Authority and experience united to impress upon him its necessary fatality, and general opinion was ready to decry his attempts as savouring more of charlatanism than of a desire to do good. And yet even when these views were most widely disseminated and believed in, morbid anatomy was continually demonstrating that facts were opposed to the dogmas of the schools—that tubercular ulcers of the lungs occasionally healed and cicatrized like other ulcers—and that

nature unassisted was capable of doing that which man presumptuously maintained was impossible.

The writings of Laennec, Andral, Cruveilhier, Stokes, Williams, and others, prove that many cases of pulmonary phthisis have, contrary to all expectation, recovered; and that, at a subsequent period, death having occurred from some other malady, the lungs have been found puckered and cicatrized from the healing of the tubercular cavern. The more recent researches of Rogée and Boudet in Paris, and J. Hughes Bennett in Edinburgh, have shown, from the indiscriminate examination of the dead in large hospitals, that puckerings, cicatrices, cretaceous concretions, and other evidences of former tubercle in the lungs, occur in at least one-third of all the individuals who die after the age of forty in this climate. Facts therefore indicate, that so far is pulmonary tubercle from being necessarily fatal, that it is spontaneously cured by nature in a vast number of cases, and that in not a few this is accomplished, even when large ulcers have been formed in the lungs, and all those symptoms present which are considered evidences of so-called consumption.

The curability of (understanding by that term recovery from) phthisis pulmonalis, is a matter therefore which no longer admits of dispute. It is a fact as certain as the curability of pneumonia, or the union of a fracture, and, like the latter, is susceptible of demonstration by means of well-preserved preparations. If, then, nature offers us such convincing testimony of her cures, why may not the physician, by following in her footsteps, and observing her method of treatment, bring about a like successful result? The circumstances necessary for the accomplishment of a cure, although difficult to be ascertained, are not beyond the reach of observation, and we anticipate that their discovery is yet destined to reward him who diligently searches for them in a right direction. In the mean time, we conceive that the present state of medical science enables us to answer the question "Is phthisis pulmonalis curable?" in the affirmative. But the question "Do we know the means of cure?" does not admit of such positive solution.

It is something, however, to know with certainty, that pulmonary consumption is not that necessarily fatal disease that it has so long been considered to be. It is a great matter by means of pathological facts to arrest that apathy which had seized upon medical practitioners; to snatch from the charlatan one of his most powerful strongholds, and legitimize a renewed effort towards the curative treatment of this dreaded disease. It is with no small interest, therefore, that we peruse every paper or work directed to this end; and we have gathered from the two publications placed at the head of this article several facts and observations, the consideration of which we think will not be altogether useless.

Neither Dr Riofrey nor Sir Charles Scudamore seem to possess any profound notions as to the true morbid anatomy or pathology of pulmonary tubercle. Dr Riofrey is content with citing the cases of

those to whom we have formerly alluded in order to prove the fact of its curability. The how or why he does not seek to explain. Sir Charles Scudamore has quoted the opinions of numerous writers, and of a few microscopic observers, with a view of showing that pulmonary tubercle must soften and be evacuated by the bronchi before it heals. He seems, however, to be wholly unacquainted with the researches of those who show that tubercle often degenerates into cretaceous and calcareous concretions, by far the most common mode of cure. His notions with respect to the minute structure of tubercle, and its mode of production, are also somewhat confused, as is almost always the case when elderly practitioners endeavour to reconcile their previous notions with the results which the microscope has enabled us to arrive at.

With respect to any benefit to be derived from this instrument, indeed, Sir Charles seems very doubtful. Thus, we are told, p. 25, that "the use of the microscope would be too inconvenient in daily practice;" but no reasons are given for this assertion. Again, he asks, p. 57, "Has the tubercular cell been found by aid of the microscope in the blood in the general circulation?—I believe not. Is this delicate instrument to be perfectly relied upon to decide this point?" No answer is given to this latter question; but we opine, that if the microscope cannot do it, no other instrument can, and, until it be done, we protest against the inference that tubercle corpuscles float in the blood.

Notwithstanding the doubts expressed and implied concerning the microscope, Sir Charles Scudamore has thought proper to examine tubercle with that instrument with the assistance of a friend most experienced and skilful in its use, and the following is the result of their joint inquiries:—

"Tubercle, when examined microscopically, is found to consist of an assemblage of corpuscles of variable size and shape, sometimes containing granular matter of exceedingly minute granules; and in some of the smaller kinds of tubercles, as in the grey miliary, besides the constituents above mentioned, there are cells of a more regular form and size, and larger than the corpuscles. In the crude or firm tubercles, the corpuscles are closely packed together, and the granular matter scanty; whereas, on the contrary, in the larger and softer kinds, the corpuscles are easily separable, and the granular matter in great abundance. The form of the corpuscles is for the most part globular or oval; but in the softened tubercles they are very irregular as to their shape, being often elongated and fusiform. They vary in diameter from about 1-3000th to 1-2000th of an inch. The granules, also, are very variable as to their size. Mixed with them are found myriads of minute globular bodies, scarcely capable of being measured by our ordinary micrometers, being much less in diameter than 1-25000th of an inch. These are most abundant in the soft tubercles, which, in fact, appear to consist of little or nothing else but granules and broken-down corpuscles. The cells are more constant in size and shape than any other constituents, and average in diameter about 1-1500th of an inch. Nuclei are sometimes apparent; but as the tubercle increases in size, the cells become disintegrated, and finally disappear."—Pp. 66-7.

This general description is tolerably correct. Tubercle cells, how-

ever, never become elongated and fusiform, and the author has confounded these elements, which are often associated with tubercle, with the tubercular corpuscles themselves. We very much doubt, also, the occasional existence of nuclei.

With respect to the chemistry of tubercle, we are told:—

“I have made very numerous chemical examinations of tubercle, and have found that all the varieties present one general result, of showing them to be composed always of albumen, occasionally with slight evidence of fibrine, always of lime in abundant proportion, and in varying degrees of combination with carbonic and phosphoric acids, more rarely with muriatic; and in some specimens I have detected the slight presence of magnesia.

“The more hard the tubercle, the larger proportion of phosphate of lime does it contain; and when of less firmness, the proportion is greater of the carbonate, and the albumen is not so dense. I have not discovered any gelatine in tubercle. In proportion as it may possess transparency, the albumen which it contains is thinner, and of the least specific gravity; and, on the contrary, when opaque, it is more dense, possessing more of the albuminous principle and less water. Hence, then, the external characters of tubercle depend chiefly on its chemical composition, and on the particular tissue in which it is formed.

“I examined a pearly-looking tubercle, commonly called the crude-yellow one, found under the peritoneal covering of the liver, about the size of half an almond, and of moderate firmness. It consisted of albumen, carbonate of lime, and a small proportion of phosphate.”—P. 63.

Sir Charles Scudamore, therefore, confirms the general results arrived at by several chemists, and leaves the question as to differences in composition between tubercle, lymph, and cancer (the only point worth determining in the present state of science), in the same condition as before.

The following passages contain Sir Charles Scudamore's views regarding the pathology of tubercular phthisis:—

“I adopt the opinion that tubercular phthisis is a specific blood disease, that there exists a tuberculous condition of the blood, and which, in the strongly marked examples of hereditary phthisis, is born with the individual as a germ. We cannot demonstrate this by chemical analysis; because the science of chemistry does not in any case of morbid impregnation, whatever be the poison, enable us to make the detection. In some instances, the taint may be latent for many years. In others, as in the examples of tubercles being found in the foetus in utero, or in the youngest infants, the effects are immediate. Of the existence of tubercles in the foetus, we find two examples related by Mr Langstaff, one by Hussen, two by Ohler, and one by Chaussier. I examined the body of an infant which died in a state of extreme emaciation at the age of four months, the mother having been in the last state of tubercular phthisis when she gave birth to it: I never witnessed so remarkable and extensive a display of tubercles, both miliary and of a larger size, the former semi-transparent, the latter grey in colour. The lungs on each side, both upper and lower lobes, the liver, the spleen, the mesentery, and peritoneum, were universally studded with tubercles. I cannot relate a more striking instance than this of the tubercular disease in its highest activity, or more favourable to my hypothesis, that a specific state of the blood, and which I call the tubercular, is the proximate cause of the formation of tubercles—the proximate cause of pulmonary consumption.

“The term is very commonly used, of deposit of tubercles in the lungs, or elsewhere. To this I object; for it implies that the tuberculous matter is pre-

capitated from the circulating blood ; whereas, I conceive that a tubercle is formed by certain secreting vessels supplying the identical part where the tubercle is found ; that, instead of secreting healthy mucus, or other tissue, as it may be, they form tubercles.

“ In every stage of tubercles, the surrounding pulmonary tissue may wear a healthy appearance. We are sure that the cancerous virus exists in the blood. How little may serve to taint the mass ! How very minute a portion of variolous virus is capable of producing, in certain constitutions, a confluent eruption on the skin.

“ The tubercular virus may exist in different degrees of intensity in different persons ; in one individual, leading to slight indications of consumption, what is called a tendency to it ; in a second subject, to the disease well marked, in the chronic form ; in a third, the acute disease, running a most rapid course.”
—Pp. 55-57.

We agree with the author in the opinion, that the proximate cause of tubercle is some change in the composition of the blood, but we cannot conceive this change to be any way analogous to that produced by a virus. It is true, we are told that this word is only used for the sake of convenience, and experiments are given which show that tubercular matter when inoculated produces no effect on animals. Yet a reference to the variolous virus proves to us that Sir Charles has not himself escaped the influence which the employment of a bad term is so peculiarly apt to exert on our views. In point of fact, there is no analogy whatever between the conditions of the blood in tubercular phthisis and variola. The change in the first case is brought on by diminished nutrition, whereby the exuded materials lose their power of forming perfect cells. The change in the latter is caused by the introduction of a poison, which exerts no influence whatever on cell formation. These facts alone, independent of many other points of difference which exist, exhibit a broad distinction between the tubercular condition of the blood, and what is present in the variolous and other states of it produced by a virus. This term, therefore, we think should not be employed, as it will only lead to confusion.

Neither does the objection brought forward to the term deposit, as applied to tubercle, appear to us well founded. We presume it cannot be denied, that this formation transudes through the blood-vessels in a fluid state, and that the more solid portions, whether organized or not, are subsequently formed or separated. We must confess, therefore, that we see no reason for not applying the term tubercular deposit, especially as it is one that has long existed in pathology, and is clearly understood by all who use it.

There are two points in the pathology of tubercle which have lately excited considerable interest, viz., its transformation into cretaceous and calcareous concretions, and the influence which carbonaceous deposits seem to exert upon its progress. On neither of these points, we regret to say, does Sir Charles Scudamore appear to be very extensively informed, although he touches upon both. This will appear from the following passages :—

“ I believe that the secretion of carbonate of lime, alone, or with the addition of charcoal, may modify the tuberculous formation ; but I am not sanguine in

expecting that the latter will commonly be thus arrested; yet, as an apparent example of the conversion of the formation of tubercle into the secretion of the lime compound, I will offer the account of a post-mortem examination with which I am just made acquainted. 'A lady, aged thirty-five, died very suddenly, in half an hour after the seizure, which was that of faintness and universal coldness, being previously in good health and spirits, and having only recently breakfasted, the heart was found to be twice its natural size, quite empty, while the lungs were full of blood. The lungs on one side were adhering to the ribs. There were signs of tubercular disease, but the tubercles had healed, and were changed into chalky concretions.'"

"I have met with many examples of calculous concretion, varying in size from a hemp-seed to that of a kidney-bean, in tuberculated lungs, and chiefly when the tubercles have been of the grey granular kind, opaque, and of the appearance of very fine threads, matted together. I have occasionally found the calculus firmly encysted, without having produced any signs of irritation in the surrounding tissue. Sometimes the phosphate of lime has predominated in the calculus, at others the carbonate.

"I attend a lady who has during the last seven years expectorated from time to time, and never been free for more than six months, small pieces of calculus, which I found to consist wholly of carbonate of lime and animal matter. A few days before she gets rid of them, she has a troublesome cough, with quicker respiration than usual, and pricking sensations at the pit of the stomach.

"I knew another lady who has occasionally coughed up calculi during the last thirty years. I was satisfied that in neither of these instances did the calculi come from the tonsils. It would appear, therefore, that calculi are occasionally formed in the lungs without the accompaniment of tubercles, and without producing serious irritation in the lungs. In one case under my care, this calculous formation, after prevailing for many years without creating any alarming symptom, was superseded by the formation of tubercles, producing fatal phthisis."—Pp. 61-64.

After the researches to which we have formerly alluded, made in the Parisian and Edinburgh hospitals, it must be considered very inexcusable in any writer upon the subject of pulmonary phthisis, to speak in the above manner concerning the calcareous concretions found in the lungs. These researches demonstrate not only that the calculi alluded to are frequent, and that they are evidences of degenerated tubercle, but have traced their formation in all its stages from crude tubercle into cretaceous, and even calcareous formations of stony hardness. No pathologist can doubt that these latter are owing, in the majority of instances, to the drying up of tubercular deposits in the lungs, and that they can be formed by themselves is an idea that does not merit any lengthened refutation.

With regard to the black deposits so frequently observed in the lungs, we are told—

"From a review of the whole of this question, I am led to think that the black pigment—the melanotic matter—is due to two sources: the inhalation of carbon from without; the deposit from venous blood in the lungs, or elsewhere, surcharged with carbon which does not meet with sufficient oxygen for its conversion into carbonic acid. As the same deposit is found in different tissues in various parts of the body, may the same theory serve to explain it—want of healthy oxidation of the circulating blood?"—Pp. 61-2.

Here Sir Charles seems to have confounded melanotic matter in

general, with the black deposits found in the lungs, and often associated with chronic tubercle. Carbonaceous deposits are at once distinguished by being unaltered on the addition of nitric acid, whereas blackened blood at once loses its colour on the application of that re-agent. Now, it is the former alone which are associated with tubercle and other chronic alterations in the lungs; and we have seen some remarkable examples of the complete exclusion of tubercle by the molecular carbon. The mode of its formation, however, and its exact influence on exudations into the pulmonary tissue, is not yet fully ascertained.

Passing now from pathological speculations to principles of treatment, nothing can be more different than the views of the two authors before us. The key to Dr Riofrey's practice is furnished us in the 74th page of his Monograph, where he says, "the true antagonism of phthisis is in the fatty condition." He claims this as a discovery altogether new. We certainly cannot yield him any merit as to originality, inasmuch as the practice of giving oil in phthisis and scrofula has long been established among the Germans, and its action in these diseases as fatty matter is fully exposed in Dr Hughes Bennett's work on the *Ol. Secoris Aselli*. We do believe, however, with the author, that the employment of fatty substances, and all those means which tend to the production of fat in the body, constitute the best treatment for phthisis. He goes on to observe—

"In theory it is easy to understand that in a lung, where there is a lesion of the parenchyma and softening of the tubercle, it is necessary that reparative elements should arrive in one way or another. The fatty element contains these reparative principles in an eminent degree. Fibrine is found already formed in animal bodies. If vegetables are less nourishing than the elements derived from the animal kingdom, it is that the animal kingdom furnishes fibrine ready formed. Now, in the softening of tubercles or the expectoration of blood or pus from the lungs, the patient loses fibrine,—he loses the nutritive reparative substance; and in such circumstances there are men rendered sufficiently insensate by a system, to bleed their patients without ceasing, in order to deprive them of nourishment. There are others sufficiently blind to expect miracles from the use of kitchen salt, chlorine, ammonia, or tar. Are you astonished when you see sensible men, regular physicians, under the empire of a systematic hallucination? Are you astonished that charlatans succeed? Do you know who are those who do succeed? They are those who have to treat patients debilitated by bleeding, leeches, and low diet.

"I have said that the fatty element plays many parts. In fact, it furnishes indispensable materials to respiratory combustion. Is it not fat which furnishes materials for combustion in hybernating animals? Is it not fat which is placed by the modern savants at the head of agents of respiration, in other words, aliments of respiration? If there is not in the human body, either in a state of sequestration and reserve, or by nutrition, agents of respiration sufficient to serve this function, respiration attacks the sources of life in taking from the tissues themselves the elements indispensable for its exercise."—*Pp.* 76—78.

"All the sciences are linked together, and the beautiful researches of Liebig, Dumas, Payen, and Boussingault will do more for the treatment of phthisis than the descriptions of many pathologists."—"Agriculturists fatten their phthisical cows. When convinced that their cows are in this state, they force

forwards the development of the fatty element. Emaciation soon ceases, and the life of these animals would be very long, although they had been phthisical, if the butcher did not put an end to it.

"In the work of M. Louis we find some words on the incompatibility which exists between the fatty state of the liver and tubercles. The author recalls a fact stated by M. Reynaud in his memoir on the phthisis of monkeys. The fatty liver of monkeys, he says, has never exhibited tubercles, and when the liver was tuberculous, there was never fatty transformation. Do you think that M. Louis has drawn any induction from this observation? Not the least in the world: he cites the fact, and that is all."—"For me, the observation of M. Louis is precious. It is a pearl in a pathological heap; for it results evidently from this fact, that tubercle and the fatty element are in a state of true antagonism."—Pp. 80, 81.

It is with the view of carrying out this fattening treatment that Dr Riofrey recommends the employment of cod liver oil, for the use of which he also gives us the authority of the following eminent names,—“Seutin, surgeon in chief of the Hospital St Pierre, at Brussels; Clugg, chief director of the anatomical department; Nuckel, Fisher, Heymann, physicians in the civil hospital of Cologne; Albers of Bonn, professor of pathological anatomy; Nasse, professor of medicine; Chelius, professor of surgery at Heidelberg; Fabricius, surgeon of the Frankfurt hospital; Forget, at Strasburgh; Groshan, clinical professor at Rotterdam; Young, at the Hague; Desertine, Alsbersbery, at Leyden; Professors Zuringar and Tillanus, the one physician, the other surgeon in chief of the hospitals at Amsterdam; Meesens, professor at Ghent: such are the authorities that I name and present to you, as having extolled without reserve cod liver oil in all scrofulous affections.”—Pp. 90.

To these names the author adds Dr Peyreya of Bordeaux, and he might have appended many others to the list. Considering that he is endeavouring to persuade his countrymen of the benefits of this substance, the citation of such names may not be without effect upon them, and upon practitioners in this country also. It is not this or any other medicine, however, that Dr Riofrey considers alone worthy of confidence. As before stated, all those means which tend to make the individual fat, and to support respiration and nutrition, are according to him beneficial in phthisis.

Sir Charles Scudamore, following the views of Lugol with respect to the value of iodine in tubercular diseases, is of opinion that the best method of introducing that medicine into the system, is by the lungs in the form of inhalation. We give his directions entire:—

“As, by mixing the tincture of iodine with water, the iodine itself separates into flakes which become precipitated, and as 7000 parts of water are required for its solution, I found it expedient to form a preparation which should be uniform, and preserve its transparency when united with water in any proportions. This admixture is effected by adding together iodine, iodide of potash, distilled water, and alcohol.

“The following is my formula:—R. Iodinii puri; Potassii iodid āā gr. vi.; Aquæ destillat. ꝑv. ꝑvi.; Alcoholis ꝑii. M. fiat mistura in inhalationem adhibenda.

"But invariably I direct the addition of a *saturated* tincture of the dried leaves of conium, which in the most favourable manner softens the action of the iodine solution, and tends to soothe the bronchial mucous membrane. Of the iodine solution I commence with the dose of 30 minims, and increase it by five or ten at a time, in a gradual manner, according to its effects and the nature of the case, till I may perhaps carry it to 240 minims; but, in the majority of instances, I confine my range to 180 minims. Whatever may be the quantity I use for each inhalation, I constantly direct that two-thirds of it be put at first, the other third when half of the time for inhaling has expired; otherwise it would be too strong at first and too weak at last; 30 minims of the tincture is the ordinary dose which I prescribe, and this need not be divided, nor does it in general require increase, as it is so much less volatile than the iodine, and enough of strength remains; but, if much of its soothing influence be wanted, either to allay irritable cough or to act as a soporific, a drachm, or even a drachm and a half, may be employed; but in such case, it is better to use it in divided portions. The water to which these preparations are to be added, should be of a temperature of 115° to 125° Fahrenheit;—as a medium, 120°; a little more or less is not material. The whole should be well blended by shaking the inhaler. This should be constructed of glass, for a metallic one instantly decomposes the iodine. Its tubes should be capacious, and the inhaler should never be quite half-filled; for if these two last circumstances are not carefully attended to, much inconvenience would arise, the inhaling would be rendered difficult, and which by proper attention is so perfectly easy a process, that an invalid with the weakest respiratory powers does not experience any difficulty.

"The last part of these preparatory steps, for the purpose of keeping up the proper temperature of the contents of the inhaler, is to place it in an open vessel, large enough to allow of the inhaler being a little removed from its sides. Water, of a temperature from 120° to 130°, is to be put into it, enough to rise to about two-thirds of the inhaler; and, to prevent any inconvenience from the vapour which issues, the vessel should be covered over with a piece of thick pasteboard, neatly fitted.

"Now desire the patient to inhale by making a rather deep inspiration, then to relax, or take off, the lips from the mouth-piece; and to inhale again immediately, carrying on the process effectively, so that the medicated vapour shall pass into the deep air-passages, but not in a quick and fatiguing manner. At the first time of using it, five or six minutes will be sufficient; but in the progress of the treatment it may be extended to twenty, twenty-five, or thirty; but I seldom in my direction exceed twenty. The frequency of repetition is from twice to thrice in the day; commonly thrice, for the first four or six weeks."—Pp. 109, 110.

In addition to inhalation, however, Sir Charles considers as essential a good diet, a pure air, equable temperature, and all those means recommended as necessary by Dr Riofrey for supporting nutrition; and we cannot help suspecting that these are the true causes of the success which Sir Charles has met with, and that the iodine inhalations have had little if any thing to do with it.

Dr Riofrey gives fifteen cases, either copied from works of the most esteemed practitioners, or communicated to him by professors or teachers of established celebrity. In all these, there was confirmed phthisis, which afterwards underwent a cure. He might have swelled the list by citing many others, of the existence of which he appears to be unacquainted. Those quoted, however, are sufficient to prove the point he is desirous of establishing, namely, that pulmo-

nary consumption is curable. We cannot discover, however, from their perusal, any particular treatment to which the cure can be ascribed. The author himself notices that recovery seems to have followed—first, change of locality; second, an artificial atmosphere; third, sedative and calmative treatment; fourth, an equal temperature; fifth, mercurials; sixth, anti-scorfulous treatment; seventh, a treatment directed against certain diseases of the skin (traitment antidartreux); eighth, antagonistic diseases; ninth, puncture of the breast; and tenth, good alimentation, and by causing individuals to become fat. It is in this latter point of view, that, according to the author, cod liver oil is so useful, concerning the advantages of which he promises us before long another Memoir. We doubt very much whether any of these, with the exception of equable temperature and good alimentation, have had any influence in arresting the disease. Neither do we believe that a good alimentation consists simply in giving cod liver oil. Different cases not only require a different aliment, but the stomach of one will digest food which another will not retain. What we require in the treatment of phthisis is a knowledge of how to adapt aliment to the different cases and varieties of the disease. The examples of cure on record are only illustrations of the mode in which nature occasionally acts on this principle. At all events, we think we can trace it in every case which has recovered, and in some perhaps better than in those recorded by Sir Charles Scudamore.

He relates thirty-eight cases, the majority of which, however, are chronic cough, bronchitis, asthma, &c., relieved by inhalation. In many cases only *suspicion* of phthisis existed. In a few, the disease was undoubtedly present, and of these Case II. offers an interesting example. It presented all the functional symptoms and physical signs of the disease, including among the latter pectoriloquy and gargouillement on the right side. The tongue was coated; she had great thirst, but loathed food. The digestion was weak and irregular. She was so reduced in strength, that she required to be carried from the bed to the sofa in an adjoining room. Digitalis had been given with disadvantage. The treatment consisted in employing the iodine inhalations, a sarsaparilla mixture, a soothing syrup, and regulation of the bowels. A tannin lotion was also applied to the chest, and in addition, "*the diet to be very supporting; in the morning early, some ass's milk and biscuit; for breakfast, black tea mixed with an equal part of hot milk, and some sugar, and a fresh egg lightly boiled; in the middle of the day, oysters, or some cold chicken or other light animal food, with sound draught porter; at dinner, meat, a little boiled vegetable, with porter; after which, she usually took some light nutritious pudding; in the evening, she had two small cups of tea, as in the morning, with milk, with biscuit and butter; at night, arrow-root, or other similar nourishment, with a small quantity of sherry.*" In the enjoyment of all these good

things, the state of the patient, as might be expected, now improved; but—

“On one occasion, from the concurring influence of an unfavourable exposure to a warm sun and a cold wind, and an accidental error in diet, the stomach and bowels became so violently disordered, with attendant derangement of the biliary secretion, that the strength sunk suddenly, and I was much alarmed for her safety. Yet this illness was soon overcome, and the constitution again rallied. The principles of treatment already described were diligently pursued, and the inhalation was never neglected; although, in the latter weeks, the frequency of its employment was limited to twice in the day: on two occasions, I changed the iodine for chlorine, for about ten days each time, in consequence of dark slight ulcerations of the tonsils, which I thought might be owing to the iodine; but I was convinced, by the patient's statement of its sensible effects, and by my own observation, that the iodine was the only efficacious remedy. I had latterly increased the quantity of the iodine mixture for each inhalation to half an ounce; not using more than before of the conium. I lessened it again.”—Pp. 119, 120.

Ultimately the patient was restored to a comfortable state of health. Sir Charles, of course, attributes the great advantage to the iodine inhalation; but to us the good diet was the real restorer. Even the author himself, though preoccupied with his peculiar treatment, could not overlook the effects of good alimentation, as the following remarks will prove:—

“In no case which has ever come under my care, have I been more satisfied than in the present, with the propriety of directing a full and highly supporting diet; always, of course, being careful to avoid producing any sensible oppression of the stomach and digestive powers, and to watch the effects of such diet in every way, as much as I would those of a powerful medicine. The good condition, for the most part, of the digestive organs, was undoubtedly one of the most encouraging circumstances in this case; as, by the converse, loss of appetite, weak digestive power, and constant tendency to diarrhoea, must weaken, if not destroy, our hopes of success. As the name of the disease, Consumption, so forcibly expresses, the waste that is going on from absorption and irritation requires a countervailing proportion of nourishment; and, as a general rule, I should wish to give the consumptive invalid as much supporting food as could be comfortably digested.”

In another case (Case III.), there was not much failure of appetite, but the digestion was weak and irregular, the bowels were unusually torpid and deficient in bile, and the urine gave a copious deposit of lateritious sediment. Under the right clavicle there was dullness, with pectoriloquism. The same treatment with good diet was employed, and recovery also took place. We are told that “he derived a sense of comfort from the inhalation. The cough was usually troublesome during the process; but the increased facility of expectorating, and the subsequent quiet of the chest for an hour or more, made full amends for the temporary irritation.” We find, however, that in Case IV. also, the cough was sometimes rendered more troublesome, and we are bound to state, that in several cases in which we have seen inhalation tried, this has been the usual result.

It is unnecessary for us to follow the cases of Sir Charles Scudamore further. It is enough for us to say that we do not attach great importance to inhalation in pulmonary phthisis, and are of opinion that the advantages described are for the most part if not entirely owing to hygienic and dietetic regulations. Such are considered to be the active agents in benefiting the patients he has treated, and such also are the reasons which caused the recovery of those described by Dr Riofrey.

Traité des Maladies des Pays Chauds, et spécialement de l'Algérie (Première Partie : de la Dyssenterie et des Maladies du Foie qui la compliquent). Par le Docteur Ch. CAMBAY, Médecin Ordinaire et Médecin en Chef, de l'Hôpital Civil et Militaire de Tlemcen, etc. 8vo, pp. 600. Paris, 1847.

M. CAMBAY'S work is but a portion of a larger work which he has in contemplation. The part of it published is confined to dysentery and the diseases of the liver which complicate that affection. Among the forms of dysentery of which he treats, are simple dysentery in its two degrees of mild and severe, hepatic dysentery, scorbutic dysentery, and chronic dysentery. Referring to the pathology of the disease, he makes six anatomical varieties. Among these he describes one variety as marked by what he terms incrustations of the lining membrane of the great intestine. M. Cambay thinks he is the first to remark these so-called incrustations, of which he gives the following account: "The greater part of the lining membrane of the large intestine is found covered with an incrustation of grey cellular points, resembling tubercles, larger than grains of sand, and having an earthy aspect." He regards these points as composed of hypertrophic spots of the cellular coat, covered by thickened mucous membrane; and hence the term incrustation is hardly applicable to them. One other variety we will notice, as the fact referred to under this head has been often doubted—namely, the detachment of the mucous membrane from the surface of the intestine in plates, or in an entire cylinder. Our readers are aware that it is long since it was believed that the inner membrane of the intestine is sometimes passed in plates or in an entire cylinder, and that it is only of late years that doubts have been thrown on it, on the ground that portions of lymph, in the form of false membrane, have been mistaken for the mucous membrane. M. Cambay's observations appear to prove that such a separation does in fact take place.

M. Cambay's plan of treatment includes blood-letting, both general and local, and he prefers applying leeches to the abdomen to applying them to the anus—opiates also; ipecacuan as an emetic; and pills composed of ipecacuan, opium, and calomel. In M. Cambay's experience the blood in dysentery is not often buffed;

it is the complication of hepatitis and peritonitis that produces that appearance of the blood. Out of a hundred cases of dysentery in which blood was drawn, no more than eight showed buff.

In this book M. Cambay has drawn on the many English works treating of the diseases of the East and West Indies, and, if he continues his plan, he will be able to make still further use of them. As is remarked in the notice of his work in the *Gazette Médicale de Paris*, "M. Cambay's knowledge of the English language has enabled him to discover valuable works on the diseases of India, the existence of which is hardly suspected in France."

The Dublin Dissector, or System of Practical Anatomy. By ROBERT HARRISON, M.D., M.R.S.A., &c. Fifth edition, with numerous illustrations, 2 vols. Small 8vo. Dublin, 1847.

THIS work has been long established as one of the best guides for the anatomical student in the dissecting room. The present edition is greatly enlarged. It forms two handsome but portable volumes, and is illustrated by 160 illustrations on wood. The text has been carefully revised throughout, so as to present the dissector with a condensed view of the present state of anatomy. A copious index at the end of each volume adds considerably to the value of the work. The cheapness and excellence of this edition cannot but command for it a large sale.

Part Third.

PERISCOPE.

ANATOMY AND PHYSIOLOGY.

VULVO-VAGINAL GLAND. By M. HUGUIER.

M. HUGUIER has given a description of a gland situated at each side of the junction of the vulva to the vagina. It was discovered by Gaspard Bartholin, and was generally described by older anatomists; but of late its existence has been almost forgotten. According to M. Huguier, this gland is about the size and form of an apricot kernel, and is provided with an excretory duct, about seven or eight lines in length, the external aperture of which is situated in the angle between the vulva and the border of the hymen. This gland is small until puberty, when it is developed with the other organs of generation; it becomes turgid during sexual excitement, and secretes a quantity of clear mucus-looking fluid, which it is said to ejaculate with some force. M. Huguier agrees with former anatomists in regarding this gland as closely analogous to Cowper's gland in the male subject; for it is situated in about the same part of the perineum as this latter is, and presents the same anatomical relations and con-

nexions. It is an appendage to the vulvo-vaginal cavity, a part which is analogous to the urethra in the male; it receives the materials for its nutrition and its sensation from the same vascular and nervous sources as does Cowper's gland; it presents also many varieties in form, size, and situation, and it may be absent on one or both sides, as is often the case with Cowper's gland.—*Archives d'Anatomie*.

INFLUENCE OF THE PNEUMOGASTRIC NERVES ON DIGESTION. By BOUCHARDAT and SANDRAS.

THE results of some experiments performed by MM. Bouchardat and Sandras, to determine the influence possessed by the pneumogastric nerve over digestion, show clearly that division of both these nerves in the neck, at once arrests the process of digestion so far as the stomach is concerned, but has no influence over that part of the process which takes place in the intestines. After feeding dogs with a mixed diet, and then dividing both pneumogastric nerves, they found, after twenty-four hours, that those substances, the digestion of which is effected principally in the stomach, such as albumen and fibrin, were quite unchanged; whereas those substances which are digested in the intestines, such as the amylaceous and fatty principles, had been dissolved and absorbed just as though the pneumogastric nerves had been undivided. In several of these experiments they found that, although no chyme is prepared in the stomach after division of the nerves, yet the starchy principles which pass into the intestine are there converted into glucose, and that the fatty matters are absorbed by the lacteals, just as in the ordinary state of health; so that the digestion and disposal of these principles appear to be quite uninfluenced by the operation. They found also that it is not by compression of the trachea by the distended œsophagus, that rabbits die when fed after division of the pneumogastric nerves as high up as on a level with the larynx.—*Comptes Rendus*, Jan. 1847.—*Medical Gazette*, April 1847.

ON THE PHYSIOLOGY OF THE MUSCLES OF THE EYE, WITH SOME NEW VIEWS OF THEIR FUNCTIONS. By HENRY HOWARD, M.D., of Montreal.

ACCORDING to our author the four recti muscles of the one eye, without considering those of the other, may be described as completely under the influence of the will, that is to say, they do not act without the will, though they cannot at all times obey it—for example, when a foreign body gets into the eye, the cornea becomes turned up by the action of the inferior oblique, an involuntary muscle, in opposition to the strongest efforts of the will on the inferior rectus. The four recti muscles acting collectively, fix the eye in vision—and also prevent the eye from being protruded in the action of the oblique muscles. As to their separate actions, the superior rectus turns the eye upwards, the inferior downwards, the external outwards, and the internal inwards; and as respects one eye, all these separate actions are in obedience to the will. The internal rectus acts involuntarily or automatically whenever the external rectus of the other eye turns it outwards—that is, according to our author, whenever the will turns one eye outwards, the other is turned inwards automatically. Our author says, that he considers the internal rectus, in opposition to other physiologists, to be the only automatic muscle of the eye. Here, however, he distinguishes automatic from reflex action, for he presently after says that the two oblique muscles “are purely involuntary or reflective.” The superior recti, our author continues, the inferior recti, and the internal recti, have a voluntary consentaneous action, while the external recti never have a consentaneous action; and these recti muscles, not the oblique, are exclusively concerned in the intermediate movements of the eyeballs, when the eyes are open, as upwards and outwards, as upwards and inwards, &c. Lastly, as to the recti muscles, he affirms that they have not the power of retracting the

eye ascribed to them by some physiologists. The inferior and superior oblique muscles are, according to our author, as above noticed, purely reflective in their action. They have, he says, no voluntary power. The inferior oblique turns the corner upwards and inwards, and this takes place the moment any irritation affects the eyeballs, the orbicularis palpebrarum closing the upper eyelid at the same moment. That it is the inferior oblique that moves the cornea upwards and inwards, in the case of irritation, he says, is proved, first, because it is turned nearly out of view, a power which the superior rectus has not; again, because after the superior rectus has been divided in an animal, the cornea turns up as before, but if the inferior oblique be divided, while the rectus superior is left entire, no irritation can produce an elevation of the cornea. To continue, the superior oblique turns the cornea downwards, and slightly inwards, acting consentaneously with the levator palpebræ superioris. Thus, when in waking from sleep, the upper eyelid is raised, the cornea is seen to be turned down, the lower eyelid at the same time being depressed. Here our author claims for himself the discovery that the true depressor of the lower eyelid, so long sought for by physiologists, is the superior oblique by a negative effect—or as the prominence formed by the cornea descends, it presses against the inner surface of the lower eyelid, and carries it forwards and downwards. The rectus inferior, by a voluntary effort also, he says, may still farther depress the lower eyelid through the medium of the cornea. The levator palpebræ superioris, and the orbicularis palpebrarum, are muscles capable at once of voluntary and of reflex action. He denies the statement, that the orbicularis with the oblique muscles prevents the protrusion of the eyeball; affirming that the supposed protrusion and retraction of the eyeball is a deception arising from the greater length of the antero-posterior than of the transverse diameter, so that, when the eye is turned upwards and inwards, the long diameter withdraws from under the finger, and returns again when the eye is restored to its usual position.—*British Amer. Journ. of Med. Science*, July 1847.

ON THE PERISTALTIC MOVEMENTS OF THE INTESTINAL CANAL. BY DR F. WILD, Cassel.

No means of irritation, mechanical or chemical, applied only to the mucous membrane or the peritoneum, sufficed to produce peristaltic movements in the living dog. Each irritation of the muscle produced a movement confined to the irritated spot, or extended but a short way beyond. A pale, sharply-defined swelling arose from contraction of the longitudinal fibres; the circular ones were also each time, though less intensively, excited, as two such swellings near one another, were, by their contractions, brought for a short period in contact. This swelling cut into, dissected off, and examined microscopically, presented all the characters of muscular fibre. Were the entire periphery, or a broad portion of it excited, shortening and constriction of the irritated portion followed; the duration and strength of the contraction being proportioned to those of the irritant. Under all circumstances the longitudinal contraction lasted longer than the circular. The act of contraction weakened the irritability of the part, more especially of its longitudinal fibres, which could not be made to contract oftener than twice, while this could with the circular ones be frequently done. After death in dogs or in living rabbits, local irritation of the peritoneum or muscle, but not of the mucous membrane, produced general movements, strongest 10'—15' after death, particularly if the animal had been for some time used in experimenting, or if the gut were cooled. These were most easily excited in the duodenum, and could not be produced after a quarter of an hour, although after a period of rest, and the restoration of the lost heat, irritability occasionally returned. The movement occurred in this manner: The irritated portion drew itself together, and then relaxed; when this relaxation had reached a certain point, the movement began

anew. This occurred five or six times, each subsequent contraction becoming less intense; sometimes only a shortening of the gut took place, and when it had reached a certain pitch, a point one or two inches apart became fixed; the motion then alternated several times from one to the other. Occasionally a propagative motion was observed; this occurring most frequently in the large intestines, but also in the small, leaving however, as in the œsophagus, small intervening portions at rest, never extending far, and often returning over the same portion from the same point. Either they extend from the primarily irritated portion peristaltically and anti-peristaltically at once; or they occur after an irritation at a distant point. All those movements just mentioned were little intensive, and followed very slowly. Movements frequently arose spontaneously, and often not immediately but a few moments after the application of the irritant, so that it was sometimes difficult to say that they were a consequence of it. Irritation of the nerves produced sometimes movements of the parts to which they were distributed, sometimes not, and this though muscular irritability was still left. Before experiments can be satisfactorily and usefully instituted on the intestines, the power of the non-striated muscles—their relations to the nerves—those of the nerves to the central organs, and the origin of the excitant power in the latter must first be ascertained. As some contribution towards this, our author makes the following deductions: 1st, Direct irritation of the muscular substance produces movements without intervention of the nerves, because *a*, the contracted portion, corresponds exactly to that irritated; did nervous agency intervene, it ought to correspond to the sphere of the nerves irritated; consequently either each two contiguous muscular molecules have a nervous particle for themselves, or, as the nerves occasionally cause general movements, their primitive fibres must sometimes conduct the nervous current farther, at others confine it to separate divisions of the same fibre,—*b*, as each piece of the non-striated muscle, however small, or in whatever manner it be incised, still contracts itself durably, would we deduce the duration of the contraction from an intervening central organ, we must suppose one to exist for each piece, which must also be in connexion with each excited nervous fibre. Who will demonstrate those numerous intestinal central organs? 2. The attraction of two molecules is not inductive of attraction of the contiguous ones, even when they belong to the same fibre. 3. This being accepted, all extensive movements must proceed from extensive irritation, and, failing others, from nervous irritation. 4. Both excitory and motory nerves are only at times capable of exciting muscular motion, consequently only at times capable of acting as muscular irritants. 5. The central organs are in a similar predicament, and seem also to originate this excitant power only with intermissions; hence the nervous agency produces movement but never contraction—*Zeitschrift für Rationnelle Medizin*, v. Band. I. Heft, 1846.

SURGERY.

CONGENITAL OBSTACLE TO CATHETERISM. By Professor THEILE of Bern.

In a man, aged thirty-five, who had hanged himself, a probe was introduced into the urethra, to ascertain if ejaculation of the spermatic fluid had accompanied the death in this instance. But in vain—the probe could not be made to enter more than an inch; neither could a tube be made to enter, when it was desired the following day to inflate the bladder. Theile determined to investigate the nature of the obstacle. There was a fissure into the urethra 6'' in length, the greater portion of which lay on the lower surface of the glans, and reached to its posterior border, thus presenting the lowest degree of hypospadias, and a deficiency of the prepuce corresponded to the fissure. When the urethra was cut from the glans along its lower wall, nothing

unusual was observed either in texture or diameter; but about 5" behind the summit of the glans there was seen a septum about $\frac{1}{2}$ " thick, running 8 or 9" along the urethra, so as to form a separate canal above the normal passage. This upper tube at its anterior extremity was about 2" in diameter, becoming gradually narrower till it ended in a blind sac. It was lined with mucous membrane throughout. The anterior border of the membranous septum being inclined downwards, the upper canal viewed from before had an infundibuliform aspect, while the diameter of the normal canal was narrowed by it so as to be scarcely greater than 1". Thus the probe as well as the tube for inflation of the bladder, on being introduced into the urethra, passed into the funnel-shaped opening of the upper canal, and it cannot be doubted that, had a catheter been tried during life, it would have run into the same anormal passage.—(*Walther und Ammon Journ. f. Chir. und. Augenheilk.* 1847, Bd. vi. H. iv. p. 492.)

ETHER SUCCESSFULLY EMPLOYED IN THE FORM OF ENEMA. By DR G. V. CUMMING, Madras.

A NATIVE boy, of about twelve years of age, was carried in from a neighbouring village in a most pitiable condition. He had fallen from a tree twelve days previously; the right humerus, near the shoulder joint, was fractured, the forearm completely mortified, and the gangrene had extended considerably beyond the elbow joint; but the line of demarcation, pointing out the dead from the living parts, had formed. It was determined, therefore, to amputate the limb, but first to try the effect of the vapour of ether, and in the form of enema, as lately practised here, with success, by my predecessor, Dr Preston. The instrument consisted of a large bladder, with an ivory enema pipe attached—the ether, however (an ounce), first poured in. The little patient being too weak to sit, the lower part of his body was propped up, the instrument then introduced, and a basin, full of boiling water, placed under the bladder of the machine; so that the bladder was partly immersed in the water, some of which was also repeatedly thrown over the bladder; but, though the latter soon expanded, after persevering in the attempt some seven or eight minutes, merely a little temporary drowsiness was induced. Shortly after this the instrument was removed, and, conjecturing that some of the vapour must have escaped about the neck of the bladder, the ether which remained (nearly half an ounce) was thrown out. The pipe being then taken off, an ounce of ether from a *fresh* bottle was introduced; when the pipe, by tying cloth well round its base, was made—outwardly at its attachment—as air-tight as possible. The second attempt succeeded in two minutes; almost immediately after which—perfect insensibility being discovered to exist—the instrument was withdrawn. The arm, about two inches and a quarter below the level of the axilla, was removed by the double flap operation, which occupied but two or three minutes, the vessels were speedily secured, and all the stitches were completed but two, when, while inserting the needle, the patient for the first time awoke, uttering a faint ejaculation. The concluding stitch made him ejaculate slightly again; but, still drowsy, it was not till several minutes after the stump had been bandaged up, or till he was carried down from the surgery to the native ward, in a detached building, that he knew the operation had been performed. Very little blood was lost during the operation, though, from its having been performed so high up, no tourniquet could be applied. The hemorrhage was effectually checked by firm pressure, with the finger, in the axilla.—*Madras Athenæum*, June 1847.

ON THE USE OF THE NITRATE OF SILVER IN THE CURE OF ERYSIPELAS. By JOHN HIGGINBOTTOM, F.R.C.S.E., Nottingham.

MR HIGGINBOTTOM has found that, if the nitrate of silver be applied *early*, it subdues local inflammation and irritation, if we employ, at the same time, the most efficient means for regulating the digestive organs.

At an early period of his practice, in slight cases of erysipelas he used constitutional remedies alone, hoping that the inflammation would have been arrested; but, having been so often disappointed, he now uses both local and constitutional remedies simultaneously, and especially the nitrate of silver. Even in mild cases of erysipelas, in which he did not apply the nitrate of silver, he found the disease very long in duration, and observed that the patients had sometimes numerous small abscesses requiring the use of the lancet, which might have been prevented altogether by the early application of the nitrate of silver.

The objections he formerly entertained to the very early application of the nitrate of silver, were the pain and inconvenience attending the discoloration of the part on which it is applied, which remains for a week or more; but these objections are trifling compared with the continued severity of the disease, if permitted to run its usual course, particularly on the head, in which there is also great danger of inflammation of the membranes of the brain and of serous effusion. He has found that, when the inflammation has been subdued by an early use of the nitrate of silver, the constitutional symptoms were immediately relieved; the constitutional disturbance is directly aggravated by the least increase of local inflammation, and in a few hours, after a decided application of the nitrate of silver, the inflammation is arrested and gradually subdued, and with it the constitutional symptoms cease.

Even in idiopathic erysipelas, there is no period of the disease when he would not apply the nitrate of silver. He has never in any cases seen metastasis, or any other bad effect from the use of this important remedy.

When it is necessary to apply the nitrate of silver over an extensive surface as in erysipelas, he has for some years used the concentrated solution in the manner proposed by Mr John Gooch, surgeon R.N., in a paper published in the *Lancet* of September 15th, 1832, entitled "Practical remarks on Erysipelas as it appeared on board his Majesty's ship, *Prince Regent*." The strength of the solution is not given in this paper; he prescribes it in the following manner:—

R.	Argenti Nitratis.....	scr. iv.
	Acidi Nitrici	gtt. vj.
	Aquæ Destillatæ.....	dr. iv.

In erysipelas of the face, when it is spreading on the forehead, or at all on the scalp, the head should be shaved as early as possible, in order that we may trace the extent of the inflammation on the scalp, which often can only be detected by pain, or by an œdema being felt on pressure with the finger. The affected part should be well washed with soap and water to remove any oily substance from the skin, and afterwards with pure water, to wash away any particle of soap remaining. The concentrated solution may then be applied several times on the inflamed part, and for two or three inches beyond the inflamed margin on the healthy skin. It requires to be applied very freely all over the scalp, where it scarcely or never produces vesication.

In about twelve hours it will be seen if the solution has been well applied. If any inflamed spot be unaffected by it, it must be immediately re-applied to it. Sometimes even after the most decided application of the nitrate of silver the inflammation may spread, but it is then generally much less severe, and it is eventually checked by the repeated application of this remedy. He has in some cases of traumatic erysipelas, found the inflammation spread more severely and more rapidly than in the idiopathic, but by the free repeated application of the nitrate of silver, it has at length been subdued.

The great obstacle to the general and free use of the nitrate of silver, even at the present day, appears to arise from the impression on the minds of many surgeons that it is a caustic—a destructive agent. If they could be divested of that idea, and use it as freely as they would a common blister of cantharides, their fears would soon subside, from repeatedly observing the safety of the application, and also its beneficial effects. Mr H. considers it a safer remedy than cantharides, as it may be applied freely over a surface, even where very active inflammation exists, or where there is an extensive surface denuded of its cuticle. This

remedy has also the advantage of not affecting the bladder, or producing strangury.

The nitrate of silver is not a caustic in any sense of the word. It subdues inflammation, and induces resolution and the healing process. It preserves, and does not destroy, the part to which it is applied. If we compare a caustic, as the hydrate of potassa, with the nitrate of silver, we find that the hydrate of potassa destroys and induces a slough and the ulcerative process; but if we touch a part with the nitrate of silver, the eschar remains for a time, and then falls off, leaving the subjacent parts healed.

If an ulcerated surface secreting pus be touched by the nitrate of silver, the succeeding discharge is immediately converted into lymph: it is the property of the hydrate of potassa, on the contrary, to induce not only ulceration but suppuration. In short, the peculiar properties of the nitrate of silver have long been kept unknown to us by the designation of lunar caustic, affording the most striking instance of the influence of a term, or of a classification, upon the human mind. The nitrate of silver and the hydrate of potassa (as indeed all caustics), are as the poles to each other, the first preserves, the second destroys; the first induces cicatrization, the second ulceration.—*Read at the Prov. Med. and Surg. Assoc.* Aug. 4th, 1847.

ON DR DOUGLAS MACLAGAN'S METHOD OF DRESSING BLISTERS.

In the May number of this Journal, Dr D. Maclagan inserted a note on the advantage of dressing blisters with cotton wadding. The Editor of the *Journal de Médecine* has added to the translation of this note some observations not undeserving attention. He asks if the pain has any thing to do with the beneficial action of the blister? Is it not rather a complication, the effect of which on the economy, may, under certain circumstances, be injurious? In the eyes of most practitioners, there cannot be revulsion without pain. A celebrated surgeon some months ago, in one of his lectures, opposed the employment of ether during the application of the actual cautery. "They seek to prevent pain," he said, "and it is precisely pain which is necessary in these cases." Now, the proof that this pain is not necessary, consists in the fact, that the action of the actual cautery is as good in persons submitted to etherization, as in those who are tortured with their sensibility intense. In the wards of M. Guersent, we have seen the red-hot iron passed over the affected articulations whilst the children were plunged in a profound sleep. The insensibility was even attempted to be prolonged by enveloping the cauterized part with compresses saturated in cold water, and yet the resolution of the engorged tissues was produced as rapidly as when the patients were submitted to the greatest agony. In fact it is not the most painful blister which produces the best effect, and we know that the necessary irritation should not be too violent in order to obtain good results.

Since the extraordinary action of ether has been discovered, we may ask what part pain acts in disease, and whether it is not most frequently a cause of destruction rather than of health? If, then, the ordinarily acute pain of a blister can be prevented by the process of Dr Maclagan, it ought to be generally adopted.—*Journal de Médecine et de Chirurgie Pratiques.* Juillet, 1847.

HURTFUL EFFECT OF ETHERIZATION IN AN OPERATION UPON A GUN-SHOT WOUND. By MR LAWRENCE of Montrose.

As every fact bearing upon the subject of etherization is at present interesting, I make no apology for troubling you with the following brief statement:—

On the 27th ult., E. C., a boy 16 years of age, while firing off a pistol received a part of its contents into the thumb of his left hand, by which fully one-half of its second phalanx was carried away. He was brought to my surgery immediately after, and before proceeding to remove the remaining portion of the

phalanx at the articulation, I employed the inhalation of ether in the usual manner. About a minute sufficed to bring him so much under its influence as to permit the necessary incisions without struggling, though not without cries. After sponging the stump, and while seizing the needle to secure the flap by a stitch, he awoke so far to a perception of what was passing, as urgently to request more of the ether to prevent him "feeling the pain." After a few deep draughts of the vapour, the narcotic effect was so powerfully produced, that the needle was thrust through the flap and the skin on the opposite side without the least manifestations of suffering. In a minute or two after, on being spoken to, consciousness returned; but scarcely had he expressed surprise at what had been done, when he was seized with a severe *epileptiform fit*, by which his whole body was violently agitated, and his face rendered turgid and distorted. On being laid in the recumbent posture, and having fresh air freely supplied, he soon recovered, but felt much exhausted, and declared he had (while under the ether) been dreaming he was in a deep pit, from which he thought he would never be extricated. He was shortly after able to walk home. Next day he was perfectly well, and has continued so; and the wound is healing satisfactorily.

The recorded cases wherein the employment of ether inhalation has been attended with injurious or unpleasant results seem to range themselves under two classes:—First, In a certain, and probably much the larger proportion, the unfortunate phenomena have been such as practitioners have been accustomed to observe occasionally in similar circumstances, before the inhalation of ether was thought of; and while this by no means *disproves* the agency of ether in their production, it ought to prevent the "*post quod ergo propter quod*" mode of reasoning into which we are so readily betrayed. Secondly, In a limited number the unfortunate sequences have been such as could hardly be accounted for, otherwise than by a reference to ether as their immediate cause. To the latter class, I apprehend, belongs the case now narrated. The boy had never previously had a fit, and neither the injury received, nor the subsequent operative procedure, can with much plausibility be regarded as the *direct* agent in the production of the one referred to. But the question may be asked, Is it likely that the injury (being a contused lacerated wound, involving nervous and tendinous structures) had *predisposed* to the occurrence of the fit? The affirmative is perhaps not altogether a gratuitous assumption. It is well known that such accidents are often the cause of traumatic tetanus, at times following pretty closely upon their occurrence. Is it, therefore, a departure from sober theorizing to suppose, that in this instance there had been produced upon the nervous centres such a degree of that specific morbid impression which in certain circumstances issues in tetanus, as proved adequate, under the superadded influence of the ether, to occasion the paroxysm referred to? From one such case, and even from several, it were obviously unsafe to draw a decided inference; but the gradual accumulation of such facts will, by-and-by, afford the requisite data whence the desiderated laws of the influence of ether may with some certainty be determined. Meanwhile, this much may be averred, that *facts*, no less than the spirit of true philosophy, rebuke, on the one hand, that scepticism which would reject *in toto* the use of this potent agent, because in some cases unpleasant results have attended it; and, on the other, that false confidence in its virtues which asserts, that under no circumstances can its employment prove injurious.—*Communicated to the Editors Month. Journ. in a Letter.*

CASE OF WOUND OF THE INTERNAL CAROTID ARTERY, AND DIVISION OF THE PAR VAGUM, IN WHICH THE COMMON CAROTID ARTERY WAS TIED.

The following case is reported by Mr S. W. Fearn:—

Mrs Osborn, aged 68, was attacked by a man, on the evening of the 2d of February 1847, armed with a pruning-knife, who inflicted three severe stabs on the

left side of the neck, and several others, of a more trifling nature, on the jaw and over the larynx. Profuse hemorrhage followed, inducing syncope and insensibility, which lasted for an hour. There was a deep wound, about three inches in length, immediately behind the angle of the jaw, extending perpendicularly downwards; another, a jagged wound, over the transverse processes of several of the middle cervical vertebræ, and a deep gaping wound midway between the occipital spinous process, and the mastoid process of the temporal bone; the other wounds were slight. The hemorrhage, when seen by Mr Fearn, was reduced to a slight oozing. On rising from bed about an hour after this, arterial blood issued profusely from the wound behind the angle of the jaw, in a stream as large as a swan-quill. This bleeding was arrested by pressure, and as it was thought impossible to get at the vessel at the seat of the injury, a ligature was placed on the common carotid artery below.

After the operation, she had much trouble to make herself understood, and there was considerable difficulty in swallowing; but whether this arose from debility, or from injury of the pharynx and glottis, it was not possible to make out. Each time, when she swallowed, the liquid seemed to get within the glottis, occasioning cough and retching, and many times during the night she vomited a dark-coloured fluid. For two days she suffered much from difficulty of swallowing and cough, and remained apparently in an insensible state. She then rallied, and appears to have been much relieved for some days, when she is again reported to be suffering severely from incessant cough and extreme difficulty of deglutition, which continued with occasional intervals of relief. The ligature separated from the carotid artery on the 21st day, and the wounds in the neck were entirely healed in a few days afterwards.

The cough and difficulty of swallowing seemed, however, to increase, and the trachea was constantly filled with bronchial secretion, which was expectorated in a frothy state. She complained of pain in the vertex, and coldness of the left side of the head. The prostration and debility gradually increased, and she sunk exhausted upwards of eleven weeks after the receipt of the injury, and the application of the ligature to the carotid artery.

On examination after death, there was found congestion of the posterior part of both lungs, the bronchial membrane was much injected, and the tubes filled with frothy muco-purulent matter—a firm coagulum existed in the contracted artery for some distance above and below the situation of the ligature. The posterior face of the internal carotid, immediately above the point where it is given off from the common trunk, presented a well-marked appearance of the wound, which had furnished the alarming hemorrhage at the time of the injury, and in the same situation the par vagum was found to have been divided. The edges of the wound had closely united, and there was no trace of the wound on the internal coat of the artery. The vessel, at the point of injury, remained permeable. The lower extremity of the divided nerve was shrunk and atrophied, and there was a slight enlargement (a neuroma) at the extremity of the upper divided end.—*Provincial Med. and Surg. Journal*, Sept. 8th, 1847.

PATHOLOGY AND PRACTICE OF PHYSIC.

DEATH OF A MEDICAL MAN FROM AN OVERDOSE OF THE SULPHATE OF STRYCHNINE, TAKEN INSTEAD OF THE SULPHATE OF MORPHIA.

DR WARNER of Bristol, U. S., had suffered for some time from an inordinate action of the heart, for which he was in the habit of taking sulphate of morphia. Being at Montpellier, U. S., in October last, he called at an apothecary's store, and asked for some sulphate of morphia. He received a bottle wrapped in an envelope, and on the second morning after he tore off the envelope, and took a

portion of what he believed to be sulphate of morphia. In about five minutes after the effects appeared—namely, a contraction of the throat and tightness of the chest, with rigidity of the muscles in attempting to move. He complained of want of air, and requested the window to be opened. Very soon a tetanic paroxysm occurred; the tetanic spasm was complete, his head was somewhat drawn back, his countenance livid, frothy matter issued from his mouth, and he uttered moans. The palpebra was in constant motion; the paroxysm lasted about five minutes, and then there was a period of partial calm. During this period of calm he had a difficulty of articulating with distinctness. He made several attempts to vomit by exciting the fauces with his fingers. There was contraction about the throat. In five minutes a little startling and stiffening of the extremities announced the commencement of another paroxysm, in which the whole body was immediately thrown into a tetanic state like the first attack, and in two or three minutes it proved fatal. About twenty minutes elapsed between the taking of the drug and the fatal event, the symptoms thus lasting about fifteen minutes. On the first attack of tetanus cold water was dashed on his head, face, and breast, and friction was practised on the extremities. When the paroxysm was over, an emetic of sulphate of copper and ipecacuan was administered, followed up by flour of mustard diffused in warm water. No effect resulted from these means. On the bottle being examined after his death, it was found labelled “Strychnine;” and it appears, that though Dr Warner had distinctly asked for sulphate of morphia, the apothecary had, in some mistake, given him sulphate of strychnine. The quantity taken is supposed to be from one quarter to half a grain. It is not stated that any post-mortem examination took place.—*Boston Medical and Surgical Journal*.

CASES OF CROUP CURED BY CAUTERISING THE LARYNX WITH A SOLUTION OF NITRATE OF SILVER.

1. DR W. N. BLAKEMAN has recorded two cases of the above. The first case is that of a child two years old, very fat and large for his age, and of the leucophlegmatic temperament. He was seen five hours after the commencement of the attack, when the skin was hot and dry, the pulse quick, with great restlessness, laborious breathing, and the hoarse barking or crowing sound peculiar to croup. A medicine composed of squill and ipecacuan was first given, by which vomiting was produced, with no relief; then six grains of calomel were administered, and the former mixture repeated, with the addition of five grains of tartar emetic; free vomiting and purging were produced, but with no relief. On the second day persulphate of mercury was given in doses of a quarter of a grain every hour, by each dose of which vomiting was excited, but without relief. On the afternoon of the same day Dr Blakeman resolved to try a strong solution of nitrate of silver. The application was made by means of a sponge, and the solution used contained a drachm of the nitrate to an ounce of water. The first application brought away a quantity of tenacious membranous matter, and a larger quantity by the vomiting, which soon followed. A second application was made ten minutes after, by which a still larger quantity was brought away, to the great relief of the symptoms. In five hours a third application was made with the same effect as to the discharge of membranous matter and the vomiting. The next morning the child was found entirely relieved from the disease.

In the second case, that of a boy six years old, with frequent pulse, skin hot and dry, breathing hurried and difficult, and loud crowing, the same application was made two hours after the commencement of the attack, when a discharge of tough phlegm took place and vomiting. After a second application the relief was complete.—*New York Medical and Surgical Reporter*.

2. DR LATOUR was sent for to a child of four years old, ill of sore throat, accompanied with a hoarse cough, fever, anxiety, and other severe symptoms, the uvula and right tonsil being covered with very adherent false membranes. Nitrate of

silver in the solid form was immediately applied to all the affected parts within reach, and a vomit was given, by which some fragments of false membrane were brought away. Next day the false membrane had not covered the left tonsil, but appeared to have extended downwards, and the larynx seemed to be involved in the disease, as there was a suppressed hissing cough, and a loss of voice. Objecting as insufficient to the usual method of applying the nitrate of silver in such cases, namely, by introducing into the back part of the throat a sponge dipped in the solution of the salt, Dr L. determined to use a stronger solution, between seven and eight grains to the ounce (fifty centigrammes to thirty grammes of water), and to adopt a method of applying it which he had already published in the "*Clinique des Hôpitaux des Enfants*." This consists in saturating a strong ball of lint in the solution, and conveying it to the opening of the larynx by means of a long curved pair of forceps, and then squeezing out the solution by compressing the ball between the blades. In this manner the solution was applied in the case referred to every eight hours for four days with complete success; the voice was first restored, and then the convalescence soon became complete.—*Gazette Médicale de Paris*, 21st August 1847.

The treatment at the commencement in the first case was surely dangerously active in a child of two years old. We have no doubt of the efficacy of the nitrate, but are not satisfied that Dr Blakeman's cases were true croup.

In the title of the last case, the solution is termed a saturated solution, which seven or eight grains to the ounce is far from being, the nitrate being soluble in its own weight of cold water.

ON THE TOPICAL APPLICATION OF CROTON OIL IN TUBERCULIZATION OF THE LUNGS. By RAYER.

M. RAYER employs the croton oil with advantage as a palliative in tuberculization of the lungs. He directs twenty-four drops for each friction. The patient is to pour by degrees this quantity of the oil on the anterior part of the chest, and to practise friction with the palm of the hand. The employment of the croton oil in this quantity, and in this manner, is attended, he says, with no danger, and it affords a very sensible relief to the dyspnoea, the restlessness at night, and the fever which so often distress such patients. The friction, he says, may be performed with the bare hand, the papulæ never arising on the palm, owing, doubtless, to the thickness of its epidermis.—*Gazette des Hôpitaux*, and *Gazette Méd. de Paris*, 21st August 1847.

We doubt if this mode of using the croton oil can be more convenient than the mode now so generally practised in this country; namely, by the application of a liniment containing from half a drachm to two drachms in the ounce of some such vehicle as the linimentum camphoræ. We are sure that, in children at least, the pure croton oil applied to the chest has produced very severe effects, and we have found no difficulty in bringing out a copious eruption by the liniment just referred to, which we regard as a most important addition to our means of counter irritation.

ON THE DIFFERENCES IN CAPILLARY APOPLEXY, ACCORDING TO ITS SEAT; THE NATURE OF THE DISEASE. By PROFESSOR DR J. F. H. ALBERS, Bonn.

THE occurrence of capillary or congestive apoplexy, without effusion of blood, has been proved beyond doubt by Cruveilhier and others. According to the latest researches, neither its seat nor mode of development is always the same. Capillary apoplexy may be seated in the brain or its membranes. When the membranes are chiefly affected, white milky spots are scattered over the arachnoid and pia mater, particularly in the spaces between the convolutions, and most commonly where the small vessels are most strongly developed. These are

frequently quite surrounded by the white cake, so as to become invisible. Nevertheless, the mass forming this white spot is seated rather in, than on the arachnoid, and but little of it can be scraped off by the scalpel. In it are found the simple exudation forms of plastic lymph, and also true inflammatory corpuscles. In true capillary apoplexy, the congestion is most frequently found on the superior surface of the brain, and next on the base, where it is most commonly found when the disease has presented the symptoms of a chronic inflammation. But little serum is found between the arachnoid and the congested pia mater. The author could not decide, in such cases, whether the membrane was more or less easily separated from the brain. Nothing remarkable was found in the brain except a toughness (*zahigkeit*) as in typhus, and the grey matter less injected than the white. How death occurs, with so slight an effusion and congestion of the meningeal vessels alone, is, though a fact, as yet inexplicable. The author quotes the case of a groom, who, after riding in very hot weather, suddenly died with these appearances. Similar appearances are found on opening those dead of delirium tremens. The vitality of the brain has indeed, in such cases, been injured by the abuse of spirituous liquors; its action is therefore the more easily obstructed. Identical alterations are found in the so-called *coup de soleil*—which has been commonly supposed to be an arachnitis, but this is not invariable. On account of the trifling degree of effusion, death cannot be referred, as in pleuritis, peritonitis, &c., to pressure on the contained organs, but only to irritation approaching to inflammation or to congestion of blood, most probably to both. As, however, in the above-mentioned diseased condition, the congestion is more in the membranes than in the brain itself, so congestion of its substance is not the sole cause of paralysis of the brain. Probably even when it occurs in the smallest vessels of the membranes, it proves a hindrance to the performance of the cerebral functions, in consequence of which death follows. Altogether, these circumstances lead to the conclusion, that the membranes of the brain stand in a much closer relation to it than the other serous membranes to the organs which they invest. In the few cases hitherto observed, the symptoms were sudden and extensive pain in the head, vomiting and speedy insensibility, with a quick hard pulse, together with those common to arachnitis.

Cerebral capillary apoplexy is much more frequent—in it the meningeal vessels are only moderately filled; but on slicing the brain a multitude of bloody points appear, out of which the blood oozes, so that the brain often assumes a reddish-yellow glimmer, there is a constant toughness of the brain, not, however, equably diffused, but often confined to isolated portions. The right hemisphere is more frequently altered than the left, and the white portion than the grey, which is often quite normal; the internal portions suffer more frequently than the external, and the cerebellum is often alone affected. The nature of the capillary apoplexy varies also with its cause, which may be, besides obstruction to the reflux of blood from the brain, certain conditions of the brain itself, as irritation or inflammation, with congestion; but as these are not always followed by capillary apoplexy, so some other condition would seem to be necessary for its occurrence. This would seem to be a speedy, copious, and uniform congestion of the blood, either in one part of the brain, or in the whole of it; and this may be caused by a peculiar alteration of the vitality of the part concerned, either by previous disease or the violence of the primary cause. The vital conditions, capable of so disturbing the circulation of the brain as to induce capillary apoplexy, are threefold, and seated in the brain itself, and one without it.

1st. A sudden cerebral irritation speedily reaching a certain height, and producible by irritants of the most varied character. The most instructive example of this is, a *coup de soleil*. The irritation of the arachnoid amounts almost to inflammation, yet no considerable effusion ever follows; this disease can, therefore, only exceptionally be reckoned as arachnitis. Isolated apoplectic-like attacks of young people, speedily called forth by vexation, anger, &c., and rapidly passing, appear to belong to this class.

2d. Cerebral inflammation, more particularly local, may arise suddenly and prove speedily fatal, or by inflammatory alteration of the brain cause paralysis, and then prove fatal by a species of slowly developing apoplexy. On cutting through the tough cerebral substance, numerous small bloody cysts (blut kapselchen) and red points are seen. The coagula in the neighbouring vessels, the inflammatory corpuscles and fibrine effused into the cerebral tissue, leave no doubt as to the inflammation of the diseased portion. By inflammatory exudation of another kind, the fibres of the brain become torn and separated, and the cerebral substance thus completely destroyed, yet remains tough (Zahe). This toughness seems to be a necessary condition for the production of capillary apoplexy, because in a tough state of the brain, the dilatation of the vessels is less easily affected, and the pressure caused by it proves more injurious; and therefore capillary apoplexy is rare in softening of the brain, this condition being then absent. It is also probable, that this tough condition of the brain is always caused by exudation of fibrine, and consequently by a more or less advanced degree of inflammation. The most convincing proof of this is given by the well-known observation of Pool, a man aged 24, who in early childhood had a severe fall on his head, and died of capillary apoplexy consequent on inflammation of the medullary substance of the brain.

Acute inflammation does not necessarily accompany softening of the brain; on the contrary, it is accompanied with considerable congestion of blood in the vessels, and is often followed by sudden death; so that, in it also, we cannot deny the influence of the pressure of the blood in producing sudden paralysis. But in this acute disease, the congestion of blood is extended over several portions of the brain; indeed often over both hemispheres and their membranes, whence its effects, even under a less degree of pressure, may be much more injurious. As a harbinger of capillary apoplexy, often preceding it for several days, and deserving of the most careful attention, may be remarked a pressing headache, lasting night and day, and not to be relieved either by local blood-letting, derivation to the skin or bowels, or narcotics; it is a sign of inflammation of the brain, and even where chronic demands a much more active treatment than it is wont to receive.

That irritation, congestion, and inflammation, paralyse the capillaries, causing an accumulation of blood in them, which, when it has reached a certain height, reacts more or less suddenly on the brain, paralyzing it, and producing a state which we call apoplexy, must be considered as proved: whether other conditions can produce capillary apoplexy remains to be shown. A knowledge of the proximate cause of the capillary apoplexy is of so much the more importance for the practitioner, as giving him thereby the possibility of discovering the proper remedy, which must be directed more against the cause of the paralysis than against the mere accumulation of blood.

3d. The third cause of capillary apoplexy is the congestion brought about by disturbances of the pulmonary circulation and respiration, as in diseases of the breast and lungs.—*Schmidt's Jahrbücher*, 1847, No. 8.

We consider that Professor Albers has confounded what British pathologists have long considered congestion, with true capillary apoplexy, that is, rupture of the capillaries, producing extravasation of blood; the white spots, also, to which he alludes on the arachnoid, are, in our opinion, rather evidences of chronic inflammation than of a mere hemorrhage.

UPON ANORMAL SOUNDS IN THE HEART AND ARTERIES, PARTICULARLY IN THE AORTA ABDOMINALIS. By DR PICKFORD, Heidelberg.

THIS paper contains a series of cases in which a blowing sound was heard in various arteries, chiefly in the aorta abdominalis. They comprise cases of gastrocismus, typhus, tuberculosis, cholera sporadica, nephritis, and one case ap-

parently healthy. All were more or less anæmic; and the author, after going over the various supposed causes of this sound, is induced to consider, 1st, From pathological cases—(*Legrand Gaz. Med.* 1834, No. 27;) 2d, From the ease with which blowing may be produced by pressing with the stethoscope on any superficial artery; and, 3d, From the experiments of Corrigan on partially compressed elastic tubes, &c.,—that it depends not upon an altered state of the blood, but on a spasmodic contraction of the arteries to which this disordered condition of the blood gives rise, and to which hyperæmia more than anæmia itself seems to dispose, agreeing so far with Beace—(*Comptes Rendus des Séances de l'Ac. de Sc.* T. xxi. No. 4, 7, Juillet, 1846.) He also supposes that this spasmodic contraction may take place in the heart, and thus cause those anormal sounds so often heard in anæmic and chlorotic patients.—*Zeitschrift für Rationnelle Medizin*, Bd. iv. 2, 3, heft.

MIDWIFERY, AND DISEASES PECULIAR TO WOMEN.

LESIONS OF THE NERVOUS SYSTEM, &c., IN THE PUERPERAL STATE CONNECTED WITH ALBUMINURIA.¹—Dr Simpson detailed some cases illustrative of the effects of Bright's disease, as denoted by the appearance of albumen in the urine under the action of heat and nitric acid. He drew the following conclusions:—

1. Albuminuria, when present during the last periods of pregnancy and labour, denotes a great and marked tendency to puerperal convulsions.

2. Albuminuria, in the pregnant and puerperal state, sometimes gives rise to other and more anomalous derangements of the nervous system, without proceeding to convulsions; and Dr S. had especially observed states of local paralysis and neuralgia in the extremities, functional lesions of sight (amaurosis, &c.), and hearing; hemiplegia and paraplegia more or less fully developed.

3. Œdema of the face and hands (going on occasionally to general anasarca) is one of the most frequent results of albuminuria in the pregnant female.

4. The presence of this Œdema (3.), or of any of the lesions of the nervous system (2.), with or without the Œdema, should always make us suspect albuminuria; and, if our suspicions are verified by the state of the urine, we should diligently guard, by antiphlogistic means, &c., against the supervention of puerperal convulsions.

5. Albuminuria, and its effects (1, 2, 3), are far more common in first than in later labours, and these constitute a disease which in general disappears entirely after delivery. But Dr S. had seen one case commencing with slight blindness, but no Œdema, and ending gradually in hemiplegia, where the palsy partially remained after delivery, and after the disappearance of the albuminuria. In another, amaurosis came on with delivery, and had been present for six months when Dr S. first saw her. She had no Œdema or other symptom of albuminuria except the amaurosis; but, on testing the urine, it was highly albuminous.

6. Albuminuria, with convulsions, &c., occurring in any labour later than the first, generally results from fixed granular disease of the kidney, and does not disappear after delivery.

7. Perhaps in puerperal convulsions, &c., produced by albuminuria, the immediate pathological cause of the nervous lesions is some unascertained but poisoned state of the blood. Was there a morbid quantity of urea in the blood? In several specimens of the blood of patients suffering under severe puerperal convulsions, furnished by Dr S. to Dr Christison and Dr Douglas Maclagan, these gentlemen had been unable to detect any traces of urea. Was the poisoning material caseine in morbid quantity or quality? The dependence shown by Gluge and others of albuminuria upon steatorrhea of the kidney, makes this connexion worthy perhaps of some inquiry.

¹ Omitted under proceedings of the Edinburgh Obstetrical Society last month.

8. In cases of severe puerperal convulsions, &c., from albuminuria, the renal secretion is in general greatly diminished, and Dr S. had found active diuretics apparently of great use along with or after venesection, antimony, &c., especially where the case was offering to become prolonged.

9. Sometimes hemiplegia supervened during pregnancy without albuminuria, but this form did not seem to interfere materially, or very dangerously, either with the pregnancy or labour—the disease running its own usual course. In one case Dr S. had seen the patient gradually but imperfectly recover the use of the palsied arm after delivery. In another no improvement occurred.

A CASE OF PREGNANCY AND PARTURITION DURING THE EXISTENCE OF CANCER OF THE UTERUS. By JOSEPH A. EVE, M.D., Georgia, U.S.

CARCINOMA of the uterus was formerly supposed to be incompatible with pregnancy; but the possibility of this complication with this disease in all its stages is acknowledged, and its influence in hastening its progress admitted, by all or nearly all authors who have recently written on the diseases of females. Pregnancy and cancer have each a prejudicial influence over the other—the former hastening the progress and fatal termination of the latter; and the latter in a considerable number, I believe in a large majority of cases, causing either an abortion or the death of the foetus when delivery occurs at the full term. The foetus sometimes perishes in utero, its farther development being prevented, and abortion the necessary consequence; in other instances the death of the foetus is the result of impediment to delivery, from the scirrhus enlargement of the mouth or neck of the uterus.

The object of the present communication is to give a brief history of a remarkable case of pregnancy and parturition in connexion with cancer of the uterus.

July 28th, 1845. I was called in haste, eleven or twelve miles in the country, to visit Mrs ———, who I was informed had been some time in violent labour. The child was expelled about half an hour after my arrival. I remarked a smell very similar to that of cancer of the womb, but did not at the time suppose it possible that it could be identical with it, for she was apparently in most excellent health, remarkably robust and stout, weighing not less than two hundred and fifty pounds, and being about twenty-eight years old, and furthermore, as the child to which she gave birth was large and healthy. Two or three months previous to her confinement, I was consulted by her family physician in reference to a sanguine discharge to which she had been subject for some time, and which I feared might depend on placenta prævia, but which I have no doubt now was consequent on carcinoma. I have since learned, upon enquiry, that as early as January she complained of severe pains in the region of the uterus, and that in the very commencement of gestation she experienced unusual sensations that caused her for a long time to doubt whether she was pregnant.

After her confinement Mrs ——— had an offensive discharge from the vagina. On expressing the opinion, when consulted in reference to it, that she was labouring under organic disease of the uterus, I was requested to visit her, October 5th, with a professional friend, and make an examination with the speculum. The touch discovered an extensive scirrhus enlargement of the neck of the uterus. We could not determine satisfactorily the extent of the ulceration by the speculum, because, before we could make a proper inspection, we were compelled to remove the speculum, for she became so excessively agitated that we feared an hysterical convulsion would have been induced. As she was young and remarkably robust, we considered this was a case in which every possible effort should be made, although even under such favourable circumstances we had scarcely the slightest shadow of hope—favourable, I mean, in reference to her age, constitution, and general health, but quite the contrary when viewed with respect to her recent gestation. We put her on the internal use of proto-iodide of mercury, and chloride of soda as a vaginal injection, with an occasional resort to the sulphate

of morphine, whenever pain might call it into requisition; she was however at this time, and for a considerable time after, comparatively free from suffering. We proposed to apply some cautery at another visit, when she might be sufficiently composed to bear its application, either the nitrate of silver or nitrate of mercury.

I was requested to visit her again, the 21st of the same month, sixteen days after my first visit. She had not yet lost her embonpoint, but the cancerous ulceration had made most frightful and destructive progress, having involved not only the posterior lip, but the posterior part of the cervix or body. It was now too late to think of any thing beyond palliative measures. We advised a lotion of the nitrate of silver, with the view of correcting the fetor and improving the condition of the ulcers, perhaps in some degree checking their course. After this she became subject to most alarming and exhausting hemorrhages at each menstrual period. She now began to lose flesh and strength rapidly, and to suffer severe lancing pains.

I visited her again the 6th of November. The disorganization was still more rapid, far exceeding any thing I had ever before witnessed. We endeavoured to support her strength by tonics, to alleviate her sufferings by opiates, to restrain the hemorrhages by styptics and astringent lotions, and to correct the horrible fetor by the chloride of soda. After the destruction of the posterior lip, posterior portion of the neck and body of the uterus, the ulceration extended through the vagina and rectum, allowing the fæces to pass from the latter through the former, and must have involved even the sacral plexus of nerves from the excruciating paroxysmal pains she suffered in that region. I never saw her after the 23d December, but was informed by my friend that she continued to linger in the most painful and deplorable condition until the 25th of June, when death kindly released her from sufferings indescribably severe, almost beyond endurance.

This case is remarkable, from having occurred in so young, healthy, and robust a subject, from the fact, that the process of gestation was conducted most perfectly, notwithstanding the presence of scirrhus certainly, and I think we may safely say cancerous ulceration, from the discharge and the characteristic fetor, parturition only being rendered somewhat more tedious and difficult. But if it is remarkable for the absence of any obvious effect of the cancer on the gestation, it is still more so for the very marked influence of the latter over the former. After delivery, the progress of the disease was extremely rapid, although in the early age, health, and vigour of the patient, it might have been expected to have run a slower and longer course.

Mr Lever considers twenty months to be the usual or average duration of uterine cancer. Dr Ashwell concurs with him, if he refers, as he doubtless does, to the stage of ulceration. I would suppose, from my own comparatively limited observation, that the ulcerative stage generally lasts at least twenty months. In this case, there intervened only eleven months between her confinement and her death, although she possessed uncommon vigour of constitution, and appeared to resist death much longer than any person could have supposed, considering the ravages of the disease and the intensity of her sufferings. I cannot speak positively with respect to the commencement of ulceration: I would infer, from the hemorrhages during gestation, and the fetid discharge during labour, that it existed previous to her confinement; but it certainly had not progressed far, even at my first visit, more than two months afterwards; it was so superficial that it was not evident to the touch, and, as I have remarked, in consequence of her extreme agitation and excitement, the examination by the speculum was not satisfactory. It is singular that ulceration had made comparatively so little progress, between the time of her confinement and my first visit, and so much between my first and second visit. It is probable, if I could have made a satisfactory examination at my first visit, a larger ulcerated surface would have been discovered; but, after making all due allowance, I am confident it was very limited compared with the progress made at my second visit.—*Southern Med. and Surg. Journ.*

ON CERTAIN CHARACTERS OF URINE DURING PREGNANCY. By DR REGNAULD.

THE urine contained in a glass vessel, and placed in a chamber, the minimum temperature of which is 14° to 16° , loses its transparency on the second or third day, and becomes more and more opaline. From this moment it ceases to redden litmus paper, and restores the blue colour to reddened paper. From the third to the fourth day, a slight white precipitate is deposited on the sides of the vessel, and there appears on the surface the first traces of a white pellicle, which, increasing in extent and thickness, generally covers the surface on the fifth day. There may also be seen in eight-tenths of such cases, crystals, which appear as brilliant points—the pellicle continues to increase up to the sixth or seventh days, but the advancing putridity now causes the phenomenon to lose its regularity, and it cannot be characterised in an exact manner. According to M. Regnauld, this pellicle, observed on the surface of urine from pregnant females, is only an exaggeration of the normal state. In such cases there is a hypersecretion of an albuminous matter, which has escaped the oxygenation that ought to have converted it into urea. Hence arises a part of the pellicle, which is otherwise very complex in its composition. During the last days of pregnancy it cannot be discovered. This depends, according to M. Regnauld, on the abnormal elimination from the kidney, of a variable proportion of lactine or sugar of milk, which, by being transformed into lactic acid, sensibly increases the acidity of the urine, and prevents the ammoniacal transformation of the urea. He believes with several observers, among whom he cites Scherer, that these characters of the urine may be found in certain morbid conditions independent of pregnancy. He believes that too much importance has been attributed to them as a means of diagnosis. Yet, where the sensible signs of pregnancy are not very manifest, they ought not to be neglected, and may have a certain value in connexion with the rational signs of that state.—*Revue Médico-Chirurgicale*, Avril 1847.

CASE OF SPONTANEOUS RUPTURE OF THE UTERUS IN THE THIRD OR FOURTH MONTH OF PREGNANCY. CONGENITAL ABSENCE OF MUSCULAR WALLS OF UTERUS ON ONE SIDE OF FALLOPIAN TUBE AND OVARY, AND OF THE OS TINCÆ.

THE following remarkable case is related in the Buffalo Medical Journal (Sept. 1846). An unmarried girl, æt. 17, was suddenly seized with pain resembling colic, and soon died.

On post-mortem examination the following appearances were observed. Cavity of the abdomen filled with coagulated blood, surrounding a fetus of the third or fourth month. Uterus ruptured along its right side, from the fundus to near the neck. Left ovary and Fallopian tube antirely wanting. Right ovary and Fallopian tube were present, but the latter entered the uterus high to the neck, and the ovary was correspondingly lower than natural.

The uterus appeared to be developed to the size of the third or fourth month of gestation. The placenta was attached near the fundus upon the left side. On the side of the uterus where the rupture occurred the walls of the organ were extremely thin. At the time of the rupture there seemed to be nothing but the peritoneum, and in the immediate neighbourhood the friability was such that it was readily torn by the fingers with slight force. On the left side the walls were of the usual thickness, but did not present the common fibrous appearance. On examination of the neck no aperture could be found, nor was there any trace of an os tincae. The neck resembled a tendon in appearance, but was of less density. There was no communication between the cavity of the uterus and the vagina. Professor F. H. Hamilton, to whom the ruptured uterus and fetus were sent, gives the following further description of the parts. "The uterus is torn near the attachment of the right broad ligament; walls on this side excessively thinned, so much so in some places as to leave little else than peritoneum; opposite side of uterus about $\frac{1}{3}$ of an inch in thickness, and of usual

consistence. Length of body of uterus, 3 inches; breadth, $2\frac{1}{2}$ inches; cervix uteri 2 inches long, firm and broad at base, but gradually and regularly tapering to a thin, flattened extremity, covered by a smooth mucous membrane. Upon careful examination of the cervix, longitudinally and transversely, no channel, or line, or cicatrix, indicating the presence of an os tinæ, could be discovered. The structure is rather more dense than that of the uterus in its natural state, but its density is uniform; it is not scarred or corrugated, nor does it present any evidence of its being a result of disease. The mucous coat which covers it lies free, and can be as easily removed as such membranes usually may be. The right Fallopian tube terminates about the middle of the right side of the uterus; right ovarium is larger than natural, but apparently healthy. On the left side there is no broad ligament, no Fallopian tube, nor ovarium, but the peritoneum covers it as smoothly as in front." The girl had always enjoyed good health, and had menstruated regularly three years, until the last three months.—*American Journal of the Medical Sciences, and Medical Gazette*, Sept. 17, 1847.

FORENSIC MEDICINE.

TRIALS OF TWO HUSBANDS FOR POISONING THEIR WIVES WITH ARSENIC. JOHN THOMPSON, AT CARLISLE; THOMAS LEITH, AT EDINBURGH.

Two recent trials of husbands for poisoning their wives with arsenic, so strikingly coincide in their principal features as to deserve to be considered in intimate connexion.

TRIAL OF JOHN THOMPSON AT CARLISLE.

At the Cumberland assizes, August 4th, John Thompson was tried before the Lord Chief Baron Wilde, for the murder of his wife in the city of Carlisle. The parties had been married for about twenty years; they had gone to reside in Carlisle not much more than a year before the death of the wife, and no mention is made of a family. In the month of September, Thompson accidentally became acquainted with a widow to whom he represented himself as a widower, and, forming an intimacy with her, he at last came to pass the night habitually with her, leaving his wife alone at home. The widow after some time began to suspect that he had a wife still alive, and in the month of April, to satisfy her, a false certificate was sent to her of the death of his wife several years before. Doubts were raised as to the authenticity of this certificate by some of the widow's neighbours, owing to there being but the Carlisle post-mark on the outside, though it professed to come from a distance. Of these doubts Thompson was made aware. In the beginning of May, about three weeks before Mrs Thompson's death, her husband purchased arsenic from a druggist under pretence of poisoning rats, giving a false account of his place of residence. A second time, about a week before his wife's death, he purchased arsenic in the same shop. For about three weeks before her death, Mrs Thompson often complained to a neighbour that she was sick and like to vomit, and had a hot burning in her throat, and that her eyes were much affected, and to that neighbour her face had appeared during that time to be much swelled and altered in appearance. On the 25th of May, there was a veal pie for dinner at Thompson's house. The next day his wife, though able to go out in the morning, was seen by several neighbours to be very ill; and several persons to whom she gave away portions of the pie were ill for several days after partaking of it, the symptoms being sickness, vomiting, pain and swelling of the stomach and bowels, and in one case sore throat—in consequence, the chief part of the pie given to the neighbours was thrown away. Thompson himself, who was present when one portion of the pie was given away, said it had disordered him the day before. On the 27th, two days after, Mrs Thompson was found in bed very ill, affected with severe vomiting and

purging, a burning heat in the throat, and prickling in her hands and feet, with excessive thirst, to quench which she had been drinking oatmeal and water. The neighbours sent for Thompson from his work, and insisted on his going for a doctor. He appears to have gone straight for Mr Mortimer, who could not come immediately, but saw her about mid-day. She told him that she had been very ill at the time he was sent for, but was then better. There was pain and tenderness over the stomach, and on the sides and lower part of the belly. Hot fomentations for the belly, and effervescing draughts, were prescribed. She continued to drink oatmeal and water throughout the 27th. On the morning of the 28th, she was still so ill that the neighbours sent for Thompson from his work to get the doctor again. Mr Mortimer, on the morning of the 28th, found her sitting on the edge of the bed, her countenance exhibiting stupor, much depression, and great anxiety; she said she had been very ill, and had been vomiting much. A piece of solid opium was administered, a prescription was given for the vomiting, and hot fomentations were again ordered, with a direction that Mr Mortimer was to be sent for if she was no better in the afternoon. She said she thought herself better in the afternoon—Mr Mortimer was not sent for. A little boy was sent to enquire for her on the morning of the 29th, who thought she was sleeping: about eleven o'clock Mr Mortimer called and found her dead—he judged that she might have been dead for three hours. Beside her bed were several empty mugs, and one containing meal and water, which drink she used without intermission during the two previous days, and most probably from an earlier period, as she was seen at her window on the 24th stirring up something in a mug. During Mrs Thompson's illness there was no one constantly with her in the house. Her neighbours, though with her for considerable periods, were not with her during the night; and Thompson, as had been his practice for some time, passed the night at the widow's during her illness as before, coming to his own house merely for a short time, at a very early hour, before going to his work. He was seen leaving his own house as usual before six o'clock on the 29th; and when told that she was dead some hours after, he expressed surprise, saying that she told him in the morning that she was a good deal better, and that she asked for a bit of bread and butter before he came out. On going home to his house soon after his wife's death, he found some of his neighbours there, who told him the doctor had given express directions that the vessels containing what his wife had vomited should not be emptied. He began to dress himself for the purpose of going for the coroner, while the house was put in order by a girl he had sent for, her instructions being to preserve the contents of the vessels referred to. Thompson, however, appears to have been left alone in the house before going to the coroner; and before evening the contents of these vessels, as well as some other articles, had disappeared. He was the first to inform the authorities of his wife's death on the day that it happened, and his house was taken possession of and shut up by them the same evening, though he was not apprehended till June 8.

In the mean time he associated with the widow, and at times, at least, represented the deceased, not as his wife, but as his landlady, though he was deterred from pursuing this deception by the prospect of thus losing the furniture of his house; and, before he was apprehended, he had made some attempts to get the furniture disposed of. When the authorities took possession of the house, the dust taken from the pockets of his working clothes, from a till in a tool-chest, &c., was carefully collected, put up, and handed over to Dr R. Elliot. A bag of oatmeal had been observed in the house, but was not carried away till June 7th, when it was given to Dr R. Elliot; the house, however, had been locked up, and under the care of a police-officer all the while. On the third day after the death of the deceased, Thompson accompanied the police-officer to the house, and, being asked if he wished any thing out of it for the funeral, he said no, but there was a bag of oatmeal which he would like to have, as he was fond of porridge for supper, though it is alleged that he had

been heard to say that he could not take porridge. The officer refused to give him the oatmeal.

On the post-mortem examination, the stomach was found inflamed and ulcerated, the ulcerations being of the size of split peas, with vascularity around the patches; the intestine adjacent to the stomach was also inflamed; the stomach contained merely a few ounces of gruel-like fluid, slightly tinged with bile; the rest of the internal organs were those of a very healthy person. The gruel-like fluid in the stomach did not unequivocally show the presence of arsenic; but from the solution obtained by boiling the stomach after being cut in pieces, and also from the intestines treated in the same manner, arsenic was obtained in abundance. Arsenic was also found in the dust collected from two of the pockets of Thompson's working clothes, and from the dust of the till of the tool-chest; while in other packages of dust collected from his pockets and from the house, there was no arsenic discovered; in *the oatmeal which Thompson wished to take away*, three days after his wife's death, arsenic was unequivocally detected, while in some barley meal taken from the house no arsenic was discovered.

These facts were brought out with much distinctness by numerous witnesses, and, instead of being shaken, were on many occasions amplified and confirmed by the cross-examination. An elaborate defence was made by the counsel for the prisoner. The learned judge took a comprehensive view of the whole evidence, and the jury found the prisoner guilty on the counts charging him with mixing arsenic with the oatmeal designed for the food of the deceased, while they passed over the charge as to mixing poison with the veal pie. He has since been executed.

In this case, the proof of the deceased's death being the effect of arsenic was beyond all doubt; and the evidence, though purely circumstantial, affords a complete moral conviction of the prisoner being the guilty instrument. It is idle to insist that circumstantial evidence is fallacious, and therefore to be held insufficient in the case of a capital charge. It may have been a good rule to distrust mere circumstantial evidence in charges of murder, when juries were disposed to rely on a mere "prima facie" view of a case, and when external violence, so commonly capable of direct proof, was the usual mode of perpetrating the crime. But now that the proportion of murders by poisoning is obviously on the increase, our rules of evidence should be accommodated to the nature of the proof, of which alone the crime of poisoning is most commonly susceptible. The proof of such a crime is necessarily circumstantial; it can hardly be in any case other than circumstantial; and it is perfect when the circumstances favourable to guilt are numerous, free from contradictions, giving each other mutual support, and coincident in their bearing on one only of the possible views of the case. Thompson's unprincipled character, his attachment to another woman, his desire to appear to her a widower, his failure in the deception by which he tried to prove himself a single man, his purchase of arsenic under a false designation, the coincidence between that purchase and the commencement of his wife's illness, the disappearance when he was alone in the house of the contents of the vessels ordered by the surgeon to be carefully preserved, his desire to get possession of the neglected box of poisoned meal after his house was shut up, and his pretence that he liked oatmeal porridge, though he had been heard to express his aversion for such food, are all powerful circumstances which mutually support each other, and coincide in their bearing on his guilt. It is quite possible that a man may be an abandoned character, that he may be unfaithful to his wife, and that, to gain the affections of another woman, he may pretend to be single, and forge a certificate of his wife's death for that purpose; that he may have arsenic in his possession, and yet that his wife may die by accident or by her own hand from the use of that poison; but when no diligence of solicitors or ingenuity of counsel can produce any circumstance bearing out either of these two sup-

positions, can the most determined scepticism uphold the one supposition or the other in opposition to the purchase of poison coincidently with the commencement of the wife's illness, the purchase of it under a false name, the removal, while the house was under his care, of the matters vomited by his wife, which he knew had been ordered to be preserved, his pretence that he liked what he disliked, to get possession of what, if innocent, he must have suspected might have been the cause of his wife's illness? These last circumstances stand in lieu of direct evidence, and the first-named circumstances corroborate the inference drawn from them. Thompson, though plainly not an unintelligent man, did not act as an innocent man would have acted. But though not unintelligent, he did not act as a guilty man would have acted who was more conversant with the kind of evidence on which a conviction is obtained in trials for poisoning; and it is plainly owing to the ignorance of guilty parties on this head, that convictions are at present obtained. And therefore, as the knowledge spreads of the points on which conviction is dependent, the greater will be the necessity for pressing on the attention of that part of the public from which juries are drawn, the sufficiency and force of a refined train of circumstantial evidence.

Fortunately the old difficulty as to the establishment of the presence of arsenic, still the most common poison, has now nearly vanished. In this case the medical evidence was most satisfactory; and though the counsel for the prisoner chose to browbeat the medical witnesses, he came off second best. Not to speak of the judicious and well-merited reproof he received from the presiding judge, he showed himself up in a way satisfactory enough to all who understood the subject. The unhappy prisoner at the bar might well exclaim, "Save me from my friends!" There was no fault could justly be charged against the steps taken by the Drs Elliot to establish the presence of arsenic. Mr Mortimer, however, who seems to have escaped all blame, acted contrary to all rules in judicial medicine when he left the matters vomited by the deceased unsecured.

TRIAL OF THOMAS LEITH AT EDINBURGH.

On September 1st, Thomas Leith was tried in the High Court of Justiciary, before the Lord Justice-Clerk, Lords Cockburn and Wood, for the murder of his wife, by poison, in the town of Dundee. The Lord Advocate and George Deas, Esq., deputy-advocate, conducted the case for the Crown; James Moncreiff and A. S. Logan, Esquires, appeared for the prisoner.

Leith and his wife had been married for about twenty years, and had several children, but did not of late live happily together. In the winter of 1845-46, Leith had a shop-girl who lived for a short time in his house, and by whom he afterwards had a child. For a considerable time before his wife's death he had become desirous of a separation, one reason he assigned for which was, that the phrenological development of her head was not such as to fit her for his wife. She was at first averse to the separation, and up to the time of her death the terms had not been agreed upon; and he seems to have latterly used her worse than usual, for the purpose of forcing her to come into his terms. For a considerable time before her death he appears to have used such language in speaking of her, as to lead some of those with whom he conversed to apprehend that he would do her some mischief. Some three or four weeks before Mrs L.'s death, his tea was sent down by his wife from the house to the shop, as usual, by one of the children, and a short time after he said he was sick, and chewed tobacco and drank warm water. He then vomited. His illness he ascribed to something having been put in the tea, in consequence of which Dr Lyell was brought to see him by a person who had by chance come into the shop. Before the doctor came, what he had vomited had been thrown out by his own direction. When the doctor saw him, he looked pale as if he had been vomiting, and complained of pain in the stomach and bowels. He said he had

taken one cupful of the tea, and then had been called away to serve a customer, and when he returned to the back-shop, after half an hour, he felt himself sick, and vomited. Dr Lyell prescribed an emetic, and on pouring out the tea, seeing a white powder in the bottom of the flagon, he took it away with him, and afterwards found the powder to consist of from 40 to 50 grains of white arsenic. When Dr Lyell returned in less than two hours, he found Leith apparently quite well, his wife being present, who, having heard that he was ill, had come to inquire for him. On being asked, she said that there was nothing unusual in the tea when she sent it; that it was a part of the same tea which she and her children had partaken of; and that, if any thing wrong was in it, he must have put it in himself. Though Leith took no active steps to have this matter inquired into, and though the police had made inquiries without finding any ground for suspecting Mrs Leith, and though he even made a merit of hushing it up, he did not cease to insinuate to many that his wife had attempted to poison him. And accordingly, though after sending for blankets to pass the night in his shop, he consented to sleep at home with his wife that night, yet he never afterwards slept or took his meals there, owing, as he insinuated, to his apprehensions of being poisoned. Some of those with whom he was accustomed to converse, believed from the first that he had himself put the poison in the tea, and even told him so—and on one occasion, defending himself from this imputation, he said, "That something would soon happen to convince every body that he was innocent of what was laid to his charge." Leith was known to have arsenic in his possession prior to the discovery of it in the tea, though, when questioned on the subject, he hesitated, and said the poison which he had got for rats was brown, though white arsenic had been previously seen in his possession. Though, after the tea story, Leith had altogether given up frequenting his own house, he was observed to enter it on one occasion, about a week before his wife's death, and remain there for a quarter of an hour, when his family were at church, and for this purpose he borrowed a key from one of the neighbours. While he thus absented himself from his family, he refused to give his wife money for common necessities, and even beat her, and threatened that he would wash his hands in her heart's blood. On the morning of her death there was but a small quantity of oatmeal for porridge; and, finding the quantity of porridge she had made was too little for all, she took some barley-meal which had been laid aside as being of bad quality, and made porridge for herself, which she took, and gave a smaller quantity to some of the children, after they had taken their oatmeal porridge. Three weeks before *scones* had been made of the same barley-meal, which were objected to as being sandy, but no bad effects had followed the use of them. Soon after breakfast, Mrs Leith and the children who had partaken of the barley-meal porridge were taken ill. The evening before she had sent to ask her minister to come to converse with her about the proposed separation from her husband, and, on going to visit her that morning about eleven o'clock, he found her and the children very ill, vomiting and purging. Immediately suspecting poison, he ran to Leith's shop, and told him that his wife and children were under poison: Leith said, "They have tried to poison me, but that failed, and now they have done it for themselves." The minister insisted on his going to see them; he at first resisted, pleading that he must attend to his shop, but finally went there with the minister. He was then urged by him to go for a doctor, and went out for that purpose. In the mean time, the deceased stated to the minister what has been already mentioned as to the two kinds of porridge for breakfast, saying the poison must have been in the barley-meal, and that it must have been put into it on the Thursday previous, when she and her children were at church. The minister secured the bag with the residue of the barley-meal, taking care to have it first identified distinctly by her and one of her girls. No doctor had come, and the minister went back to Leith's shop, and found him there, when he said he had been to three doctors, and all were from home. On the minister again

urging him, he sent his shop-boy for Dr Gardner, who lived about half a mile off, desiring him to tell Dr G. to come to his shop. Dr G. was not found, and finally Drs Crichton and Nimmo were got by the minister. The stomach-pump and other means were used, but all in vain. About an hour before her death, the minister said to her, "Annie, you are going to eternity. I urge on you affectionately, if you know any thing respecting this matter, throw off the lie, tell every thing you know, and don't go to eternity with a lie in your mouth." She turned round, smiled, saying,—“My dear father, I have told you all the truth.” Leith was not present at his wife's death; he had left the house about two hours before, on the minister refusing to give him the bag of barley-meal, which he saw him holding in his hand, though nothing had been said in his presence to indicate the importance attached by the minister to the possession of the bag.

Besides the bag of barley, containing about a handful, the matters withdrawn by the stomach-pump, and a small quantity of the matters vomited, were secured with the requisite precautions by the doctors; the rest had been thrown away.

Leith was apprehended the same evening, and deliberately affirmed, with reiteration, to the superintendent of police, that he had not been alone in his own house for the previous six weeks.

On the post-mortem examination of the body of the deceased, the vessels of the brain were found somewhat turgid, and there was a small quantity of serum at its base; in other respects, the contents of the head were natural; the contents of the thorax were natural, with the exception of some old adhesions between the pleura pulmonalis and pleura costalis. On opening the abdomen the stomach and bowels were externally red. On slitting up the gullet the two upper thirds were found natural, the inferior third somewhat red, gradually deepening as it approached the stomach, which was of a dark red colour throughout the whole inner surface, and in some parts there was an appearance as if blood had been extravasated underneath the villous coat. The duodenum was red and vascular at its juncture with the stomach, but fainter in colour and less vascular as it retired from it. The great intestines and the other viscera were natural. The report is signed by Messrs Crichton and Nimmo. A second report signed by the same gentlemen, and also by Alexander Hamilton, chemist, describes the result of the analysis of the residue of the barley-meal, and of a portion of the contents of the stomach collected at the post-mortem examination, and the presence of arsenic was unequivocally proved in both by an infallible series of tests—namely, the proper colour of the precipitates, the reduction to the metallic state, and the re-conversion of the metal to the state of arsenious acid, and a subsequent application of the liquid tests, with the proper result. A third report from Dr Douglas Maclagan of Edinburgh, applies to the state of the stomach, and a portion of the gullet and of the small intestines, to the nature of the fluid drawn off by the stomach-pump, and that of a portion of the barley-meal, all of which with proper precautions had been sent to him. The mucous membrane of the stomach, and the part of the intestines sent to Dr M., presented an appearance corresponding to that described by Messrs Crichton and Nimmo, and in the tenacious mucus of the surface of the stomach were found crystallized grains having the form and appearance of arsenious acid; while in the fluid which had exuded from the stomach and intestines, in the fluid drawn off by the stomach-pump, and in the barley-meal, arsenic was unequivocally detected by the application of Reinsch's process.

As to the cause of death there could be no doubt; and the facts before stated were brought out with much distinctness on the trial by many witnesses. The Lord Advocate addressed the jury for the crown, and Mr Moncreiff for the prisoner, after which the Lord Justice-Clerk summed up and commented on the evidence at great length. The jury, after deliberating for upwards of an hour and a half, returned a verdict finding the prisoner, by a

majority, guilty in terms of the libel, but unanimously recommended him to mercy. The Lord Justice-Clerk said that he would immediately transmit the evidence in the case, along with the recommendation of the jury, to the proper authorities ; and, on speaking to his brethren on the bench, they thought it right, in order to do justice in the case, that they should know the grounds on which the recommendation had been given. The chancellor of the jury said, he believed that the grounds upon which the jury had come to that resolution were, that they disapproved of the punishment of death as one element, and also that a considerable number of them would otherwise have brought in a verdict of not proven.

In this case, as in the former, arsenic was indisputably the cause of death ; and the guilt of this is brought home to Leith with irresistible force, not by direct evidence, but by a train of circumstances which compel assent. In the former case, the circumstances inferring guilt were of a broader and more decided character. Here, the chain of circumstances, though equally decisive of the guilt of the party, is of a finer cast, and requires a nicer degree of pains and judgment for its just appreciation. There was not an equal proof of an essentially unprincipled disposition in Leith, as in the case of Thompson ; though the suspicions entertained of him before the murder by some of his acquaintances, are presumptive evidence of his character being known to be bad. It would not, however, have been admissible to set him down for a man likely to commit murder solely because he was unfaithful to his wife, and had become a Socialist. The incident of the poisoned tea begins the evidence against Leith. Here there is not the shadow of a ground for suspicion against his wife. It is one of the established facts of the case, that he put the poison himself into the tea. Every one who considers the whole circumstances brought out on the trial, will be convinced of this. A second point against him is, not the having poison in his possession at all, but his having misrepresented the nature of that which he said he got against rats to Mr Johnston, the minister, when asked if he ever had any. Another important point against him is, his private visit to his own house, when he knew his family would be at church, for just such a number of minutes as would suffice to introduce arsenic into a bag of meal, and on the very day on which he had announced that an event would soon happen which would clear him from the charge of putting poison into his own tea to inculcate his wife ; and this visit, it is to be remembered, he took pains to deny when apprehended. These may be regarded as the most direct of the circumstances against him ; and how well is the inference from these corroborated by the other minor circumstances, while not a single particular turns up favourable to the supposition either of suicide or of accident ? What, then, are these minor circumstances ? Among these, are his attachment to another woman, whom he continued to visit privately up to the time of his wife's death. His recent threats against his wife's life ; his refusal latterly to give her the means of supplying food to herself and children, so likely to force her to have recourse to the barley-meal, before thought unfit for use ; his manifest reluctance to lend assistance or get medical advice, when told by Mr Johnston that his wife and family were suffering from poison ; his message to Dr Gairdner, not, as was most natural in an innocent man, to go to the house where his wife was ill, but to come to the shop, where he might first see him ; and, finally, his desire to get possession of the bag in which was the residue of the poisoned barley-meal, of which he could know nothing had he been innocent of introducing the poison into the meal.

Yet with these elements of the most complete moral conviction before them, some of the jury doubted of his guilt. It appears that out of the fifteen jurymen, ten voted him guilty, and five for bringing in a verdict of " not proven." Surely these five jurymen had not well considered the nature of the evidence, to hold a doubt under such circumstances. Or, rather, had they had not de-

ceived themselves with the fallacy, that circumstantial evidence is an insufficient ground of conviction, and, though morally convinced of Leith's guilt, were short-sighted enough to allow, as far as in them lay, a monstrous crime to pass unpunished, by availing themselves of the intermediate verdict of "not proven," permitted by the law of Scotland? We must say, however, that, consistently with their oaths, these jurymen were not entitled to propose such a verdict, unless they did in truth entertain a doubt of Leith's guilt. If they were morally convinced of his guilt, they were bound to bring him in guilty; the intermediate verdict is allowable only when the evidence does not go beyond strong suspicion. We have observed of late some expressions of regret among medical jurists in the southern part of the island, that such an intermediate verdict as "not proven" is not permitted by the law of England. We cannot join in this regret. We think it deserves the consideration of the legislature, whether the "not proven" of the Scotch law be not of pernicious influence in capital cases. It nourishes the fallacious distinction among jurymen between moral conviction and legal evidence, and permits weak-minded men to compound with their consciences, and throw off the burden of being instrumental to a capital punishment, even with irresistible evidence before them, particularly in that most dangerous and most frequent of crimes in advanced states of society—secret assassination by poison.

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVI.

MEETING XIV.—*Wednesday, 7th July.*—Dr HAMILTON, President, in the Chair.

ON THE MORBID ANATOMY AND PATHOLOGY OF THE TYPHOUS FEVER WHICH HAS BEEN PREVALENT IN EDINBURGH DURING THE SESSION 1846-7. By Dr HUGHES BENNETT.—Until the commencement of the present session cases of intestinal lesion in typhus fever have been exceedingly rare. During the three years the author had acted as pathologist to the Royal Infirmary previous to the present session, and during which he had examined upwards of 500 individuals who had died of the disease, he had only met with it three times. Since November, however, they had become more frequent, and the author had studied the morbid anatomy of typhus in reference to the opinions put forth by Rokitsanski, Engel, Hammernick, Gunsburch, and other pathologists of the Vienna and Prague schools of medicine. According to these observers, typhus fever is accompanied by a peculiar dyscrasia of the blood, and the intestinal and other lesions produced, contain a peculiar deposit called typhous deposit, which bears the same relation to the constitution of the blood as tubercle and cancer do to the tubercular and cancerous cachexiæ.

The cases of fever admitted into the Royal Infirmary from the 1st of November 1846 to the 30th of June 1847, amounted to 2071. Of these 278 died. Owing to the imperfect arrangements which exist in the Royal Infirmary, however, regarding post mortem examinations, only 63 of these cases had been inspected after death. It was upon these data that the following statements were founded.

The organ most frequently affected was the spleen. In the majority of cases it was more or less enlarged and softened, presenting a mahogany brown

colour and creamy consistence ; so that when pressed, nearly the whole of its parenchyma could be squeezed out of its capsule. In ten cases the spleen contained one or more infiltrated masses of typhous deposit, of a yellowish or brownish-yellow colour. In two cases the deposit had softened and burst into the peritoneum, causing fatal peritonitis.

The organs most frequently affected after the spleen were the lungs. The most common lesion was bronchitis, the bronchial lining membrane being of a deep mahogany or purple colour, more or less infiltrated with serum or exudation. The fine bronchial tubes were frequently filled with a muco-purulent matter, more or less fluid, and in a few cases choked up with a reddish-brown creamy substance, probably a modified form of the exudation (typhous deposit) described by Remak. The apices of the lungs were very commonly cedematous, yielding on section a copious, greyish, frothy fluid. In fifteen cases the lungs were more or less consolidated by exudation, which seldom presented the characters of normal hepatization. It was sometimes of a dirty-yellow tint, at others of a brownish chocolate colour, existing in masses of irregular outline and of variable size, resembling the typhous deposit previously spoken of, occasionally found in the spleen. In three cases there was pulmonary apoplexy.

The intestines presented the lesion so well described by Bretonneau, Louis, Cruveilhier, and others (dothineritis, typhoid ulcer, &c. &c.), in 19 cases. The peculiar elevation, sloughing, and ulceration of the round and oval patches had been carefully observed, and the anatomical description given by Mr Goodsir (Monthly Journal, p. 353—1842) for the most part confirmed. The elevated patches had been observed occasionally to extend as high as the duodenum, and as low as the rectum. In one case numerous dothineritic elevations, about the size and shape of a split pea, extended all over the ascending and transverse colon. In a few cases the isolated follicles in the large intestines were observed swollen and empty, presenting in their centre a dark-blue or black spot. In others, the round and oval patches of the small intestine were hypertrophied, elevated above the mucous membrane, and of a greyish or slate colour. Perforation of the intestine, causing fatal peritonitis, had occurred in three cases. Dysentery, with flakes of lymph attached to the mucous membrane, over the ascending and transverse colon, was associated with intense dothineritis in one case. Oval and round cicatrices, exhibiting different stages of the healing process of the intestinal typhous ulcer, were observed in two cases.

In all the cases where the intestinal ulcerations were recent, the mesenteric glands were enlarged, soft, and friable, and of a greyish or reddish purple colour. Some of these glands reached the size of a hen's egg. On section they presented a finely granular surface of a dirty yellow-greyish, or dark fawn colour, produced by infiltration of the typhous deposit, which was generally soft and friable, but sometimes in one or more parts of the swollen gland broken down with a fluid of creamy consistence.

In two cases there were glossitis and laryngitis, with tonsillitis ; in one case abscess in the kidney, and in one an abscess in the posterior mediastinum.

The brain did not appear to participate much in the disease. It presented only occasional congestion, with slight effusion into the sub-arachnoid cavity, or into the lateral ventricles.

The blood in the great majority of cases was fluid, and of a dirty-brownish colour. In those instances, however, where the disease had been protracted, and especially in such as presented well marked typhous deposit, firm coagula were found in the heart and large vessels.

In seven cases no lesion whatever could be discovered.

The typhous deposit consists of a yellowish or flesh-coloured exudation, sometimes passing into a brownish colour, from the admixture of more or less blood. When first formed it is of tolerably firm consistence, as in the spleen and glands of the mesentery and intestines, but rapidly undergoes the process of softening. In parenchymatous organs it may be slowly absorbed or resolved,

as in the lungs, spleen, and mesenteric glands, or it may produce ulceration or gangrene. In either case, should the individual recover, cicatrices with puckering in the tissue are produced, the parenchyma around the deposit having contracted and become indurated. On mucous membranes the deposit is thrown off in the form of slough, and discharged by the excretory passages, leaving a characteristic round or oval-shaped ulcer. This process may be followed by cicatrization or perforation of the gut by ulceration. In the former case it leaves a round or oval-shaped depression in the mucous membrane, often of a blue colour, which subsequently becomes covered with epithelium : in the latter case it produces death by peritonitis.

The minute structure of the typhous deposit varies in different situations. In the lungs, spleen, and intestinal canal, it contains at an early stage a number of roundish or irregularly shaped corpuscles. They are about the $\frac{1}{100}$ of a millimetre in diameter, contain several granules, with a nucleus about the $\frac{1}{500}$ of a millimetre in diameter. Acetic acid renders them more transparent. They are conjoined with numerous granules and molecules, which become more abundant as the process of softening advances. In the mesenteric glands a higher degree of cell formation takes place. Cells are formed about the $\frac{1}{50}$ of a millimetre in diameter, containing from two to six, and sometimes even more nuclei, which become very distinct with thick edges on the addition of acetic acid, whilst the cell wall is partially dissolved. The same cells may occasionally be seen in the elevated typhous deposits of the intestinal glands. Sometimes the only appearance observable in the deposit, is that of numerous molecules and granules mixed with blood corpuscles.

Dr Bennett considered that the pathology of this affection consisted in a primary alteration of the blood, caused by the peculiar miasm or poison causing typhus fever ; that, under such circumstances, local inflammations were set up in particular organs, and that the exudation attending it, instead of presenting the usual appearances, and undergoing the usual transformations, became modified so as to constitute the typhous deposits.

Dr Christison noticed, some time ago (Art. Fever, Lib. of Medicine), that the Edinburgh fever was especially characterised by pulmonary complications. We know that in France and Germany intestinal lesions are most common. During the present session, however, the latter had also become frequent in Edinburgh, and it was a fact worthy of observation, that this change occurred at a time when scurvy and bad diet prevailed, circumstances often known to be associated with this form of fever in France and Germany.¹

Numerous preparations were shown to the Society, exhibiting typhous deposits in different organs.

Dr Alison expressed his strong sense of the obligation the society was under to Dr Bennett for this communication. The subject was one of much interest. There were two forms of fever at present prevailing in and about Edinburgh—the common typhus and the short fever : the enteric fever described as occurring in Glasgow, has not been so well marked here. He should like to know if deposits of the kind referred to by Dr Bennett had been observed both in the common typhus and in the short fever. In typhus, Dr Alison had remarked, decolorised deposits were not uncommon ; these decolorised deposits he regarded as the results of the inflammations which supervene in typhus. Dr Alison had not observed these decolorised deposits frequent in the spleen.

Dr H. Douglas said, they were not connected with the severity of the symptoms, nor had they any observable relation with affection of the intestines.

¹ The Edinburgh fever epidemic of 1846-7, has found an excellent historian in Dr Bennett's pathological assistant, Dr Waters, whose Thesis on this subject was crowned with a gold medal at the graduation of the present year. We trust that the Essay will shortly be published.—Ed.

Professor Syme had some doubts as to the diet of the peasantry abroad being inferior to that of the same class in this country.

The President remarked, that the diet of the peasantry varied much in different countries, and referred to the great difference between the diet of that class in Belgium as compared with that of the same class in Piedmont.

Dr Gairdner wished to understand on what grounds *Dr Bennett* inferred that the deposits found in fever were of the same nature in different organs. Did he trust to the microscope, to chemical examination, or to the mere outward appearance of the deposit?

Dr Bennett replied, that he relied on careful examination, combined with the use of the microscope. The typhous deposit is very different from pus, and also from hepatization; the organisation of its constituent elements is, for the most part, less complete, although, in some cases, a high degree of cell development had been attained.

Dr Alison was willing to rely on *Dr Bennett* as to the existence of a sufficient distinction between purulent and other ordinary deposits and that peculiar to typhus; but he asked whether *Dr Bennett* considered typhus to depend on the deposits. The blood might be so affected in fever, that such deposits will take place when inflammatory action arises; but he presumed the deposits have nothing to do with the existence of the fever.

Dr Bennett stated that such was his own opinion. He regarded typhous deposits as local exudations, modified by the pre-existence of typhus fever.

DOES ETHERIZATION INCREASE OR DIMINISH THE MORTALITY ATTENDANT UPON CAPITAL OPERATIONS IN SURGERY? A STATISTICAL INQUIRY. By Professor SIMPSON. —The substance of this paper will be incorporated in Professor Simpson's communication contained in the last and future numbers of this Journal. Its general object was to show that surgical operations, performed under the influence of ether, were not only less painful but less fatal than those performed without it.

Professor Syme said, that the use of ether for preventing pain, had seemed to him, from the first time of its proposal, a subject of great importance and requiring the most careful trial. In conducting this investigation, he anticipated difficulty, from the public prejudging the question at issue, through the natural desire of deciding it in favour of their wishes. He was therefore sorry, but not surprised, to see the newspapers from day to day inflaming the public mind with exaggerated pictures of "painless operations," and to find the operating theatre of the Royal Infirmary converted into a place of public amusement; on entering which, upon one occasion, he was led to suppose, by the persons he saw there assembled, that he had, by mistake, gained admission to a meeting of presbytery. He reminded the Society that he had, all along, deprecated discussion of the ether question, until time had been afforded for observation and reflection; and mentioned that he had recently committed to the press the first statement of his opinions upon this subject, which he had felt warranted to offer, as the result of actual experience.¹ From the first trials that he had made of the ether, he was by no means prepossessed in its favour, and had abandoned it for a time, as Mr Liston and several other surgeons had done. He had been induced to resume it, however, from hearing that these gentlemen had done so, on the introduction of a more perfect mode of administration. He himself had latterly used a simple and efficient instrument, which he understood was the invention of *Dr Simpson*. He expressed his persuasion that many other members of the profession, enjoying opportunities of hospital practice, were pursuing similar courses of inquiry, and would communicate their results so soon as they were satisfactorily ascertained. He therefore regretted that the circular of the Professor of Midwifery had extorted expressions of opinion which must be deemed crude and imperfect, inasmuch as their authors had not felt disposed to offer them spontaneously.

¹ Monthly Journal, August 1847.

He stated his conviction, that in a question such as the one under discussion, statistics, however plausible, were the most fallacious means of settlement; and that, if any man wished to prove the worse the better reason, or to make black appear white, he could not proceed upon grounds better suited for the purpose. He remarked, that the cases of amputation adduced in favour of etherization, were probably more favourable for success than those occurring in the ordinary course of practice; as, after the Grantham inquest, it would require no small boldness to operate, after the administration of ether, in unpromising cases. He also remarked, that when he had mentioned the high average of mortality attending amputation, it was with the view of enforcing the importance of employing certain modes of procedure; and that, if these had been practised, as he trusted they had been, in the recent period to which the history of ether was limited, the alleged superiority of success would admit of a different explanation. He stated, in conclusion, that some time ago the Professor of Midwifery had, apparently to his own satisfaction, if not to the conviction of the Society, proved, from the evidence of "statistics," that ovariectomy was not more dangerous than the great operations of surgery,—such as lithotomy, amputation, and the ligature of arteries; and that to show how far statistics were to be trusted in deciding such questions, he would mention the following facts of Edinburgh practice. In the Royal Infirmary, since adopting his present plan of lithotomy, he had performed seventeen operations, of which two only had proved fatal, and these in patients labouring under disease of the kidneys. He had performed amputation at the ankle in twenty cases without a fatal issue, except in one instance, which was that of a girl, who he believed had died from the effects of ether; and he had tied the femoral artery seventeen times without a single instance of fatal effects from the operation. He had thus performed fifty-four operations of the kind alleged to be on equality with ovariectomy in respect to danger, at the rate of one death to eighteen cases. The Professor of Midwifery had stated that ovariectomy was an operation too startling for any cases of its performance "to remain hid," and therefore it might be concluded that all the instances of its performance by Edinburgh practitioners, since its late revival, were the three operations performed by the Professor of Midwifery, or under his auspices, the whole of which had proved fatal.

Dr Simpson observed that Mr Syme had quite mistaken the object of his (Dr Simpson's) investigations, when he thought he had asked for the *opinions* of his correspondents about etherization and its effects. He was quite aware, as Mr Syme had properly stated, that an individual's opinion about it might have changed once or oftener since its introduction, according as the cases under that individual's own range of observation seemed to be favourable or unfavourable to its use. It was to correct this very form and source of error, that Dr Simpson had undertaken that investigation, the results of which had just been stated to the Society; for no limited number of cases—such as fell within a limited time under any one surgeon's observation—was a sufficient basis on which to decide the question, whether etherization decreased or increased the usual mortality attendant upon surgical operations. He had asked his correspondents not for their individual *opinions*, but their individual *facts* and cases. The statistical tables he had shown to the Society, contained not an accumulation of opinions, but an accumulation of facts;—and it was only on such a large and accumulated basis of facts, that any thing like a correct and accurate judgment on a problem of this kind could ever be formed. In this and all similar inquiries, truth and precision could only be attained by generalizing upon large numbers. If we generalized from few and isolated cases we were ever apt to be deceived, however anxious we might be to avoid a wrong judgment and erroneous conclusion. If, for instance, the proportion of the two sexes to each other had been a perfectly new and undetermined problem (such as the problem whether does or does not etherization render surgical operations more fatal)—and if he had asked a hundred correspondents to tell him their *opinions* of the proportions between the two sexes, as derived from the limited

observation of data which they were able to make in their own individual families—he would have found some reporting highly in favour of the preponderance of males born, others highly in favour of the preponderance of females born, and others doubtful, &c. But if he had asked these hundred correspondents merely to report to him the *facts*, of the exact number of boys and the exact number of girls in their individual families, and thus got 3 or 400 cases or data to calculate upon, he, no doubt, would have found, as is always found in European society in its present state, that the proportion of males born to females born, was nearly as 104 or 106 to 100. This was precisely the plan and principle which Dr S. had adopted for the investigation of the effects of etherization in surgery. Mr Syme had objected altogether to the statistical method of inquiry in a surgical matter of this kind; but, with Professor Miller, he could not believe that Mr Syme was in earnest in bringing forward such an objection. Certainly statistics had hitherto been little used in surgery, but assuredly they were becoming, and would become more and more introduced; and as assuredly they would yet prove an invaluable means of aiding its advances, clearing its inquiries, and settling many doubts and questions in it. And that Mr Syme did believe in their applicability to surgical investigations was evident, from a paper which Mr Syme had read to the Society a year or two ago. In that paper Mr Syme justly urged the propriety of attaining some new method of amputating the thigh, for this statistical reason, among others, that amputation of the thigh, when performed according to the prevalent modes, was so fatal, that the “stern evidence” of hospital facts proved, that from 50 to 70, out of every 100 persons operated upon, died. The argument was a most proper one to use, provided Mr Syme believed in its justness. But it would have been most improper for Mr Syme to have used such an argument for such a purpose, provided he did not believe in its truth and justness. In regard to ovariectomy Mr Syme argued, that because three cases of it had terminated fatally in Scotland, it was a decisive statistical proof that this operation was always unsuccessful. Dr Frederick Bird of London had performed ovariectomy six times, and had lost, he believed, none of his patients; and hence, on the same ground, Dr Bird or others might as justly argue, that the operation of ovariectomy was always successful. The one conclusion would be as correct as the other, yet both would be most evidently absurd and erroneous. But combine these two sets of cases, the 6 and 3, and out of them you have a mortality of 1 in 3, which is nearer the truth; but to get the full truth you must collect all the unsuccessful, as well as the successful cases, and the proportion between them will alone show you the correct results, and proper conclusion. Mr Syme, from a list of cases of operations which he mentioned (selected in regard to time), had attempted to show that by statistics you cannot arrive at true results. Most certainly not, provided you select your cases either in regard to time or ought else; or provided you only take a small number to found your calculations upon. You must take all your cases at all times, and omit none. It ever and anon happened, that a surgeon had a run of good or a run of bad cases; and either would deceive, if alone selected and calculated upon to show the average mortality. The very groundwork and essence of all statistical inquiries was, that all comparable cases should be included. Toss up a well-balanced penny 2 or 300 times, and at the end you are pretty certain to find that it has turned up nearly an equal number of times heads and tails. But still there might be a run of heads, or a run of tails for 5, 10, or 20 times in succession, without, if the penny were a well-balanced one, the general result being at all altered. Mr Syme had alluded to lithotomy. Out of the first 40 patients on whom the late Mr John Bell performed lithotomy, not one died. But when his operations amounted nearly to 100, his mortality was so much reduced as to be not much above the usual average. Mr Martineau had published results so very successful, as apparently to surpass all others in lithotomy. He recorded only 2 or 3 lost in some 80 or 90 cases. But these results did not

include *all* his cases; they were selected in regard to time, for they included only a certain number of years; and when his unsuccessful were added to his successful years, there was nothing extraordinary in the general average that the whole presented. Statistics might easily be thus perverted if improperly used, but so could every other instrument of reasoning and investigation. If, however, only *all* the data, facts, or cases, were correctly laid down, and correctly analysed and calculated, it was as impossible to misrepresent, to one's self or to others, the result, as it would be to represent the addition of 2 and 2 to be 5, or 8, or aught else than 4. In the statistics of other amputations which he had laid before the Society, he had faithfully entered every one fact and case reported to him, whether favourable or unfavourable, whether good, bad, or indifferent. Many of his correspondents were quite adverse to the use of ether, others in favour; and in none, did he believe, that the cases for etherization had been selected by the operators in any way whatever. Dr S. had not ventured to calculate up the results till the preceding day. They had both surprised and gratified himself; and he felt assured, that the profession would look upon them as possessing some interest and weight in the decision of this most important inquiry.

Professor Miller stated that Mr Liston's position in regard to the use of sulphuric ether, in the present day, he considered as quite different from that of Mr Syme. Mr Syme had been a lukewarm adherent at the first; had tried it and failed, and tried it again; had entirely abandoned its employment, and denounced it *ex cathedra*; and now, at the eleventh hour, had become its advocate and supporter. Mr Liston, on the contrary, had been the means of introducing it into this country, and had been one of the most zealous and enthusiastic of its adherents; as, indeed, a published letter of his to Mr Miller, abundantly testified—a letter, by the way, which, having been read to Mr Miller's class within an hour and a half after its reception, had been the first announcement of ether's anodyne use on this side the Tweed. Mr Liston had been a zealous supporter of ether from the first; afterwards, had cooled a little, but had never wholly abandoned it; and was now as enthusiastic as ever in its application to surgery. The period of his coolness he (Mr Miller) understood to have been of short duration; and caused mainly by the unsatisfactory results of insufficient exhibition—while at the same time, doubtless, he had been somewhat soured by the crowding and jostling of spectators in the confined spaces of the London operating theatres, as well as by the loss of time which the imperfect exhibition of ether must ever occasion in private practice. Mr Miller had no doubt the dentists every where were beginning to rue greatly their loss of time by ether; but now that its exhibition had been simplified and improved (and in London taken up as a distinct profession), the objection on that score had been in great part, if not altogether removed. There had been crowding of the operating theatres here too, but of this Professor Miller was far from complaining. He thought it but natural that the public, so much interested in the discovery, should wish to see its application practically tested; and he thought it but fair and right that their curiosity should be indulged, provided such indulgence did not interfere with the economy of the Institution. Mr Syme had alluded to his having apparently found himself in presence of a presbytery one day, in the hospital theatre. He alluded, Mr Miller supposed, to the presence of Dr Chalmers. In that case, Mr Miller could readily pardon the misapprehension; for that illustrious man might well be held as a representation of an entire presbytery in his own person. His presence on that occasion was felt by Mr Miller to be an honour to the Institution and the cause, and most consistent with the character of that most illustrious of Scotchmen; who was not so much, certainly not more, distinguished for his eminence as a divine, than for the broad and pure philanthropy which ever interested him most keenly in the promotion, by every means, of the temporal as well as of the spiritual interests of his fellow men. As to Mr Syme's censure of statistics, he was at a loss to understand what was meant; unless, indeed, the whole of that

part of the statement was intended to be jocular. (This intention, however, was disclaimed by Mr Syme.) Well, then, he could only repeat that he was unable to comprehend the drift and grounds of Mr Syme's argument in this instance; unless it were to employ statistics when they suit, but to repudiate them wholly when unsuitable. They had often heard, lately for example, of the superiority of ligature of the femoral artery over compression, for the cure of popliteal aneurism, illustrated and enforced, if not proved, by Mr Syme's list of seventeen successive and successful cases. If this were to be taken as a sample of the veracity of statistics, certainly the effect would be unfavourable to the value of statistics in general, leading to an erroneous conclusion; for, without wishing to be a prophet of evil, there could be no difficulty in foretelling that unfortunate cases would come to cut down the average; just, as he remembered Mr Liston had at one time cut twenty patients for stone without losing one, and then lost several in succession. In like manner, doubtless, the statistics of ether will be found to vary; and it is only by striking the average of a large and varied number of cases, that any satisfactory result can be attained. So far as statistics had already gone, it was most gratifying that the balance was found so decidedly in favour of the abolition of pain; and he coincided quite with Dr Simpson in the ardent hope and belief, that the further evidence of accumulated statistics would only tend more and more to confirm this great result. Mr Syme had said that the employers of ether had been bold; he (Mr Miller) would rather say they had been judicious; seeing that, hitherto, in the multiplicity of patients and appliances, so few casualties had occurred; doubtless because a good selection had been made in the cases on which the new agent had been brought to bear. It was doubly gratifying that so few accidents had occurred, and that statistics, ably handled by Dr Simpson, had shewn that not only was pain removed, but life also saved, in the greater operations of surgery. Dr Simpson had naturally chosen amputations as the test; and it would not do for Mr Syme to say, that the superior successes in that operation within the few past weeks were due to improvements in the mere art of amputating. Such improvements, even when they did exist, travelled with wonderful slowness; and, but the other day, Mr Syme had been heard publicly stating that his excellent operation at the ankle-joint had not yet been received in the London hospitals. He (Mr Miller) thought that the Society were deeply indebted to Dr Simpson for the valuable communication he had made. For his own part, he had always, from the first, been a humble, but zealous and consistent advocate for ether's use in surgery (applied with judgment and discretion); he doubted not but, ere long, it would be confirmed as a large and grateful boon to suffering humanity; and, in the future, he would always hold it to be a stout argument on the ether's side, that so decided an opponent as Mr Syme had now become an authority in its favour.

Dr Hughes Bennett observed, that as the discussion had very naturally turned upon the general value of medical statistics, he could not forbear expressing his belief in the correctness of Mr Syme's opinion, namely, that they were very fallacious. This conviction had been forced upon him in the situation he held as pathologist and keeper of statistics to the Royal Infirmary. It was well known that the managers of that institution had done all in their power to encourage, and gone to considerable expense in order to obtain, a perfect series of tables. Dr Bennett had taken the greatest pains to further their endeavours, and for that purpose had introduced into the tickets suspended over the bed of each patient, all the statistical information required, which tickets corresponded to the admission papers kept in the waiting room, showing the daily admissions and dismissals taking place. In addition to this, an assistant visited each ward daily, and ascertained from the patients themselves the circumstances required, which were compared with the tickets and daily tables. Before each patient left the hospital, the physician himself writes on the ticket the name of the disease, its complications, day of dismissal, and termination of the case. A register is kept in which all the patients admitted are daily entered, their age, occupation, residence, &c., inserted; and, when they go out,

the name of the disease and its termination is copied from the ticket signed by the physician. It would be thought that such a system approached as near perfection as was possible, and yet from the occurrence of circumstances which it was not necessary to particularize, but which in a large institution it has been found impossible altogether to prevent, the correctness of the returns is more or less vitiated. Hence, notwithstanding all the care employed, one source of error arises from the imperfect working of a complicated system.

But supposing that this difficulty could be overcome, an insurmountable obstacle even to approximative correctness in medical statistics, exists in the present state of medical science itself. When in arithmetic we add two figures together, each figure represents the like thing. The same with respect to quantities in algebra, or lines and spaces in geometry. But age, constitution, idiosyncrasy, locality, disposition, season, complication, diet, treatment, and numerous other circumstances, produce such differences that it is scarcely possible to find two cases of disease alike in all points, while in the majority of instances they are very dissimilar. Thus, when we add one case of so called pneumonia to another, we do not possess the sum of two like quantities as is represented, but of two dissimilar ones, and the error thus produced is increased when we add a third, and so on. The correctness of this principle at once becomes evident on entering minutely into particulars; for it would be easy to prove, 1st. That the greatest difference exists as to the meaning both of pathological and nosological terms. 2d, That supposing the meanings of terms were fixed and recognised, the condition they are supposed to represent are in many cases not to be detected by the most skilful in diagnosis; and in the majority of cases where such skill does not exist, are only in a few cases recognizable with certainty; and, 3dly, The conditions themselves, or the pathological states, are continually mistaken, even when the body is dissected (which happens in comparatively few cases), on account of our imperfect knowledge of morbid anatomy, the naked eye being altogether unable in many instances to detect the nature of the morbid lesion when present.

It must therefore be evident, that the statistics which are at present drawn up by an institution like the Royal Infirmary, must necessarily be fallacious, notwithstanding all the care employed; and, supposing that the physicians and surgeons had reached the highest point in the cultivation of the art. If such be the result with respect to tables formed in a metropolitan hospital under the most favourable circumstances, it follows that those compiled from less certain *data* must be unworthy any serious consideration. The tables of the register-general for instance, made up from returns received from practitioners amidst the bustle of general practice, frequently by inexperienced persons, such as apprentices, and in some cases, as he had been informed, by shop-boys, must surely be of this character.

The results of surgical operations, although apparently more satisfactory, are in point of fact not so. Here there are the same differences of age, sex, constitution, idiosyncrasy, &c. Different operations (such as the flap or circular mode of performing amputation, below the knee or above the ankle,) are frequently confounded together. This had been done in one of Dr Simpson's tables of amputation of the leg. But supposing these points to be carefully attended to, does the skill of the surgeon, the mode of operating, and the kind of after treatment, exert no influence? Yet we find operations by different surgeons, and various experiences, all mingled together to produce one sum total. The most skilful metropolitan surgeon is put on a par with the country practitioner, and the experience of long practice is of no more value than that of the tyro. It is well known that even in one person's practice, he operates differently at different periods of time. Mr Syme had well illustrated this with regard to lithotomy, and told us that since his alteration of the method of its performance, his success had been much greater than formerly. Yet, according to the reasoning of Dr Simpson, all the operations must be added together, and those performed during the inexperience of youth and the senility of advanced life, must, as with Mr Mar-

tainau, be put on the same level with the cases that form the boast of mature age, and the most perfect powers of mind and body. Again, Dr Simpson had compared operations performed during the present year with operations performed many years back, when surgery was by no means so advanced as it is at present. He had also introduced operations performed in different countries, in order to swell the amount. Dr Bennett, however, believed that large numbers, obtained from such incongruous sources, and differing from each other in so many essential points, so far from producing accuracy, could only lead to inextricable confusion.

Dr Bennett was aware that a very different opinion was entertained by statisticians, whose principle was, the greater the number the more certain the results. He (Dr Bennett) contended, that however this rule might apply in cases where the individual data were precise and equal, it could not apply in medicine, where no certainty prevailed regarding the elementary facts. Medical statistics, indeed, were not founded on facts, but on opinions; for, whether an individual laboured under pneumonia, phthisis, or fever, was only the opinion of the practitioner. He had frequently found, as pathologist to the Infirmary, that the lesion entered on the tickets was not only not to be found after death, but that some other disease existed which had never been suspected. But supposing the diagnosis in this respect was always perfect (which no one could maintain to be the case), it did not follow that the cause of death was ascertained as many supposed, for the great majority of lesions were only indirectly connected with the cause of death. On all these points he argued that medicine gave us very imperfect information; but statisticians made calculations, and drew results from them, as if medical science was as exact as the mathematical or chemical sciences.

In this manner Dr Bennett was of opinion that such calculations not only were erroneous in themselves, but that the principle involved in them must lead those who adopted it to the most degrading view of the medical art. Dr Simpson maintained this principle to its full extent, and had illustrated it by the fact, that if a well-balanced penny were tossed up a certain number of times, heads and tails would come down with the like frequency. If medicine was to be cultivated with views of this kind—if, as had been asserted, death took place just as frequently under every kind of treatment—if the practice of the most experienced and judicious was of no more avail than that of the inexperienced or ignorant, what would be the utility of studying medicine at all, or laboriously endeavouring to push forward diagnosis or pathology as a foundation for treatment? Such opinions, if carried out, must necessarily paralyze all efforts at cure, and give rise to a scepticism that would overthrow the very foundation of medical education. He believed that it was to the spread of this false philosophy, and to fallacious conclusions, drawn from the adding together of cases, that the daring attempt had been lately made in this country to show, that the modern quack systems of practice were as beneficial to mankind as the rational system founded on diagnosis and pathology. Hence why every system in turn has been shown statistically to be superior to its rival. He (Dr B.), however, was satisfied that the very excess to which medical statistics were carried by their present supporters, would at no distant date exhibit to every man of common sense the gross fallacy involved in them. Whilst he was willing to allow that there were many points to which the application of statistics was undoubtedly useful to science and the public, he contended that the present state of medicine did not permit them to throw any more light than experience had long enabled us to do, on the frequency of diseases, the causes of death, or the success of treatment. At some future period statistics might be useful in medicine; but that could only be when diagnosis and pathology were more advanced. Now, however, on adding together 2 and 2 cases of any so called disease, whilst statisticians make it appear on paper that the sum total is 4, the truth often is, that the disease named was only present once or twice.

Dr Simpson stated that he would certainly have felt astonished at Dr Bennett's remarks, if he had believed them to be aught else than remarks made for the mere sake of discussion. Dr Bennett had maintained that the difficulties accom-

panying the practical application of statistics, were an argument against the truth and principles of statistics. He might as rationally have maintained that the difficulties accompanying the practical application of logic, were an argument against the truth and principles of logic. In every mode of investigation we have difficulties to overcome, but that is no reason why we should reject all modes of investigation. (Here Dr Bennett stated that his argument was not directed against the principle of statistics in general, but against their application to diseases with our present imperfect knowledge of them.) Dr Bennett had most correctly stated the one great difficulty accompanying the application of statistics to questions coming under the investigation of the physician, viz., the impossibility of making a correct diagnosis of internal diseases in all cases; and hence the danger of calculating upon a case, as one of phthisis, for instance, when it was one of chronic bronchitis or pneumonia. But study these diseases in any other way whatever than statistically, and you have the very same difficulty to overcome. Only statistics enforce you to make the individual observations more full and true and accurate, if possible; and surely that was no objection to them. But this same difficulty in relation to statistics does not hold good to any such extent in regard to the statistical mode of inquiry as applied to surgical diseases and questions, and doubtless surgery was the department in which it was yet destined to work out the greatest changes. For instance, in the class of cases which he had brought before the Society to-night, there were only three matters of observation or diagnosis on which perfect accuracy was required in each individual component observation, in order that perfect accuracy might be obtained in the ultimate generalizations. And he did not think that it required any *great* exertion of diagnostic talent to state the individual facts quite correctly on all of the three required points; for the three amounted to the following—1. Was or was not amputation performed? 2. Where, or at what point was it performed, in the thigh or leg, in the arm or forearm? and 3. After the amputation was performed, did the patient die, or did the patient not die? Most tyros in the profession could be entrusted with such diagnosis; and accurate reports on these three points were all that were required for the diagnostic truth of the inquiry, and the deductions from it. Dr Bennett objected to statistics as being an attempt to apply a piece of arithmetic or mathematics to the elucidation of pathology and practice. On the same ground it was at one time objected to the stethoscope and plessimeter when they were first introduced, that *they* were an attempt to apply a piece of mechanics and acoustics to the elucidation of pathology and practice. They all knew that Dr Bennett was the farthest possible from subscribing to that argument. And it was our bounden duty to use every proper and legitimate means to advance our science and its art, and no doubt statistics was one of the most important among these means. Indeed it was used constantly by those who professed to reject it, and that, too, though they might not know they were using it. Some time ago, Dr Bennett himself had read a paper to the Society, proving statistically that tubercular phthisis must occasionally undergo a spontaneous cure, as he had found pathological evidences of its former cure in a certain per centage of all the patients whose bodies were opened by him at the Royal Infirmary. Again surgeons declared to us as the result of their experience, that death was more frequent after amputation of the thigh for injury, than after the amputation of the thigh for disease. They were thus so far just stating a statistical result which was more or less vague, or more or less true, according to the statistical precision with which they calculated. Trusting to the mere impressions of cases left on their memories, Percy and Dupuytren erroneously declared that amputations for injury were less fatal than amputations for disease. Strict statistical calculations, however, in the hands of Lawrie, Phillips, &c. &c., have, on the other hand, lately proved that amputation of the thigh for traumatic causes, was nearly double in fatality to amputation of the thigh for morbid causes. In conclusion, Dr Simpson begged entirely to doubt that the statistics of the Edinburgh Hospital were

kept with exemplary accuracy. Of late years, they did not, for example, show the number of operations performed, or their nature, or their mortality. He had found it utterly impossible to derive from them either the number of amputations performed during the last few years, their locality, or their results. And yet almost every provincial hospital in the kingdom could at once furnish you with perfect accuracy with all such information.

The PRESIDENT then adjourned the Society until the first Monday in November.

ENEMIES TO SCIENCE AMONG THE NOBLES.

A GREAT sensation has every where been excited by the fact, that men of science in the pursuit of knowledge have been obstructed in their peaceful investigations by certain peers and landed proprietors. The public press has indignantly protested against the right of these noblemen to shut up the highways and byeways, and to depopulate whole districts of the country for the purpose of converting them into wild deer forests. With these, and many other grievous subjects of complaint, however, it is out of our province to speak. It is not as hunters and deer-stalkers we have to do with them. Neither can we dwell on the mortifications and hardships which the tourist in search of pleasure or health has experienced. What we are desirous of alluding to is the circumstance, that certain sporting lords and gentlemen, by obstructing the observations of naturalists, and by discourteously treating learned men in their botanical, geological, and mineralogical investigations as common trespassers on their estates, have earned for themselves the unenviable title of the Enemies of Science.

It is monstrous to suppose that the Braemar mountains, the Grampians, and Glen Tilt, are to be shut out from scientific investigation by certain noble dukes and sporting gentlemen. These districts represent the alpine vegetation of Britain, and it is in them that the botanist finds illustrations of this region. It is there that we meet with such rare plants as—

<i>Luzula arcuata</i> (characteristic of the summits of the highest hills in Bræmar)	<i>Salix lanata</i>
<i>Stellaria cerastoides</i>	— <i>arenaria</i>
<i>Astragalus alpinus</i>	— <i>reticulata</i>
<i>Saxifraga cæspitosa</i> (Benaven, Braemar, only known locality in Britain)	<i>Sonchus alpinus</i> (<i>Mulgedium alpinum</i>).
— <i>rivularis</i>	<i>Oxytropis campestris</i> (Glen Fee, Clova, only known British station)
<i>Carex leporina</i>	<i>Woodsia hyerborea</i>
— <i>vahllei</i>	<i>Erigeron alpinum</i>
— <i>rareflora</i>	<i>Gentiana nivalis</i>
— <i>rupestris</i>	<i>Juncus castaneus</i>
— <i>aquatilis</i>	<i>Alopecurus alpinus</i>
— <i>vaginata</i>	<i>Phleum commutatum</i>
— <i>Grahami</i> (Glen Fee, Clova, only spot in Britain)	<i>Lychnis alpina</i> (on a single hill in the Grampians, only British station)
	Besides a number of other rare species.

If, then, botanists are excluded from these districts, they are cut off from the study of Alpine plants, and what renders the outrage to science more glaring is the fact, that many of these are found nowhere else in Britain, except on the Braemar and Grampian range, and several are confined to single spots on these hills.

Again, it is in these districts that the geologist finds the most perfect illustrations of those theories which have enabled him in modern times to explain

the past revolutions on the surface of the earth, and to trace the changes it has undergone from the most distant ages. It is in Glen Tilt that Hutton, in 1785, discovered the junction of the granite with mica-slate and limestone, whereby he offered the first positive fact in opposition to the Wernerian doctrines. This striking proof of the correctness of those views now admitted by the scientific world, nature has placed in the property of the Duke of Athol, and until lately it has been freely visited by naturalists from every civilized land. But now, that nobleman not only threatens to prosecute every gentleman anxious to confirm his notions by the personal inspection of this natural formation, but offers them direct molestation, on the plea that they are disturbing his deer. More than one foreign professor of eminence, has in consequence been obliged to return to his country, with the belief that the present Dukes of Scotland are even more uncivilized than their barbarous ancestors. It is at least certain, that they are ready to sacrifice for the empty pleasures of the chase, not only the solid benefits which the study of science, and intellectual pursuits confer on mankind, but that courtesy and readiness to oblige, which, more than rank, win for the noble and titled the regards of men.

It is singular to reflect that at a period when government is encouraging scientific expeditions in Australia, Van Diemen's Land, the Arctic Regions, and other districts at the limits of the empire, our Highland glens and mountains in its centre are shut out from investigation. It is curious to know, that whilst the Marquis of Northampton, is, as President of the Royal Society, at the head of science in England, and Prince Albert visits annually the British Association for the Advancement of Science, other noble lords are altogether insensible to the honour and gratification to be derived from favouring the cause of knowledge. We can only regret that nature should have bestowed upon the latter, riches which they have not minds to appreciate; or that fortune should have been so provoking as to render the ignorant and churlish, possessors of objects of interest to the enlightened and polite. We trust, however, that the educated and civilized of their own class, will be struck with the injury done to their order by the titled enemies of science, and that they will in this manner be forced in very shame to concede the privilege its cultivators require of them.

CAVENDISH SOCIETY.

WE are anxious to direct the attention of our readers interested in chemistry, to a society recently instituted in London, on the plan of the Rae and Sydenham Societies. It has named itself after the Honourable Henry Cavendish, famous among British philosophers for the beauty and accuracy of his researches in physical science, and has for its object "the promotion of chemical science, by the translation and publication of works and papers on chemistry, and its application to agriculture, manufactures, mineralogy, medicine, and pharmacy." The subscription is one guinea annually; and the council, trusting to a sufficient number of names being given in, are making arrangements for the early issue of some of their contemplated volumes. Among these we observe, Translations of Gmelin's Organic Chemistry, Kepp's History of Chemistry, G. Rose's Crystallography, &c., all works of the highest value.

Prospectuses of the society lie with our publisher. The general secretary is Robert Warrington, Esq., Apothecaries' Hall, Blackfriars, London. The secretary for Edinburgh is Dr Thomas Anderson, Surgeon Square. From either of these gentlemen further information may be procured, by those of our readers who take an interest in the scheme.

INTERDICTION OF THE NEW REGULATIONS RESPECTING ATTENDANCE AT THE EDINBURGH UNIVERSITY.—In our June number, we noticed that the new regulations of the patrons of the University, whereby one-third of the prescribed classes for graduation might be attained among the extra-academical teachers, had

been determined on, and that it was anticipated they would come into operation next session. We regret to say, however, that the Professors have caused an interdict of Court to be served upon the Town-Council, whereby these regulations are indefinitely suspended. Whilst we consider it very natural that the Professors should endeavour to preserve their privileges, we are still of opinion that they operate very detrimentally to the interests of the medical school. We know, for instance, that several advanced students have been induced to spend the last year of their studies in Paris, rather than be forced to attend the lectures of the Homœopathic Professor, and it must be conceded that regulations having such a truly oppressive influence cannot be productive of good.

THE MUSEUM OF MATERIA MEDICA OF THE ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.—It has been for some years an object of great interest with the Edinburgh College of Physicians to form an extensive Museum of *Materia Medica*. A considerable collection had been made principally under the auspices of Professor Christison, which, from time to time, was enriched by various donations. Still, it became evident that the formation of such a museum as would do honour to the College, would be the labour of many years. It was therefore well disposed, on entering upon its present premises, which are well fitted for the display of a large collection, to purchase the celebrated museum of Martius, which is now effected. All those who know any thing of the University of Erlangen, will remember that this rich collection occupied the central part of the building, and was the chief object of curiosity there. At the annual meeting of naturalists and physicians of Germany in 1840, we examined it with great care, as it was universally allowed to be the richest and most extensive one of its kind ever formed. This is the collection which is now being arranged in a spacious room set apart for that purpose by the College of Physicians in Edinburgh; and when fully displayed, will, with the former collection, constitute the first museum of the kind in this country. When ready for inspection we shall again allude to it, and mention its merits more particularly.

BOOKS RECEIVED.

1. Cholera, Dysentery, and Fever Pathologically and Practically considered; or the nature, causes, connexion and treatment of these diseases in all their forms. By Charles Searle, M.D., M.R.C.S.E., &c. London. Small 8vo. 1847.

2. A Guide to the Use of the Buxton Waters. By W. H. Robertson, M.D., &c. Fourth Edition. London. 12mo. 1847.

3. A Copy "of Reports on Sir William Burnett's Disinfecting Fluid." Ordered by the House of Commons to be printed, 20th July 1847.

4. A Letter to Benjamin Rotch, Esq.

on the Plan and Government of the additional Lunatic Asylum for the County of Middlesex, about to be erected at Colney Hatch. By John Conolly, M.D., &c. London. 8vo. 1847.

5. Seventeenth Annual Report of the Belfast District Asylum for the Insane Poor, for the year ending 31st March 1847. Belfast. 8vo. 1847.

6. The Dublin Dissector, or System of Practical Anatomy. By Robert Harrison, M.D., M.R.S.A., &c. Fifth Edition, with numerous illustrations. 2 vols. Small 8vo. Dublin. 1847.

TO SUBSCRIBERS.

ON account of the quantity of matter which has been for some time accumulating upon us, we have found ourselves under the necessity of enlarging the size of the Journal for the present month.

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No. 17. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Etherization in Surgery*: PART II.—*Proper Mode of Investigating its Effects; Statistical Propositions and Results; &c.*
By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh. [Continued from p. 166 of September No.]

“La possibilité de l'application de la statistique à la médecine, est une vérité tout aussi bien démontrée que la réalité de la circulation.”—*Dezeimeris, Dictionnaire de Médecine*, vol. xxviii. p. 550.

VALUE AND NECESSITY OF THE NUMERICAL METHOD OF INVESTIGATION AS APPLIED TO SURGERY.

THE vast practical importance of the doctrine of statistics, and its power of elucidating, simplifying, and deciding many and various inquiries in surgical and medical science, is now becoming daily more and more acknowledged by the members of the profession. The doctrine itself has been long, not only acknowledged, but acted upon by governments and by the public at large. The political laws and expensive machinery pertaining to the registration of the deaths and diseases of the inhabitants of England, and of other kingdoms of Europe, are founded upon the soundness of the doctrine. In our numerous life assurances and annuity companies, millions of money are unhesitatingly staked upon the truth of it. And the principle upon which the usefulness and stability of the whole doctrine of medical statistics rests, is a very simple one. It amounts to this: Among facts, data, or unities of a variable chance—such as the probabilities of death within a given time, or the probabilities of attacks of parti-

cular diseases within a given time, or the probabilities of averting death in particular diseases by particular methods of treatment, or operation—there is ever a mighty uncertainty as to the results, if we consider only single cases, or a small and limited number of instances; but our results approach more and more to certainty, in proportion as we deduce these results from a greater and more extended number of instances,—from a larger and multiplied series of facts. There is always great uncertainty and instability in regard to the results of single or isolated cases; but a proper aggregation and conjunction of cases affords results which are comparatively certain and stable.

In the present investigation into the effects of etherization upon the mortality attendant on surgical operations, I have followed the statistical method of inquiry. But as the doctrine itself of statistics, as applied to such questions, is still, I fear, very imperfectly understood by the profession in this country, I shall here take the liberty of premising a few observations upon this mode of inquiry.

I have already stated, that the great and leading principle upon which all statistical inquiry is grounded, consists in the fact, that in unities or entities of a doubtful chance, while the result, or event, in *individual* instances, is ever variable and uncertain, the result, or event, when calculated from, or upon *masses* of instances, becomes comparatively certain and invariable. I shall show the truth of this abstract remark, in the form of illustrations, of a few of the fundamental principles or propositions upon which the doctrine of medical statistics is founded, and this more especially with a view to the bearings and important advantages of the statistical or numerical method of inquiry as applied to questions in surgery.

FIRST PROPOSITION.—*The absolute number of deaths from all causes, in a given time, in a given population, is always nearly the same.* The probability of life or death to individuals within the limits of a given period, is proverbially uncertain. Nothing is more uncertain, for instance, than the number of individuals that will die in the currency of a single year in any particular family, street, or village in England and Wales. But nothing could be more certain than, *cæteris paribus*, the number that will die during the currency of a single year in the whole of England and Wales. Estimating, for instance, as we do in *all* modes of reasoning and philosophizing, from the experience of the past what will be the experience of the future under similar circumstances, we may state beforehand as certain, that in 1845 (the results of which have not yet been published) the total number of deaths in England and Wales amounted to about 350,000. For the returns of the Registrar-General for England and Wales have now been collected and published for seven years,—viz., from 1838 to 1844 inclusive, and the total numbers that died during each of these years were fixed and determinate, to the extent shown in the following table:—

No. I.—*Table of Absolute number of Deaths in England and Wales, and of the percentage of deaths among the whole population during Seven successive years, from 1838 to 1844 inclusive.*

Year.	Absolute Number of Deaths.	Percentage of Deaths among the Population.
1838	342,547	2.2 in 100
1839	338,979	2.1 in 100
1840	359,634	2.2 in 100
1841	343,847	2.1 in 100
1842	349,519	2.1 in 100
1843	346,446	2.1 in 100
1844	356,950	2.1 in 100

In reference to the preceding table, we must bear in recollection one point, that in this, as in other statistical inquiries, there is always a range of oscillation, and limits of possible error; but, as Gavarret has well demonstrated, the extent of these oscillations and limits of possible error are themselves easily ascertainable, and capable of being reduced to mathematical calculation and correction.

SECOND PROPOSITION.—*The absolute number of deaths from individual diseases and specific causes in a given time, in a given population, is always nearly the same.*—For if it be true, as shown under the first proposition, that the exact number dying annually in England is nearly the same, it is equally true and demonstrable that the particular causes or forms of disease producing these deaths recur in successive years in the same number and proportion. What is true regarding the whole, is true in regard to its parts. For the purpose of illustrating this secondary fact, I shall take from the Registrar-General's reports nine returns, three referring to medical, three to surgical, and three to obstetric affections and complications; and I shall add one pertaining to the department of medical jurisprudence. Each of them shows the comparative certainty of large numbers. For while, for example, no man could predict who or what number of a small community would die annually of croup, or tetanus, or ovarian dropsy, yet the absolute number dying each year of these and other affections throughout England, when calculated on a large scale, comes annually in all, except epidemic and zymotic diseases, to be nearly the same, as the following table sufficiently demonstrates. In fact, their numbers are, if possible, more determinate than the numbers of the total deaths; because, while the absolute mortality of a kingdom is liable to be varied by variations of a temporary and transient nature in the existing epidemic and endemic influences, &c., those individual diseases and causes of death, the etiology of which is more fixed, are more stable in their results.

No. II.—*Table of Absolute number of Deaths annually in England and Wales from twelve different Diseases or Causes of Death.*

Causes of Death.	No. Dying in 1838.	No. Dying in 1839.	No. Dying in 1840.	No. Dying in 1841.	No. Dying in 1842.
Croup,	4463	4192	4336	4177	4457
Jaundice,	841	800	875	864	952
Apoplexy,	5630	5293	5451	5581	5361
Hernia,	507	474	480	475	529
Tetanus,	129	122	142	118	118
Carbuncle,	35	38	33	28	40
Childbirth,	2811	2915	2989	3007	2687
Malformations,	166	214	211	206	217
Ovarian Dropsy,	45	34	43	44	52
Violent Deaths,	11,727	11,632	11,594	11,100	11,092

The regularity with which the same disease thus destroys, in successive years, nearly the same number of individuals, may appear remarkable to those who have not given attention to the study of medical statistics, and who have, consequently, not marked the fixed and determinate nature of the results which this means of investigation always elicits, when it is enabled to work upon a sufficiently large basis of facts, or a sufficiently large series of data. But this constancy appears, if possible, still more singular, when we turn to such a subject as that included under the last column in the table, viz. "violent deaths." Under this head are included deaths by mechanical injuries, by chemical injuries, by asphyxia, &c., and, if the returns were more specific, it would no doubt be found that the number of violent deaths from *each* separate division of causes was annually nearly the same. Even causes originating in passions of the human mind, and leading to violent death by murder, are, *cæteris paribus*, repeated in nearly the same number in each successive year. The moral man is subject to laws as fixed as the physical man. Some years ago, Quetelet showed from the comparison of the annual number of deaths in Paris, and the annual number of crimes committed throughout France, that the statistics of human crime are as fixed as the statistics of human mortality; that each age paid as uniform and constant a tribute to the jail as it paid to the tomb; that the numbers of any specific crime in successive years was, like the numbers of deaths from any specific disease in successive years, always nearly the same; ay, that the very instruments by which the same crime (as murder) was perpetrated in different years, were always in nearly the same proportion.¹

¹ "If all human actions could be registered," says Quetelet, "it might be supposed that their numbers would vary from year to year as widely as human

THIRD PROPOSITION.—*The absolute number of those that recover, should, cæteris paribus, be as fixed as the number of those that die from individual diseases in a given time, in a given population.*—The preceding table (No. II.) shows how many died of the several affections included under it, during a succession of years. If our statistics were more specific and detailed, we ought to be able to tell also how many recovered each year from attacks of each of these affections, as well as how many died from them; and if we could thus count the number of recoveries as well as the number of deaths, by striking the proportion between them, we would obtain the average mortality of each disease. The deaths, for instance, from croup, amount on an average to 4325 each year. But if at the same time we knew the total average number of cases of croup that occurred every year (say, for the sake of illustration, that they amounted to 13,000 in all), then the mortality of the disease would amount to nearly 1 in 3; or out of every three patients attacked with croup, two would recover and one die.

The Registrar's reports, however, do not furnish us with returns of the recoveries as well as of the deaths in any special disease or complication, with one exception. Under the division of births he gives the number of children born each year, and under the term childbirth, in the division of deaths he gives the number of mothers who perished each year under parturition or its consequences. Being thus furnished not only with the annual total number of deaths that took place from parturition, but also with the annual total number of cases

caprice. But this is not what we in reality observe, at least for that class of actions of which we have succeeded in obtaining a registry. I shall quote but a single example; but it merits the attention of all philosophic minds. In every thing which relates to crimes, the same numbers are reproduced so constantly that it becomes impossible to misapprehend it—even in respect to those crimes which seem perfectly beyond human foresight, such as murders committed in general at the close of quarrels, arising without a motive, and under other circumstances to all appearance fortuitous or accidental. Nevertheless, experience proves that murders are committed annually, not only pretty nearly to the same extent, but even that the instruments employed are in the same proportions."—*Treatise on Man*, p. 6. The following table, abridged from Quetelet, may enforce still more the truth of his observations.

Table of the Annual Total Number of Murders, and Instruments of Murder, in France, collected from the Reports of Criminal Justice, from 1826 to 1831.

Modes of Murder.	In 1826.	In 1827.	In 1828.	In 1829.	In 1830.	In 1831.
Total Number of Murders, . .	241	234	227	231	205	266
By Gun and Pistol,	56	64	60	61	57	88
By Knife,	39	40	34	46	44	34
By Stones,	20	20	21	21	11	9
By Kicks and Blows,	28	12	21	23	17	26
By Cudgel, Cane, &c.,	23	28	31	24	12	21
By Stabs, Cuts, and Bruises, .	35	40	42	45	46	49
By other means,	40	30	18	11	20	39

The difference in 1830 and 1831 from the preceding four years were no doubt owing, in a great degree, to the Revolution of 1830, and its immediate effects.

of parturition that occurred, we have the data in this instance for calculating the proportion of recoveries to the proportion of deaths in childbirth. And the following table presents the results for the only four years of which the full data have been yet published.

No. III.—*Table of Proportion of Deaths in Childbed in England and Wales, from 1839 to 1842.*

Years.	No. of Children born.	No. of Mothers dying in childbed.	Proportion of Maternal deaths in childbed.
1839	492,574	2915	1 in 169
1840	502,303	2989	1 in 168
1841	512,158	3007	1 in 170
1842	517,739	2687	1 in 192 ¹

FOURTH PROPOSITION.—*Statistics enable us to prove that the general mortality, the mortality in particular departments of practice, and the mortality from individual diseases, are capable of being altered by altering the attendant circumstances.*—Under the three preceding propositions, in showing the attendant results to remain from year to year the same, I have supposed the attendant circumstances to remain also the same. But if we change the conditions in which the community or the individuals composing it are placed, we change also the results. The great power which *art* possesses, is the power of altering these attendant conditions and circumstances by improved hygienic measures, improved medical practice, improved modes of operating, &c. And the effects of this alteration might be interminably questioned and doubted, provided we had not the power of proving it by simple statistical evidence. I shall adduce some examples.

Improve in regard to salubrity, &c., the circumstances in which a community is living, and you increase the value of life in that community, or diminish its mortality. Thus, in 1786, the yearly rate of mortality for the whole of England and Wales was 1 in 42; or, in other words, 1 in every 42 of the inhabitants died annually. In 1801, this mortality was found to be 1 in 47. In 1831, it had diminished to 1 in 58; showing a difference of 38 per cent. in the short period of half a century.²

Again, if we wished to prove that our practice, in relation to any

¹ In relation to the etiology and pathology of puerperal fever—the most common cause of death in childbed—it is not uninteresting to remark, that in 1842, when the deaths following parturition were so few, the number of deaths from erysipelas was also much diminished in number. This fact may, I think, be adduced as a reason additional to the many lately urged in proof of an identity in causation and character between erysipelas and puerperal fever.

² Dublin Review, vol. ii. p. 97.

particular department of the profession, was more successful now than formerly, or more successful under one mode of treatment than under another, our only certain form of evidence consists in a similar appeal to statistics. Mere assertions and opinions avail little in such a question. Figures and statistics can alone properly decide and determine it. And here, as elsewhere, they offer a kind of evidence, which is not less remarkable for its intelligibility and simplicity, than for its precision and certainty. I shall again take the case of childbirth as an example. The following table, calculated from the mortality bills of London, and given in a more extended form by Dr Merriman,¹ demonstrates statistically—what could not be satisfactorily proved by any other form of evidence—that the practice of midwifery has become more and more improved in London, and inferentially in the whole kingdom, during the last two hundred years; the ratio of deaths in childbed having gradually become less in number, and that to such a degree, that the proportion of parturient mothers lost, during the last years of the 17th century, were nearly double the number of those lost during the first years of the 19th century:—

No. IV.—*Table of average number of Mothers dying in Childbed in London from 1660 to 1820.*

Years.	Proportion of Mothers lost.
For 20 years ending in 1680 . . .	1 in every 44 delivered.
For 20 years ending in 1700 . . .	1 in " 56 "
For 20 years ending in 1720 . . .	1 in " 69 "
For 20 years ending in 1740 . . .	1 in " 71 "
For 20 years ending in 1760 . . .	1 in " 77 "
For 20 years ending in 1780 . . .	1 in " 82 "
For 20 years ending in 1800 . . .	1 in " 110 "
For 20 years ending in 1820 . . .	1 in " 107 "

Further, supposing we desired to prove, in regard to any one complication or disease, that some particular mode of treatment or of operation was more successful than another, we can resort to no other definite mode of decision than statistics; and no other known mode of investigation could yield the same simple and satisfactory results. In illustration, let me adduce an instance from obstetric surgery.—The normal conjugate diameter of the brim of the pelvis is four inches. Sometimes, however, it is morbidly contracted. When, as occasionally happens, this bony canal is so much diminished in size that its opposite walls are not more than from two and a half to three inches distant from each other, the mutilation and destruction of the child's head by craniotomy was formerly supposed, by British accoucheurs, to be the only proper, or indeed possible, mode of delivery. Latterly, after great and strenuous opposition, a revolution

¹ Synopsis of Difficult Parturition, p. 343.

in practice has taken place in such cases; and, at the present day, the artificial induction of premature labour at the seventh month is the established rule of treatment. The child's life has thus certainly been saved, in many instances, from otherwise inevitable destruction. But is the life of the mother not placed, as has been sometimes alleged, in greater jeopardy by it? Statistical evidence answers the question by showing, that while craniotomy is fatal to the mother in about one in every five cases, the induction of premature labour is not fatal to her life in more than about one in every fifty cases. Nor would it be possible to place such a question beyond the possibility of doubt except by statistics, built upon a proper and sufficient basis of data. Mere impressions and arguments would not solve the problem. Dr Osborne declared, for instance, that craniotomy was rarely fatal; and Madame Lachapelle described the induction of premature labour as rarely safe. Statistics have amply belied both opinions.

FIFTH PROPOSITION.—*Statistics offer a test by which the impressions of unrecorded and limited experience are corrected; and they furnish a mode of investigation capable of resolving many existing practical problems in surgery.*—It is only since statistics began to be applied to surgical investigations, that surgeons themselves seem to have become aware of the excessive rate of mortality accompanying most of their capital operations. As long as the mere impressions of cases upon the memory was depended upon, and the individual cases or facts themselves not noted or counted, the most erroneous opinions prevailed regarding the rate of mortality following upon surgical practice. All conclusions drawn from the memory are, observes Malgaigne, “horribly fallacious (*horriblement infidèles*), and it is,” he adds, “to their employment that we owe the astonishing delusions almost generally professed regarding the real danger or fatality of amputations.”¹

In his *System of Surgery*, Mr Benjamin Bell, one of the most esteemed surgical practitioners and writers of his day, in some general remarks upon amputation, observes, “In the present improved state of the operation, I do not imagine that one death will happen in twenty cases, even including the general run of hospital practice: and in private practice, where due attention can be more certainly bestowed upon the various circumstances of the operation, the proportion of deaths will be much less.”² But in 1844, Mr Inman collected the statistics of 3586 cases of “amputations generally, including secondary, primary, for accident or disease;” most from hospital, but some from private practice, and he found that out of these 3586 cases, there died 1146, or 1 in every 3 $\frac{1}{10}$.³ In a very valuable communication, Mr Fenwick has published a collated table

¹ Archives Générales de Médecine for April 1842, p. 391.

² A System of Surgery, by Benjamin Bell. Vol. vii. p. 254 of 7th edition.

³ Lancet for 5th October 1844, p. 39.

of 4937 amputations. Out of these 4937 cases 1565 died, or the operation was fatal in the proportion of 1 in every $3\frac{1}{5}$ submitted to it.¹ The late statistical investigations of Phillips, Lawrie, Malgaigne, Gendrin, &c., have all fully borne out the same view with regard to the great mortality attendant upon amputations.

Nor are opinions formed from a single accurately observed case, or a very limited number of data, to be depended on as the ultimate probable measure of the value or fatality of an operation. An impression from an individual case often leads us to form a wrong estimate of the average danger or average safety of an operation, while adequate statistics at once show us the truth. Mr Pott² saw amputation at the hip-joint performed in one case, and from that case drew the general deduction, that in this operation the want of success would be uniform. Mr Syme, after performing amputation at the hip-joint in one case, from that case drew the opposite deduction. "I firmly believe³ (says he), that if the operation be done properly, and above all, quickly, its success will be general, if not uniform." In his excellent "System of Surgery," Professor Fergusson of London, depending upon his impression of cases, but not actually counting them, observes in regard to amputation at the hip-joint, "although no reasonable practitioner would ever attempt it except as a last resource, it is somewhat satisfactory to know, that with all the disadvantages under which it has been performed, particularly in military practice, and notwithstanding the fearful shock that must of necessity attend such an extensive mutilation, the success of the operation has probably been such, that 1 patient out of every 3 on whom it has been performed has been saved."⁴ But when we turn from such conclusions to the simple evidence of numerical facts, all of these three opinions are seen to be erroneous; and, in addition, the mind at once obtains a precise and definite idea of the degree of danger attendant on the operation when we simply appeal to statistics and find, as Mr Sands Cox has shown,⁵ that out of 84 cases in which amputation at the hip-joint has now been performed, 26 were successful, and 58 unsuccessful; or, out of every 10 operated upon, 7 died and 3 recovered.

Now the certainty and correctness of the knowledge which we

¹ Monthly Journal of Medical Science for October 1847, p. 238.

² Chirurgical Works. Vol. iii. p. 217, 218.

³ Edinburgh Medical and Surgical Journal. Vol. xxi. p. 27. "Successful case of Amputation at the Hip-joint." The patient died "at the commencement of the eighth week from the operation."

⁴ Practical Surgery. Page 362. The rate of mortality which Mr Fergusson here attributes to amputation at the hip-joint, is in fact the rate of mortality belonging to all the *minor* amputations of the limb, taken as a whole, as has been shown in the preceding paragraph. The actual fatality of amputation of the hip-joint is above 2 in 3, instead of being 1 in 3.

⁵ British and Foreign Medical Review. July 1846, p. 112.

obtain in this and similar instances from merely and simply *counting* up a hundred accurately recorded cases, is infinitely superior to a hundred separate opinions and arguments upon the matter. A hundred writers upon such a subject as the probable degree of fatality accompanying amputation at the hip-joint, would no doubt give us every variety and conflict of opinion on the subject. A hundred cases of it correctly noted and counted, would give us a result not admitting of any variety or conflict of opinion, except it were objected that the numbers on which the calculation was founded were too small for a perfect conclusion; and this objection can always be met by collating additional data in order to extend the basis of our calculation, and thus remove and free it from this chance of possible error.

Upon my own mind, the strongest conviction is impressed, that the numerical or statistical method of inquiry is yet ere long destined to advance and promote surgical science, by revolutionizing some departments of surgery, by rectifying a number of its existing errors, by clearing away many of its doubts and difficulties, and by settling and determining for it definitely, various of those practical questions upon which the opinions of the best operators are constantly and ever changing. In the introductory remarks to his last work on surgery,¹ the late Sir Charles Bell remarked, "Men's opinions go to extremes; they vibrate like the pendulum." But the application of statistics to surgery will betimes impart greater precision, and accuracy, and stability to its opinions; for it forms, I believe, the simple, and, at the same time, the only possible means of deciding numerous doubtful and disputed questions in the practice of it. It has, for example, been much and long debated whether the circular or the flap method of amputation is the safer and the better mode of operating. The most opposite sentiments are still expressed, and the most opposite practices still prevail in reference to this point. Some surgeons and some surgical schools earnestly maintain the preferableness of the one method; and others as strongly uphold the greater safety and greater propriety of the opposite plan. Even the same mind, with every anxiety to arrive at nothing but the truth, may repeatedly change, and at different times hold different opinions upon the matter.² But the question at issue between the flap and circular methods of amputation is principally this;—which

¹ Institutes of Surgery, p. 22.

² In the first observations which Mr Syme published on amputation, (Edinburgh Medical and Surgical Journal, Vol. xxi. p. 31,) he strongly maintained "that the circular mode of amputation is in every point of view bad;" and, writing in 1842, he still held that "amputation of the thigh ought always to be performed by making flaps."—(Principles of Surgery, p. 156). In 1845, Mr Syme believing, from statistical evidence, "that there is something radically wrong in the principle" of amputation of the thigh, both by the flap and circular methods, proposed in their stead amputation of the knee; and thus dividing the thigh-bone through its condyles, instead of through its shaft.—(See Monthly Journal of Medical Science for May 1845, p. 337). In the same

operation least endangers the patient's life? Now this question is one which could be satisfactorily settled by statistical investigation, and no doubt will ultimately be so. Probably one or two thousand amputations of the limbs are performed every year by the hospital surgeons of the United Kingdom. If our object were to ascertain whether amputation of the thigh by the flap or circular methods were the safer as regarded the *life* of the patient, and our hospital surgeons were only to note carefully and collect the results of this operation for a year or two, so that we should have the statistical returns of both operations and their results upon a sufficient number of unselected cases, we would thus become furnished with data, the mere counting up of which would show us (infinitely better than any argument) whether the two modes of amputation differed at all in their relative degree of fatality; and if they did differ, which was the most dangerous of the two; and what was the degree of the comparative excess of danger of the one over the other. By the same form of statistical inquiry upon the same or other cases, and by analysing or decomposing each separate case into as many parts as it contained distinct objects, we could further ascertain and determine all the various minor points, such as—which operation required the shortest period of convalescence—which ultimately secured the best form of stump, &c. &c.

And here I would beg to add one observation relative to the probable future importance and bearing of statistics upon surgery. It will be found that, in most of the past literature of their profession, surgeons have almost invariably contented themselves with recording their own deductions from their own cases, without recording the cases themselves. They have left us their inferences, but have not left us the grounds and bases of these inferences. They have generally given us, not their individual cases or individual facts, but the opinions which they themselves thought fit to draw from these facts.¹ The result has been, that, in numerous instances, inferences of the most erroneous and contradictory kind have been drawn, in consequence merely of the elementary facts observed and generalized upon, being far too few for the establishment of a correct deduction. I have already offered an instance of this in the two opposite opinions expressed by Mr Pott and Mr Syme regarding amputation at the hip-joint, from single cases observed by each, as contrasted with the actual and ascertained degree of danger connected with that operation. A sufficient series of individual facts, collected from the practices of several different surgeons, may thus point out a deduction

Journal for November 1846, (p. 225,) he does "not persist in advocating amputation at the knee," but avows himself now satisfied that the old circular method of amputation may be "employed at the lower third of the thigh safely and advantageously," "and should be preferred to the flap operation at a higher part of the limb, when the circumstances afford room for choice."

¹ In the past records of midwifery, on the other hand, we have all their important *individual* facts and cases left on record for us, in the works of Mauriceau, Portal, Giffard, Smellie, &c. &c.

quite at variance with the so-called experience and opinion of the individual authors themselves. Few surgeons allow that strangulated hernia, when properly operated upon, is very fatal in its results. "The operation," says Mr Pott, "if applied to in time, very seldom fails; so seldom, that I believe I might venture to say, *not* 1 in 50 dies of it if timely and judiciously executed."¹ But, out of 77 cases recorded in Sir Astley Cooper's work on hernia, 36 died; out of 183 operations for this disease, collected by Malgaigne, 114 proved fatal; out of 545 collected by Dr Inman, 260 died; or 1 in every 2 perished instead of 1 in 50. Again, amputation of the thigh is fatal in nearly the same proportion; or about 1 in every 2 or 3 who are subjected to it dies. Out of 987 cases of this operation collected by Mr Phillips,² 435 of the patients, or 1 in every 2 $\frac{2}{10}$ perished. Yet, speaking of the degree of danger accompanying amputation of the thigh, as apparently inferred from his own observations upon the point, Mr Ormerod, in a late work on surgery, observes that, "considering the severity of the operation, and extent of the injury done to an individual by the removal of a limb above the knee, the success of the amputation there is very great; the patients are often placed under bad circumstances for operation, and their health very much reduced, yet death from failure, after the removal of a limb for disease, is *very rare* indeed."³ I repeat, that if, instead of dealing in vague and valueless generalizations and opinions of this stamp, surgical authors would only carefully note and record all their individual operations and results, with a view that betimes a sufficient collection of data might be thus gradually gathered together, in order to settle particular questions of surgical science or practice, then their collated facts would, in determining such questions, be indescribably more valuable than their individual opinions. For the facts and testimony of surgery would thus become *cumulative*, and increasingly conclusive upon any points on the investigation of which it was deemed proper to direct the power of its evidence, instead of being lost and frittered away, as at present, on the formation of a host of isolated opinions, which are too often not less perplexing from their contrariety than from the confidence and dogmatism with which they are severally advanced. It is interesting to reflect how much, in all of these respects, might soon be accomplished by proper and systematical annual reports from the great public surgical hospitals throughout the country; and at the same time it is distressing to consider what masses of valuable information are yearly lost from the mere want of such reports.

SIXTH PROPOSITION.—*Statistical Evidence alone enables us to ascertain correctly the effects of various minor conditions upon the Fatality of Operations,—such as the influence of the age, sex, &c., of the patient; the special success of different operators, &c. The*

¹ Chirurgical Works, Vol. ii. p. 180.

² Medical Gazette for 1844. P. 805.

³ Clinical Collections and Observations in Surgery, p. 135. London: 1846.

results of surgical operations are, like the results of diseases, varied by age, sex, constitution, idiosyncrasy, &c. On the influence of these, and other minor points, some surgeons may have been led to form and express opinions more or less correct; but it is only by employing the numerical or statistical method of examination, that a perfect degree of accuracy of judgment can be possibly attained on such matters. Without statistics, all opinions on these points would have remained doubtful and undetermined; by statistics, their influence can be at once discovered and measured, and that, too, by a kind of evidence which is at once simple and convincing. I shall adduce one or two points as an example of the whole.

Let us take the influence of *age* upon the results of the operation of lithotomy. Various late authors have published the ages of their patients, and reports, which might be reduced to show the influence of age upon this operation. I shall content myself with tabulating, for this purpose, the earliest of the kind ever published; viz., those of Cheselden. He has left records of the ages and results of lithotomy, in 213 cases operated upon by him at St Thomas' Hospital.¹ Out of these, only 20 patients died, or the small number of 1 in $10\frac{1}{2}$. But I shall throw all the 213 cases into a tabular form; and it will be at once seen, from this view of Cheselden's recorded data, that the danger of lithotomy increases in a ratio progressive with the age of the patient.

No. V.—*Table showing the influence of the Age of the Patient upon the Mortality of Lithotomy.*

Ages of the Patients.	Number of Cases.	Number of Deaths.	Ratio of Mortality.	Per Centage of Deaths.
Under 10 years, .	105	3	1 in 35	3 in 100
From 11 to 20 years,	62	4	1 in $15\frac{1}{2}$	6 in 100
From 21 to 40 years,	22	5	1 in $4\frac{2}{5}$	22 in 100
From 41 to 80 years,	24	8	1 in 3	33 in 100

Let us take another illustration from lithotomy of the capability of statistics, proving one more of these minor points, such as are alluded to in the general proposition. Without statistics, it would be difficult or impossible to demonstrate the influence of the mere *size* of the stone extracted upon the results of the operation of its extraction. But by statistics it can be readily proved that the mor-

¹ Cheselden's Anatomy, p. 332.

From the admirable researches of Mr Edmonds and Mr Farre, we know that the mortality of disease *in general*, and the mortality of *individual* diseases (as small-pox, &c. &c.), increases from puberty upwards, in a regular geometrical progression, and that the rate of increase is about 3 per cent. every year, or more nearly 34 per cent. every 10 years. I believe, that an adequate collection of data will very probably show that this same "constant" mortality regulates the degree of liability to death in lithotomy, amputation, and other surgical operations.

tality of lithotomy rises higher and higher in proportion as the stone increases in size; and hence, in all probability, in proportion as the operation increases in severity and difficulty. The following table, calculated from the Norwich data furnished by Mr Crosse, in his valuable work,¹ affords the required numerical evidence for this generalization.

No. VI.—*Table of the mortality of Lithotomy, calculated according to the different Weights of the Stone extracted.*

Weight of Stone.	Number of Cases.	Number of Deaths.	Ratio of Mortality.	Per centage of Mortality.
2 oz. and under	648	65	1 in $9\frac{63}{65}$	10 in 100
From 2 to 4 oz.	46	23	1 in 2	50 in 100
From 4 to 7 oz.	9	5	1 in $1\frac{4}{5}$	55 in 100

Statistics in surgery have been objected to on the ground, that in combining cases, in order to arrive at a general result, we do not take cognizance of the superiority of the practices of individual operators. “We find (argues Dr Bennett) operations by different surgeons, and various experiences, all mingled together to produce one sum total.² The most skilful metropolitan surgeon is put on a par with the country practitioner, and the experience of long practice is of no more value than that of the tyro. It is well known that, even in one person’s practice, he operates differently at different periods of time. Mr Syme had well illustrated this with regard to lithotomy, and told us that, since his alteration of the method of its performance, his success had been much greater than formerly. Yet, according to the reasoning of Dr Simpson, all the operations must be added together, and those performed during the inexperience of youth and the senility of advanced life, must, as with Mr Martineau, be put on the same level with the cases that form the boast of mature age, and the most perfect powers of mind and body.”³

Now, in this as in other points, I believe that the statistical method of inquiry forms the means, and the *only* means, of enabling us to prove the very items which it is alleged that statistics lose sight of and conceal. Take, for instance, the identical examples adduced. The present *general* average mortality of lithotomy, as performed by *all* operators on subjects of all ages, is, according to Dr Willis, about 1 in 8.⁴ Out of 5900 cases collected by Mr Inman, 765 patients

¹ Crosse.

² Of course this is necessary when we wish to ascertain the *general* average success of an operation in the hands of *all* surgeons, and not its *special* average success in the hands of any *individual* operator or operators.

³ Monthly Journal of Medical Science, October 1847. P. 307.

⁴ “The average mortality from lithotomy, on all hands, appears at present to be about one in eight.”—Dr Willis’ Urinary Diseases. 1838. P. 347.

died, or 1 in $7\frac{3}{4}$. Out of 14 cases operated upon by Mr Syme, and recorded in his surgical reports in the Edinburgh Medical and Surgical Journal (vol. xxxiii. to vol. xxxix.), 5 died, or 1 in $2\frac{4}{5}$. Since adopting his present plan of lithotomy, however, he had performed 17 operations in the hospital, of which 2 only have proved fatal, or 1 in $8\frac{1}{2}$. Now, this difference could not be educed or stated with accuracy in any other way than by figures, or by the statistical method; for by it alone can we determine the *special* averages of different operators, or of the same operator at different times. But "take care (observed Sir Astley Cooper) how you draw any deduction from particular cases. I and many others have for a length of time met with extraordinary success in operating for the stone, when 4 or 5 unsuccessful cases in succession have come, which have generally brought down the result to the amount I mentioned, viz., that 2 in 15 die."¹ Mr Martineau's practice afforded a curious illustration of the necessity of this caution.

In the 11th volume of the "Medico-Chirurgical Transactions of London," Mr Martineau published an account of 74 cases, in which he had performed the operation of lithotomy in the Norwich Hospital from the year 1804 to 1840.² Only 2 of these 74 died, or 1 in 37. We learn further, however, from a paper of Dr Yelloly,³ that Mr Martineau operated in the same hospital on 73 additional cases, (147 in all). Out of these 73 additional cases, 15 died, or 1 in $4\frac{9}{10}$. And I repeat, that it is statistics only which could properly and fully prove to us this great *special* difference in the success of Mr Martineau's practice at different periods. At the same time, however, the same case proves to us further, that if we wished to obtain not this *special* average of practice at a *selected* time, but the *general* average of all his practice at all times, it would amount to nearly the general average of most other operators. For out of his whole 147 cases, 17 died, or 1 in 8, which we have seen to be nearly the common degree of success in lithotomy, according to the investigations of Drs Willis and Inman. The special average success of some operators has been greater than this. We have already seen that Cheselden, out of 216 recorded hospital cases, only lost 20, or 1 in $10\frac{1}{3}$. The special average success of other operators has been less. Out of 356 Parisian cases collected by Dupuytren, 61 died, or 1 in 6. Such differences, I repeat, could never be proved and substantiated, unless by statistics.

SEVENTH PROPOSITION.—*Statistics afford us in general the only true and ultimate "measure of value" of any proposed alternative oper-*

¹ Lectures on Surgery. P. 321.

² He records 84 cases in all, with 2 deaths; but 10 of these 84 cases had occurred in private practice.

³ Philosophical Transactions for 1829, p. 63. "The whole number of Dr Rigby's operations [in the Norwich Hospital] was 106, with 15 deaths; and of Mr Martineau's 147, with 17 deaths."

ation, or of any new practice in surgery. Every well-informed writer has naturally and almost instinctively recourse to this form of proof, when originating a new, or reviving a neglected operation. For example, in his able treatise on “Excision of Diseased Joints,” Mr Syme, in recalling the attention of surgeons to the operation of Park and Moreau, and in showing the advantages and safety of excision of the elbow joint, as compared with the alternative operation of amputation of the arm, most properly uses the following statistical argument : “ I have,” he remarks, “ cut out 14 elbow joints, and the operation has been performed in Edinburgh three times by other practitioners ; of all these 17 cases, only 2 have terminated fatally ; and in one of them the patient would, I believe, have died from any operation whatever, while, in the other, the disease was found so extensive as to render the excision almost impracticable. I believe the result of 17 amputations in similarly unfavourable constitutions would not be so satisfactory.”¹

Anatomical and pathological or other considerations may suggest to us the propriety or impropriety of any newly proposed condition or mode of operating ; but an appeal to statistics is the only means of ultimately and definitely deciding upon its merits or demerits. For instance, surgeons were long afraid to place a ligature upon the carotid artery, fearing the difficulties of the operation, and the probabilities of danger to, and derangements in, the cerebral circulation. Statistics, however, show that this reasoning was so far unsound. Ligature of the carotid has now been performed above two hundred times.² Out of that number it has proved fatal in the proportion of about 1 in every 4 patients operated on ; and consequently it has become an established operation in surgery,—many capital operations being more mortal than this.

After seeing the comparative safety with which ligature of the carotid and other large arteries was performed, surgeons reasoning

¹ Treatise on the Excision of Diseased Joints, p. 26.
² I have collected the following table from an elaborate essay of Mr Norris, on Ligature of the Carotid, &c., published in the 27th Number of the American Journal of the Medical Sciences.

Table of the Statistical Results of 203 Cases of Ligature of the Carotid Artery.

Cause for Operation.	No. of Cases.	No. of Deaths.
Aneurisms	38	16
Wounds, &c.	30	15
Extirpation of Tumours	18	6
Cerebral Affections	6	0
Erectile and other Tumours, &c.	42	13
Brasdor's Operation	15	4
Total.....	203	54

on these results, believed it would be justifiable to tie the arteria innominata. The results, however, have belied the *à priori* reasoning. Ligature of the arteria innominata has now been performed, according to Mr Norris, in eleven cases. All the eleven patients died.¹ In three other cases, occurring in the practice of Post, Key, and Porter, the operation was commenced, but not completed. Two of these three patients died. In the third (Mr Porter's) the idea of tying the artery was abandoned after it was exposed; the wound was closed up, and the patient recovered. And, doubtlessly, these statistical results will, ere long, compel surgeons to acknowledge this operation to be one which it is unjustifiable in them to practise.

Every newly-proposed practice in surgery thus offers, as it were, a new problem for statistical solution. In the instance of etherization a small number of cases might be sufficient to satisfy any unprejudiced mind, that the etherized state was one in which the patient was saved from the pain of the surgeon's knife; but a small number of cases could not prove—any more than abstract reasoning could prove—whether etherization were, or were not, a safe practice as regarded the probabilities of the ultimate recovery of the patient after severe operations; whether, that is to say, it increased, diminished, or altered in any respect these probabilities. And hence the origin of the present communication. But before attempting to show the solution of this problem by statistics, let me premise in another proposition an answer to the chief objections which have been urged against the method of inquiry that I have adopted.

EIGHTH PROPOSITION.—*The objections of late years offered against the application of statistics to practical inquiries in surgery and medicine, seem altogether founded upon a misconception of the objects and principles of statistical investigation.*—The application of the statistical or numerical method of inquiry to the solution and determination of questions in medical and surgical science, is of comparatively late date.² Like most other innovations, its introduction has been more or less strongly opposed;³ and the principal objections which have from time to time been urged against the employment of it, have been the following:—

¹ The operations in these eleven cases were performed by Mott, Graefe, Hall, Dupuytren, Norman, Bland, Lizars, Hutin, Arendt, Liston, and Kuhl.

² See some historical notes regarding it in a paper by Mr Marshall, in the Edinburgh Medical and Surgical Journal, No. 116.

³ Our semi-civilized brethren of China, with their fixed hatred of all improvements and innovations, seem to have a particular dislike to statistics, and upon grounds amusingly similar to those of some members of the medical profession in Europe. "Moreover (says Mr Fortune), they [the Chinese] cannot appreciate statistical inquiries; they always fancy we have some secret motive for making them; or that the subject cannot be of the slightest importance either to ourselves or others."—*Three Years' Wanderings in China*, p. 3.

1st Objection:—The numerical or statistical method consists of a calculation of probabilities. There is no doubt whatever of the truth of this allegation. But if it formed a valid objection against the application of statistics to medicine and surgery, it would form equally a valid objection against almost all other modes by which the human mind struggles to acquire increased knowledge, either in medicine or in any other department whatever of science and art. For, as the great French mathematician Laplace observes,—and I could not quote a greater and higher authority on such a point,—“To speak strictly, almost all our knowledge is but probable; and among the small number of things which we can know with certitude, in the mathematical sciences themselves, the *means* to arrive at truth are founded upon probabilities; so that the *entire* system of human knowledge is subjected to the theory of probabilities.”¹

2d Objection:—The numerical method calculates together as similar, facts which are not sufficiently similar to be a basis of calculation of probabilities. M. Double and others who have propounded this objection, have affirmed, that no two cases in medicine or surgery are entirely or exactly the same, and hence that they cannot be counted together as the same. But if this strange and illogical averment were true to the extent to which its supporters maintain, and if every single case to which the physician was called, and every single operation which the surgeon performed, were an individuality and unity so dissimilar from all other previous cases of disease or operation which he had witnessed, as to be incapable of being grouped or classed in any way with them,—then we could not possibly have any general facts, principles, or rules to guide us in the practical exercise of our profession. And, if medicine and surgery had no such general laws or principles, there would be necessarily an end to their existence, either as sciences or arts. Grant, however, that there are specific diseased states in medicine, and specific operations in surgery, with some general facts or rules applicable and peculiar to each disease, and each operation; then, it is further evident, that each of these general facts must have been originally founded upon a basis or deduction of particular facts,—that the fundamental particular facts must be always *more* or *fewer*—consequently capable of being counted,—and, consequently, coming within the range of the numerical method of inquiry.²

But it has been further objected, allowing that individual cases of disease may be arranged into groups or species, still the individual

¹ Essai Philosophique sur les Probabilités, p. 1, &c.

² If the reasoning of M. Double were admitted, “it would,” as Gavarret properly observes, “altogether strike down medicine from the position which it ought to occupy in the temple of human knowledge. What language,” he adds, “can a physician address to his pupils, who will not see, any where, but individualities? On what ground can he recommend them such or such treatment for their patients, since they ought never to meet, in their practice, any thing comparable with what their master has seen? According to this inadmissible

cases composing these groups are often so inaccurately *observed* as not to form a sufficiently true basis for statistical comparison and inquiry. This objection, however, applies to all other modes of medical investigation as well as to the numerical. There is precisely this very same difficulty to overcome, in whatever way or by whatever method we attempt to study and generalize upon diseases. There is this difference, however—the statistical method compels and exacts *more* care, and caution, and correctness in our study, and in our records of cases than other plan of generalizing; and, certainly, *this* forms an argument in favour of the adoption of statistics, rather than an argument in favour of the rejection of them. For it is an acknowledged truth, in medical as in all the other sciences, that the greatest attainable degree of accuracy in our fundamental or elementary facts is necessary, that we may reach the greatest attainable degree of accuracy, and consequently of utility, in the general practical conclusions or laws which we venture to deduce from these facts. From time to time we are obliged, in *every* known form of medical reasoning and generalization, to revise our fundamental facts, and change or modify our conclusions as our knowledge of pathology, diagnosis, &c., increases. The same holds true of the numerical method. And at present, the principal obstacle against applying statistics, more fully than has been done, to some departments of the physician's study, confessedly consists in our occasional inability to make a perfect and undoubted diagnosis of some *internal* diseases, and hence in the liability of our comparing and calculating together cases that are not specifically similar. In statistics, however, as applied to surgery and surgical questions, the same obstacle does not meet us, at least in the same degree. Supposing we wish, for instance, as will subsequently be my object in the present investigation, to calculate what proportion of persons dies under particular amputations,—we only require perfect accuracy on three facts, each and all of which could certainly be noted, and, by a little care, noted with perfect accuracy, and without much, if indeed any, chance of error. For they amount to these points in each case, viz.: 1. Was amputation performed? 2. In what part of the limb was it performed? and 3. After its performance, Did the patient live, or did he die? In this and many other points of surgery, to which the numerical method of inquiry is capable of being directed, all the necessary elementary matter could assuredly, with any common attention and accuracy, be readily collected without much probability or possibility of error. Of course, it is unnecessary to add that in this, as in all other modes

hypothesis, medical experience would be a word without meaning; the student who has never yet seen a patient, would necessarily know as much as the most perfect physician. For if the career of the latter, and his predecessors, is consumed in the sterile observation of a succession of therapeutic individualities, the healing art cannot but be composed of a series of isolated attempts, without a common tie, and from which it would be impossible to draw any general conclusion, or any precept for the future.”—*Principes Généraux de Statistique*, p. 42.

of philosophizing, our observations and deductions must be pursued with stern and strict honesty, and with a view to the attainment of truth, and truth only; and that, for this purpose, *all* the pertaining individual facts or cases must be always given, and always counted. There must be no omission; no concealment; no selection of any kind.

3d Objection:—The statistical method of inquiry is different from and opposed to the inductive method.—No opinion could be more erroneous. The numerical method is assuredly not opposed to that strict observation of individual facts, and that strict generalization of them, which constitute the double basis and essence of the inductive method; but the very reverse. 1. It demands in the same way the strict observation of individual facts; but it demands that the observation and record of them be made, if possible, with still greater care and accuracy than heretofore. 2. It educes in the same way the general principles or laws of practice from the comparison and analysis of these observed facts; but it enforces more rigorous accuracy than heretofore in the deduction of these principles or laws, in proportion as figures are more certain than memory, and actual enumerations more certain than general impressions. Its required mode of observation and mode of generalization are the same as in the common inductive method, only more rigid and hence more rigorously correct. Its object is not to supplant but to supplement our former methods of inquiry,—not to make them useless, but to make them more useful by making them more accurate. It is an instrument which enables us to draw our deductions, not only with greater simplicity, but also with greater truth and precision. “No man (says Bacon), be he ever so cunning or practised, can make a straight line or perfect circle by steadiness of hand, but this may be easily done by help of a rule or compass.”¹ And those who maintain that the numerical method is different from and opposed to experience and induction, might as rationally argue, that when we try to ascertain the general fact of the rapidity of a patient's pulse, we employ two different methods when we attempt to attain the desired information *without*, and *with* the aid of a stop-watch. We here practise two methods that are not different or opposed to each other. They are logically the same in all respects. But the one method is relatively more accurate, precise, and determinate than the other. And in this as in other applications of the numerical or statistical method, the language we employ becomes at the same time simple and decisive. For if we say, for instance, that the beats amount to 120 a minute, our language is far more clear and simple than if we mentioned that the pulse was “*quick*,” or “*rapid*,” or “*frequent*,” &c. We state a definite and intelligible fact, instead of using some comparatively indefinite and uncertain term, which the very temperament of every speaker and hearer may interpret differ-

¹ See Bacon's *Advancement of Learning*, in Montague's edition of his Works, vol. ii. p. 182.

ently. The great aim and object of the statistical or numerical method of inquiry, as applied to surgery and medicine, is the determination of their general facts, or general laws, with the utmost attainable degree of accuracy; but, in truth, figures are not only the strictest and most correct way of *educing* their general facts,—they form also the shortest and most correct way of *stating* or *expressing* them after they are educed.

ARTICLE II.—*Report of the Surgical Operations performed at the Newcastle-upon-Tyne Infirmary, during a period of seventeen years and a half.* By SAMUEL FENWICK, M.D., Lecturer on Pathological Anatomy at the Newcastle-upon-Tyne School of Medicine and Surgery.

(Continued from Page 254.)

3.—*The success of an Amputation differs according to the nature of the accident, or the disease, for which it is performed.*

It has been long observed, that a considerable difference exists in the results of amputations performed for different causes, even when the operation takes place at the same part. Some have imagined that cases of traumatic amputation are more successful, and from that have deduced the maxim, that the more robust the health of the patient, the more favourable will be the issue of the operation; whilst others again, believing the opposite to be the fact, have taught, that an individual who for some time has been suffering from a disease of the limb, will have a greater chance of recovery than another who has undergone the mutilation for an accident.

It is, however, only within the last few years that a sufficient number of hospital records have been published to enable us to settle this difference of opinion satisfactorily; and, as many still argue upon the former assumption, I have attempted to show, in the next tables, the results of experience on each side of the question.

Table XI. is formed from the registers of the Newcastle Infirmary; the other two are collected from various reports.

TABLE XI.

A view of the results of Amputations performed for accidents and diseases at the Newcastle Infirmary.

	Traumatic Amputations.						Pathological Amputations.					
	No.	Dead.	Aver. Mort.	Cure.	O. P.	Time of Death.	No.	Dead.	Aver. Mort.	Time of Cure.	O. P.	Time of Death.
Diseases and injuries of thigh and knee,.....	8	5	1·6	70·33	...	6·	50	9	5·55	49·06	67·8	15·77
Diseases and injuries of leg and ankle	38	14	2·71	60·33	73·4	13·5	74	15	4·93	53·12	43·75	20·13
Amputation of shoulder joint,.....	5	2	2·5	85·66	...	1·5
Diseases and injuries of arm and elbow,.....	24	4	6·	53·33	93·	25·75	11	1	11·	41·88	...	16·
Diseases and injuries of fore-arm and wrist,.....	6	1	6·	42·4	...	8·	9	3	3·	42·66	...	5·66
Total,.....	81	26	3·11	56·82	79·	12·88	144	28	5·14	50·	53·	23·85

TABLE XII.

Shows the Mortality following Amputations of the lower extremity, performed for diseases and accidents, in different Hospitals.

	THIGH.									LEG.								
	Pathological.						Traumatic.			Pathological.						Traumatic.		
	No.	Dead	Aver. Mort.	No.	Dead	Aver. Mort.	No.	Dead	Aver. Mort.	No.	Dead	Aver. Mort.	No.	Dead	Aver. Mort.	No.	Dead	Aver. Mort.
Liverpool Northern Hospital,	19	1	19'	10	3	3.33	13	3	4.33	27	6	4.5						
Glasgow Infirmary (Lawrie),	92	19	4.84	36	27	1.33	35	12	2.91	27	18	1.5						
Chester Infirmary,	9	2	4.5	4	3	1.33	2	4	4	1.						
Massachusetts and Pennsylvania Hosptls., ..	28	5	5.6	22	10	2.2	20	3	6.66	29	9	3.22						
Hotel Dieu (Roux),	12	5	2.4	4	1.	...	9	2	4.5	3	3	1.						
M. Malgaigne,	153	92	1.66	46	34	1.35	113	55	2.05	79	50	1.58						
Mr Alcock,	43	27	1.59	11	4	2.75						
Lyons Military Hospital,	4	3	1.33	5	3	1.66						
Paris do. do.,	5	1	5.	5	3	1.66						
Royal Berks Hospital,	1	3						
Total,	313	124	2.52	174	112	1.55	193	75	2.57	193	100	1.93						

TABLE XIII.

Shows the Mortality following Amputations of the upper extremity, performed for diseases and accidents, in different Hospitals.

	SHOULDER JOINT.						ARM.						FORE ARM.					
	Pathological.			Traumatic.			Pathological.			Traumatic.			Pathological.			Traumatic.		
	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.
Liverp. N. Hosp.,	1	1	1	1.	7	1	7.	10	3	3.33	7
Glasg. Infirmary,	2	1	2.	4	2	2.	17	3	5.66	36	18	2.	4	18
Royal Berks Hosp.	1	2	1	3	4
Chester Infirm.,	2
Massachusetts and Pennsylv. Hosp.	1	1	1	1.	4	11	2	5.5	5	16	1	16.
Hotel Dieu,	1	2	2	1.	2	2
M. Malgaigne,	7	4	1.75	7	7	1.	61	24	2.54	30	17	1.76	17	5	3.4	11	3	3.66
Mr Alcock,	10	2	5.	38	19	2.	9	4	2.25
Lyons Milit. Hosp.	2	1	2.	7	6	1.16	1
Paris Milit. Hosp.,	2	1	2.	4	2	2.
Total,	12	5	2.8	29	15	1.93	91	28	3.25	133	69	1.92	28	5	5.6	68	8	8.5

Unless we altogether refuse our belief in facts, no doubt can remain upon this subject—no case can be more completely proved; with the exception of the fore arm, we find the results of the pathological amputations every where more favourable than those of the traumatic.¹ In one hospital only 1 death occurred in 19, after removal of the thigh for disease; whilst for accidents the mortality was as high as 1 in 3.33: in another, after amputation of the leg, 1 out of 4.5 perished when it was performed for pathological causes, and out of 3 amputations required for diseases all died. Again, if

¹ Although, perhaps, not strictly correct, yet, in accordance with the phraseology of other writers, I have used the terms pathological and traumatic amputations, as designating the amputations performed,—in the former case for diseases, and in the latter for accidents.

we examine the average success of all united, the same difference is apparent ; thus, in amputations of the thigh for disease, 1 dies in 2·52, and after those of the leg 1 in 2·57 ; whilst 1 dies in 1·55 in the former operation, and 1 in 1·93 in the latter when required for accidents.

But a still more remarkable circumstance appears, viz. that, in calculating the chances for a patient, it is necessary rather to consider the cause for which the operation is performed, than the limb which is removed. Thus, although we have before proved that a vast difference exists between the results of amputations of the thigh and those of the removal of an arm, yet here we discover the mortality to be less in the former when required by disease, than in the latter when undertaken for accidents : in the one case we have a mortality of 1 in 2·52 ; in the other, 1 perishes out of 1·92. It is difficult of explanation why this rule is reversed in the fore arm, the mortality, both at the Newcastle Infirmary and at Paris, having been greatest in the pathological amputations : in the former hospital, 1 out of 6 died after the amputation performed for injuries, and 1 in 3 when the operation was performed for disease ; and in the hospitals of Paris, Malgaigne gives the mortality for pathological amputations performed at this part at 1 in 3·4, whilst in the traumatic class it was 1 in 3·66.

In order to throw some light upon the causes from which the difference of mortality between pathological and traumatic amputations arises, the following Table has been added.

TABLE XIV.

A comparison of the Mortality observed in the Newcastle Infirmary, at different periods, after Amputations performed for accidents and diseases.

NEWCASTLE INFIRMARY.

	Within 4 Days.	4 to 7 Days	2 Wks.	3 Wks.	4 Wks.	5 Wks.	6 Wks.	7 Wks.	9 Wks.
Injuries of thigh,	2·66	...	5·	4·
" leg,	9·5	...	8·5	7·5	26·	25·
" arm,	8·	21·
Amputation of shoulder joint,	2·5
Diseases of thigh and knee,	25·	48·	23·5	...	15·	42·
" leg and foot,	74·	36·5	14·2	33·	64·	63·	62·	61·	60·
" arm and elbow,	11·
" fore arm and wrist,	9·	8·	7·

During the first four days, or, in other words, during the period of shock, the amount of death is ten times more in the thigh amputations, and eight times more in the leg amputations after an accident, than when these operations are performed for disease, and this is borne out by the statements contained in Table X. In that Table, whilst none had died of 74 from shock, exhaustion, or delirium, in the pathological amputations, 12 had perished out of 106 who had submitted to the operation for accidents, or 1 in every 8·83. Again, with respect to gangrene of the stump, which we before remarked as depending on shock, and which, we said, had usually its fatal termina-

tion during the first four days, although the average mortality had been 1 in 17·66 after amputations for injuries, no death had occurred from it amongst those previously affected with long-standing complaints.

Secondary hemorrhage, on the contrary, is most fatal after amputation for diseases, few persons undergoing an amputation for an accident being cut off by it. It will be observed, that the cases which in Table X. are stated to have died from it after traumatic amputation, were both in the class of the secondary operations, so that no person suffering a primary amputation had sunk from its effects. It is probable that this arises from the greater debility of the patients not permitting the system to rally after the loss of blood; for the occurrence of secondary hemorrhage is not much more frequent amongst amputations for diseases than for accidents. Thus, in Mr Alcock's returns, 1 case out of every 6·3 was affected with it after primary amputation; whereas in the secondary class, which may be said to be composed of persons worn down by disease, 1 out of 4·72 was attacked: but of the former class, whilst only 1 in 57 died, in the latter 1 out of 13 perished; and in four other cases of secondary amputations, the depression produced by it assisted in destroying the patient; so that, whilst in the primary amputations an unfavourable effect was produced in only 1 in 57 cases, in the secondary, either death or a very dangerous state of depression was observed in 1 in every 6·5.

Erysipelas and visceral inflammations, according to Table X., are by far more frequent in amputations for accidents than in those performed for diseases. In the pathological class none had died from the former cause, and only 1 in 24·6 from the latter; but in the traumatic operations 1 out of every 26·5 had perished from the one, and 1 of every 10·6 of the other. It will be also observed, that as both classes are pretty nearly equal in their liability to phlebitis, the chief excess of mortality, in the second and third weeks after amputations for injuries, is produced by inflammatory complaints. From this the practical rule may be deduced, that, after amputations for injuries, we should most carefully regulate the diet of the patients, confining them, in the majority of instances, to an antiphlogistic regimen for the first three weeks, at least, after the operation, and that constant care should be employed to catch the first symptoms of inflammatory disease.

After the fifth week, according to Table XIV., although no cases of traumatic amputation had been cut off at the Newcastle Infirmary, several instances of death appear to have occurred amongst those operated upon for diseases; and, on referring to the Glasgow Report at Table X., it is found that the only case of bed-sore belonged to this latter class. Another cause of death besides exhaustion is, that in some cases of pathological amputation, diseases of important viscera, as phthisis, which had previously been latent, become quickened into activity, and destroy the patient after the removal of the limb.

In the next Table, the pathological amputations are subdivided, so as to display the mortality existing in cases of amputations performed for different diseases.

TABLE XV.

Shows the success following Amputations performed at the Newcastle Infirmary for different diseases, the Amputations being classified according to the part at which the disease existed.

	Thigh and Knee.					Leg and Foot.					Arm and Elbow.					Fore Arm & Wrist.				
	No.	Av. Mort.	Cure.	O. P.	Death.	No.	Av. Mort.	Cure.	O. P.	Death.	No.	Av. Mort.	Cure.	O. P.	Death.	No.	Av. Mort.	Cure.	O. P.	Death.
Diseased joints and bones,	49	5·44	49·34	67·8	15·77	49	16·33	52·13	42·	34·66	5	...	41·4	8	4	42·66	...	7·
Ulcers,	17	2·12	59·71	37·	10·5
Cases marked "diseased,"	2	1·	33·5	1	1·	3·
Tumours,	1	1·	38·	1	...	29·
Gangrene,	1	61·	...	1	11·	...	16·
Diffused aneurism,	1	48·
Malignant disease, ..	1	...	50·	4	4·	50·33	...	9·	3	46·5

The diseases of the joints and bones are here classed together, because the nature of the affection in the latter has not been, in all cases, accurately detailed; and the majority, consisting of caries, are nearly similar to the disease of the joints usually requiring amputation. We observe how much more favourable are amputations when performed for this cause, than for other diseases; thus, in the leg cases, the mortality for ulcers is 1 in 2·12, whilst, for diseased joints and bones, it is only 1 in 16·33.

The next table shows the amount of death to be exceedingly small, in whatever hospital the records of the operation for diseased joints and bones are examined.

TABLE XVI.¹

A View of the Mortality following Amputations performed for diseases of the Joints and Bones at different Hospitals.

	Thigh.			Leg.			Arm.			Fore Arm.			Total.		
	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.
Liverpool North- ern Hospital,...	15	1	15·	9	1	9·	4	1	4·	28	3	9·33
Edin. Infirmary, ..	13	2	6·5	7	21	3	7·
Massachus. Hos- pital,	20	3	6·66
University Col- lege Hospital,	37	5	7·4
Glasgow Infirmary, ..	24	6	4·	9	2	4·5	8	3	131	23	5·69
Hotel Dieu,	12	5	2·4	9	2	4·5	1	2	24	7	3·42
Newcastle Infirmary.	49	9	5·44	49	3	16·33	5	8	2	4·	111	14	7·92
Total, .	113	23	4·91	83	8	10·37	18	1	18·	13	2	6·5	372	58	6·41

¹ The total number of amputations at the Glasgow Infirmary, consists of the whole of the diseased joints and bones as given by Dr Lawrie. Those placed under the heads of thigh, leg, &c., are composed of a few of the diseased joints alone.

How strikingly different are the conclusions which must be drawn with respect to the event of amputations, when thus classified, from those arising from the inspection of the mortality of the most successful general division; viz., that of amputation for pathological causes! The surgeon, looking only at the number of deaths compared with the amount of cures in a general summary of the results of all amputations, must feel inclined, with a late author, to "raise a question as to the value of operations at all;" whilst, when he learns that seventeen out of eighteen of the individuals whose arms have been removed for diseased joints have been thereby saved from certain death, he views operative surgery in its proper light, as one of the greatest blessings which have resulted from the exercise of human skill.

In the case of the thigh, for whatever cause we amputate it, a considerable number of the cases seems to perish; but this mortality greatly diminishes as the amount of body we remove is lessened:—thus, whilst of amputations performed above the knee for diseased joints, 1 person out of 5 appears to have died; below that point, only 1 in 10 has perished.

The small number of deaths, after amputation of the arm, is worthy of remark, on account of the value of amputation as compared with resection of the elbow joint.

Now, although no deaths have occurred of five amputations of the arm at the Newcastle Infirmary, yet of nine cases of resection of the elbow for disease, three have died, or 1 in 3. Of the six cured, I have been able to trace two who, within two years after the operation, died of phthisis. The average period of cure in the amputation of the arm, at this same infirmary, was 41·4 days; whilst that in those who submitted to resection, was 59·5 days; and the average time of dismissal of those who left the hospital as out-patients, with their wounds still unhealed, was 121 days. In deciding, therefore, upon the nature of the operation we will perform upon a diseased elbow, the surgeon must carefully consider, whether the amount of bone requiring removal will afford a sufficient chance of a useful arm to compensate his patient for the greater hazard to life incurred by submitting to resection.¹

It is, however, somewhat remarkable, that the amount of death should be greater after amputations of the fore arm than after those of the arm, when in traumatic amputations it is so much less. The increase of death is caused by two cases reported from the Newcastle Infirmary; both were operated upon in unhealthy weather, and on the same day, and both, I believe, died from erysipelas—one on the fifth, and the other on the ninth day after the operation. No death is recorded in any of the other hospitals.

¹ It must be remembered, that this mortality of the operation of resection, is only the result observed at the Newcastle Infirmary. Surgeons of other institutions have, I believe, been more successful; but I have not, as yet, collected the returns of their practice.

I have not been able to obtain a sufficient number of the post-mortem examinations of cases dying after amputation for diseased joints and bones, to throw any light upon the nature of the diseases to which this class of operations is most liable, but have added a calculation of the comparative danger of the different classes of pathological amputations, in each week after the operation, as observed at the Newcastle Infirmary.

TABLE XVII.

Shows the Mortality observed at the Newcastle Infirmary within certain periods after Amputation performed for different Diseases.

Newcastle Infirmary.	1 to 4 days.	4 to 7 days.	2d week.	3d week.	4th week.	5th week.	6th week.	7th week.	9th week.
Diseased Joints and Bones, ... }	55·5	54·5	26·75	0 in 103	34·33	50·	98·
Ulcers,	17·	8.	4·6	5·5
Other Diseases,...	16·	0 in 15	15·	14·	13·	...	12·	11·	...

Compared with the vast mortality after amputations for accidents, the chance of death, after an amputation for a diseased joint, seems to be, in the first four days, scarcely worthy of notice; not two deaths in 100 amputations taking place during the period of shock. The danger seems pretty equal in the first and second weeks, after which time, we may imagine, from the absence of mortality in the following period, that the danger from inflammatory complaints is trifling. In the fourth and fifth weeks, it may be reasonably supposed that the usual cause of death has been exhaustion, to which we before found persons suffering amputations more liable when the operation was performed for pathological than for traumatic causes.

TABLE XVIII.

A comparison of the Mortality at different Hospitals following Amputations performed for various diseases.

	Massachusetts Hospital.		Liverpool Northern Hospital.		Glasgow Infirmary.		University College Hospital.		Edinburgh Infirmary.		Newcastle Infirmary.		Total.		
	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Dead	Av. Mort.
Ulcers,	15	15·	5	1·25	17	2·12	37	13	2·84
Diseased soft Parts,	3	3·	12	4·	9	9·	3	1·	15	5	3·
Tumours,	2	2·	4	2	2·	16	5	3·2
Gangrene,	4	...	8	4·	5	2·5	1	1·	2	2·	20	6	3·33
Aneurism,	2	1	1·	1	...	4	1	4·
Deformities,	1	...	2	3
Amputation of } Stumps,	4	...	2	...	1	...	7
Malignant Diseases,	2	8	8·	10	1	10·

The removal of a stump appears to be attended with less danger

than an amputation even for a diseased joint, and perhaps the explanation of the fact is pretty apparent. In the second amputation, although the patient be sufficiently reduced by the suffering for which he submits to the operation, he is seldom so much exhausted by hectic as to excite any fears of his perishing from shock; in addition to which, having before undergone the trial, he is not so much depressed by the fear of a fatal result. The system has also become accustomed to the loss of a portion of the body, and the fact of the patient's having before recovered, is a good indication that no latent disease exists which might produce death at a late period after the amputation.

The operations for malignant diseases seem, at first sight, peculiarly successful; but, on referring to Table XV., this is easily explained. It will be there seen, that at the Newcastle Infirmary one in four died of those who suffered amputation of the leg for malignant diseases; whilst, in the class of diseased joints, only one case of leg amputation perished out of 16·33, and the numbers of the operations on the arm and thigh for malignant disease are too small to expect any mortality. The greater number of the amputations of the leg in this class of malignant diseases, at this infirmary, were performed for fungoid diseases of the bones, a complaint which does not seem to have the same tendency to be reproduced as tumours of a similar appearance when existing in the other structures of the body. In the fatal case only was any enlargement of the glands of the groin remarked. The disease in the thigh case consisted also of a fungoid tumour connected with the bone. In the arm, two cases were cancerous ulcers, and one was a fungoid tumour.

The mortality, after amputations performed for deformities, is probably regulated by the same general rule that we apply in our consideration of the hazard of removing a limb for a tumour, which may obstruct the motions of the limb, as in both the patient is usually in the enjoyment of good health. If in such cases the patient be much reduced by pain or other causes, his chance of recovery will be allied to that of those affected with a diseased joint; whereas, if his only complaint be the inconvenience arising from the disease, and his condition of body be robust, his hazard will be much greater. Only three cases of amputation for deformity are given in Table XVIII., and none terminated fatally, although I have seen persons who have been relieved of their limbs for this cause by over-zealous operators, who have forfeited their lives to the experiment. Indeed, no surgeon can be justified in removing a limb for deformity alone, even at the most earnest request of his patient, unless he first explain to him the danger he will incur by the operation; and, if the surgeon agree to undertake it, the most careful preparation should be used, as, taking together the numbers of the two classes just quoted—viz., those of amputations for tumours and deformities—we have five fatal cases out of nineteen operations, or 1 in every 3·8.

Of the four cases of aneurism in Table XVIII., three were ampu-

tations of the thigh, of which two were performed for diffuse femoral aneurism, and both recovered; the other died from phlebitis after the operation, which was required on account of hemorrhage from a popliteal aneurism. The fourth case was an amputation of the arm, performed on account of hemorrhage occurring after a ligature had been placed on the brachial artery, which had been previously wounded in venesection.

Along with the cases of gangrene have been included four amputations for frost-bite, performed in the Massachusetts hospital; but as this class contains all descriptions of mortification, but few practical conclusions can be derived from its results. In one case, in the Newcastle Infirmary, where the leg was removed below the knee for gangrena senilis, the disease reappeared in the stump, upon which amputation was again resorted to, and the patient, notwithstanding that he was at an advanced period of life, eventually recovered.

The most unsuccessful of all the pathological amputations are those in which ulcerations form the prominent feature. If we exclude the cases mentioned in the report of the Massachusetts hospital, of the nature of which I am ignorant, we find twelve deaths in twenty-two amputations, or 1 in every 1.83. It is from the great mortality so generally remarked as following amputations performed for ulcerations, that some have recommended union by granulation after such operations; they supposing that death usually results from the sudden stoppage of a discharge to which the system had been habituated.

It appears to me, that in the consideration whether it is better to attempt the cure of amputations, performed for cases in which a long-continued discharge has existed, by means of adhesion or granulation, two questions are involved. The first of these is, Does the stoppage of a long-continued discharge, under all circumstances, prove injurious? and, 2dly, If such be the case, what is the best method of preventing danger?

Now, in proof that the stoppage of a discharge is apt to produce danger, we may refer to the great mortality following amputations for ulcerations, when compared with that consequent on the same operation performed for other diseases. Besides which, it is frequently seen in hospitals, that an individual, shortly after being cured of an ulceration, is attacked by some other disease, the symptoms of which, although the complaint before existed, had not previously appeared. A case, strikingly illustrative of this, occurred a few years ago at the Newcastle Infirmary. A thin spare man, of intemperate habits, had been for many years troubled with chronic ulcers of the legs. His health, in other respects, appeared excellent; but the sores resisted every means tried to heal them. For many years he frequented the hospital, and every surgeon connected with the institution had treated him ineffectually. At last the sores

commenced healing, and in a few weeks he was quite well. He had only been dismissed for a fortnight, when he again presented himself, labouring under a new set of symptoms. General dropsy had appeared, and he died in a few weeks. On a post-mortem examination, the heart was found vastly enlarged; and yet, during the many years that this disease must have existed, he had never complained of the slightest inconvenience from it, until the discharge from the legs had been stopped. Again, Mr Phillips has informed us, that in Great Britain, of 60 cases of different diseases in which ulcerations existed, and in which immediate union took place after amputation, 15 died—or 1 in 4; whilst of 26 instances of union by granulation, only 5 deaths occurred—or 1 in 5·2; and of 57 persons in whom, in other countries, the former plan had been tried, 14 perished—or 1 in 4·07; whilst of 70 whose wounds had united by granulation, only 15 died—or 1 in every 4·6.

On the other hand, we seldom amputate a diseased joint unless the complaint be of considerable duration, and the system has been, therefore, for some time accustomed to discharge; and yet so little danger has resulted from the ordinary method of dressing the stump, that only 1 death has resulted of 18 amputations, performed in different hospitals, for diseased elbow. Again, how constantly do we observe leucorrhœa and dysentery stopped within a few weeks without the slightest injury, and even with considerable benefit to the general health.

How, then, are we to reconcile these adverse facts? By attending to the state of the constitution of the patient. Thus, it is a matter of daily observation, that if, in an individual of otherwise healthy constitution, we remove a limb, which, by its disease, has long worn down his strength, an immediate improvement is the result; but if, on the contrary, the same amputation be performed on another, in whose lungs tubercles are existing, the wound of the knife will be scarcely healed before symptoms of the fatal disease manifest themselves in full force. Now, with respect to cases of ulcers, not only are the greater number of persons requiring the amputation at an unfavourable age; but, in general, the same state of constitution which has necessitated the operation, acts also in producing dangerous consequences after its performance. Few long-standing and extensive ulcerations exist, without their being either dependent on visceral disease, or coexistent with a constitution broken up by syphilis or intemperance; and it should be also remembered, that in by far the greater number the veins of the limb are so thickened and diseased, that they will afford an increased chance of the introduction of purulent matter into the circulation. With respect to Mr Phillips' calculations, it can scarcely be supposed that such a general comparison can be of much service in settling a point of this nature; for where no classification is attempted of the parts at which the operation was performed, and where neither the ages of the patients, nor the precise nature of the disease requiring the removal

of the limb, is taken in consideration, it is evident that very erroneous conclusions might be drawn.

If, then, the stoppage of a discharge be only injurious in unhealthy constitutions, it is evident that, where the complaint is plainly of a local nature alone, as is usually the case with a disease of the joint, it would be folly, by preventing all adhesion, to protract the cure after the removal of the limb. But where we are obliged to amputate in cases in which a long-continued discharge is, to a certain extent, a beneficial action, or where the constitution is obviously unhealthy, it will be well to guard against the sudden removal of that discharge after amputation.

With respect to the second question, authors have recommended two plans,—union by granulation, and the insertion of an issue. It appears to me, that if the continuance of a discharge be alone required, the latter of these will most fully accomplish the object. For, whilst the open state of the wound of an amputation must keep up the liability to phlebitis, the discharge will be less under control than when coming from an issue.

But as to the method of amputating which is the best to be employed, whilst my own experience has taught me to concur with the observation of a military author, that surgeons, in general, are too anxious to glue up an amputation wound by the first intention, I believe that complete union by adhesion should be especially avoided when the operation is performed in cases in which chronic ulcerations exist, as the patent condition of the varicose veins, which usually accompanies such disease, must give an increased chance of any pent up pus being introduced into the circulation. In amputations, then, for chronic ulcers, in which there is either a suspicion of latent disease, or where the veins of the limb are varicose, in addition to the employment of the circular method, it would, perhaps, be advisable to introduce a strip of lint between the edges of the wound, so as to prevent immediate adhesion, and thus allow free exit to any discharge which might be secreted.

(*To be continued.*)

ARTICLE III.—*Contributions to Pathology and Rational Medicine.*

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No. X.—CLINICAL AND HISTOLOGICAL RESEARCHES ON CANCEROUS AND CANCROID GROWTHS.—(*Continued.*)

OBSERVATION IV.—*Cancer of Breast—Excision.*

A WOMAN aged about thirty-five, unmarried, laboured under a tumour, the size of a small orange, which she first noticed five months previously. It was irregular on the surface, and the skin was of a red colour. She had occasionally experi-

enced lancinating pain in it, shooting towards the axilla. Her general health was good. The breast was excised, August 16th, by Mr Syme. The wound healed readily, and, towards the end of September, the cicatrix was sound.

Description of the Tumour.—The tumour was the size of a small orange, of globular form, closely adherent to the skin anteriorly, and posteriorly embedded in the substance of the mamma. It was tolerably circumscribed except at one point, where it sent a prolongation nearly an inch long laterally into the gland. On section it generally presented a dirty grayish colour, reticulated all over with an orange yellow matter, and here and there presenting small irregular patches of a crimson colour. In consistence it was soft throughout, in one place pulpy, and every where yielded a copious milky fluid on pressure.

Microscopic Examination.—The fibrous structure of the tumour was exceedingly loose, generally running in parallel lines straight or waved, and not forming cysts. In the meshes between these fibres numerous cancer cells were present. These were generally of an oval form, though some were round, and varied in size from the 1-50th to the 1-20th of a millimetre in diameter. Some contained a nucleus with two nucleoli; others, a double nucleus. Mixed with the cells were numerous granules, and several naked nuclei, characterised by their oval form and great delicacy. The orange-coloured creamy matter in the reticular form was composed of numerous compound granular corpuscles and masses—(See Fig. 11). On the addition of acetic acid, the cancer cells exhibited the appearance formerly described—(Fig. 12.)

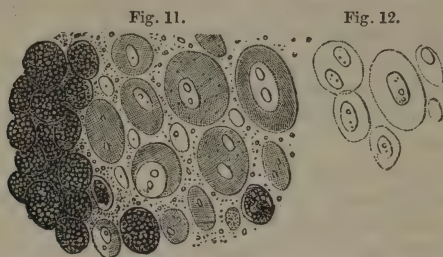


Fig. 11. Cancer cells and compound granular corpuscles in the tumour described Obs. IV.—
Fig. 12. The former acted on by acetic acid.

Remarks.—This tumour presented all the characters of Muller's carcinoma reticulare, and was considered by all who saw it to be a very malignant growth. Some of the corpuscles were large and well developed, whilst others existed in every stage of formation, from the simple oval nucleus to the large cell with double nucleoli. The large quantity of fatty matter in the form of compound granular corpuscles, constituted a striking part of this tumour, presenting all the appearance and structure of the fawn-coloured softening of the brain. Whether this peculiar formation be owing, as some suppose (Walshe), to the occurrence of inflammation in the growth, or whether, as has been lately maintained by Reinhardt and Virchow, it is the result of disintegration of the cancer cells themselves, is a point I shall discuss fully hereafter.

OBSERVATION V.—*Cancer of the Breast—Excision.*

I received by post, April 13, 1847, a slice of a tumour removed by Mr Fergusson of London, from a female breast the previous day. The woman was aged

sixty-three, and had, says Mr F., "so malignant a tumour that I could scarcely be induced to operate. Nevertheless, up to the present time (October) there has been no return of the disease." The section presented a white, glistening structure, here and there of pulpy consistence, with small patches of a yellow substance, soft as cream, deposited in it. It yielded, on pressure, a tolerably copious milky juice.

Microscopic Examination.—The fibrous structure of the tumour was more dense than in the last case, composed of fibres running in parallel lines in bundles, between which the corpuscles had been developed. These were of the same character as in Observation IV.; only a few of them presented a caudate form, and some presented several prolongations—(Fig. 13). Acetic acid caused the cell wall to become more transparent, and the nucleus very apparent—(Fig. 14). The fibrous structure was also rendered more transparent, and oval or elongated nuclei could be seen scattered through it, (Fig. 15). The yellow, creamy substance was composed of numerous granules and molecules, which entirely disappeared on the addition of ether—(See Fig. 16).

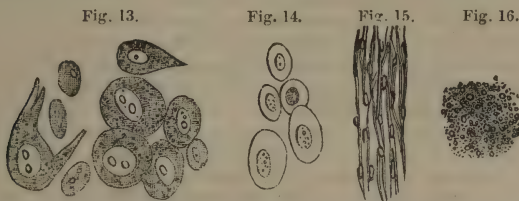


Fig. 13. Cancer cells in portion of tumour described Observation V. Fig. 14. The same acted on by acetic acid. Fig. 15. The fibrous structure acted on by acetic acid. Fig. 16. Structure of the yellow, creamy substance.

Remarks.—In this tumour, we observe that several of the cancer cells were elongated at one end, so as to present a caudate appearance. This form of cell is now recognised by histologists not to be characteristic of cancer, although there can be no doubt that many such cells were originally oval, and subsequently more or less elongated by pressure. We have here an instance, also, of cells throwing out various prolongations, and assuming a very irregular form. Yet the existence of a nucleus with double nucleoli, as seen in the figure, prove to us that they possess the same mode of development, and are identical with the round, or oval cancer cells. In this, as in the last Observation, the quantity of fat accumulated is worthy of notice, existing in the granular rather than in the corpuscular form.

OBSERVATION VI.—*Cancer of the Breast—Excised—Return of the Disease—Re-excision—Re-appearance of the Morbid Growth.*

The patient, a remarkably fat and otherwise healthy woman, æt. fifty, from the country, had had the mamma excised for cancer two years previous to her consulting Dr Douglas Maclagan. He found that the cicatrix was again affected with the disease; and, as she insisted on its being again removed, it, as well as the whole of the mamma, was cut out, May 6, 1846. The wound, in the course of healing, took on unhealthy action, and sloughed. It had, therefore, to heal by granulation; but, before this was completed, she was sent by Dr Maclagan into the country for change of air. She returned in June 1847, with the disease again established in the cicatrix. Neither on this, or at any previous time, had the axillary glands been affected, and her general

health has always been excellent. Dr MacLagan now refused to excise the tumour for the third time. She returned to the country, and has not since been heard of.

Description of the Tumour.—The part excised consisted of the entire mamma, which was very voluminous, loaded with fat, and weighed upwards of two pounds. Anteriorly, there was a deep cicatrix, surrounded by puckering, about two inches long, in the centre of which was a nodule of firm carcinomatous matter, about the size of a walnut. It was slightly elevated above the surface, and was firmly adherent to the skin. On section, it was seen to be of white colour, with smooth surface, a little softened at one point, and sent several prolongations posteriorly and laterally into the substance of the gland. Scraping with the knife, and very strong pressure, produced from its surface a thick, white, pultaceous fluid.

Microscopic Examination.—The cancerous juice squeezed from the softened portions of the diseased mass, contained some round and oval cancer cells exactly similar to those described in previous Observations. A considerable number of them, however, had undergone marked changes in their form, being elongated, caudate, spindle-shaped, or approaching a square shape—(Fig. 17.) One of these, of a caudate form, represented at the top of the figure, measured 1-12th of a millimetre in length. The nuclei were generally of an oval form, and contained a double nucleolus. In the cell just now indicated, the nucleus measured about 1-50th of a millimetre in its longest diameter. It contained at one extremity two nucleoli situated transversely to its long axis, and, at the other extremity, a large vesicular nucleolus 1-125th of a millim. diameter, enclosing a granule. On adding acetic acid, the same change was observed as in the previous observations—(Fig. 18.) A section of the morbid growth with Valentin's knife, exhibited its stroma to consist of a fibrous mesh-work, in which the cells, mingled with numerous granules and free globules of oil, were embedded. Considerable collections of compound granular corpuscles could also be observed here and there.

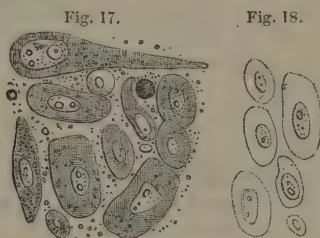


Fig. 17. Cancer cells in growth described Obs. VI. Fig. 18. The same acted on by acetic acid.

Remarks.—This morbid growth had been excised twice, and although on the latter occasion the entire mamma was removed, it again returned. The cancer cells were large and well developed, and exhibited in a characteristic manner one of the modes in which the nucleoli are developed within the nucleus. The alteration in the form of the cells, is here, as in Obs. V., deserving of notice.

OBSERVATION VII.—*Tumour of Breast—Excision.*

On the 20th of May 1847, I received from Mr Page of Carlisle, a slice of a tumour removed by him from a female's breast. The following is the note he was so good as to give me of the case;—"The woman is forty-two, married, no children, always enjoyed good health. An uncle had cancer of the face, which was removed many years before his death, and there was no

return of the disease. About four months before admission she was suddenly attacked with pain in the right breast, of an acute throbbing character, at times very severe; at first confined to one spot, where there was a hard swelling; subsequently there were three prominent parts, all hard, and in the neighbourhood of the nipple, and in all the same pain. The breast rapidly increased in size, but the pain was principally seated in the prominent parts. On admission the whole breast was greatly increased in size, heavy, the skin very vascular and tense, and in some parts slightly prominent portions, in which there was an indistinct sense of fluctuation. The breast perfectly smooth on the subjacent parts; considerable superficial tenderness, rendering an examination difficult. No retraction of the nipple, and no enlargement of the neighbouring glands. The tumour weighed $4\frac{3}{4}$ pounds, and was composed of two distinct structures dispersed irregularly throughout the whole mass; a portion of each of which you received. Her recovery has been uninterrupted."

Description of the portion of Tumour received.—The portion of tumour I received was about three inches long, an inch broad, and two lines in depth. At one end it consisted of an amber-coloured, tolerably firm gelatinous mass, resembling slightly opaque gelatine, in which, here and there, faint striæ were observable. Towards the centre of the slice several white lines were seen, here and there mixed up with the jelly-like substance. These lines became more and more numerous, and crowded together towards the other end of the section, which offered the appearance of a perfectly opaque white mass, resembling in its general aspect and consistence firm *blanc mange*, and presented a marked fibrous structure when torn. No milky juice could any where be squeezed from the surface.

Microscopic Examination.—The amber-coloured gelatinous portion was composed principally of a structureless blastema, containing here and there fibres of extreme delicacy, which, with careful management of the light, might be observed to assume the form of fusiform corpuscles. In the midst of the jelly-like blastema, at different depths, were a number of compound granular masses. Some of these, when brought into focus, presented a number of granules, varying in size from the 1-500dth to the 1-200dth of a millimetre in diameter, which highly refracted light, and were aggregated together without being enclosed within a cell wall. When out of focus, these masses presented a brownish dark shadow, as represented—Fig. 19. Where the amber-coloured jelly was passing into the white substance, the fusiform cells became more numerous, and were mingled with a number of oval nucleated cells of great delicacy, varying in size, and especially in length; some being caudate, others spindle-shaped, as towards the right side of Fig. 20. As the white substance became firmer, these elements were more compressed together, as is seen towards the left of the same figure. A thin section of the densest part of the white portion presented a fibrous structure, wholly composed of fusiform cells, which, on the addition of acetic acid, became more transparent, whilst the nuclei were rendered very distinct—(Fig. 21.)

Fig. 19.

Fig. 20.

Fig. 21.

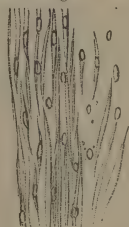
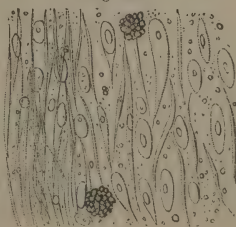
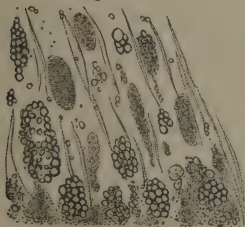


Fig. 19. Structure of jelly-like matter in tumour described Obs. VII. Fig. 20. Structure of the same matter passing into fibres. Fig. 21. The same acted on by acetic acid.

Remarks.—This interesting specimen of morbid growth exhibited an unequivocal example of a jelly-like or simple blastema, in which fibrous tissue was forming. The developement could be watched throughout all its different stages. In the jelly-like matter were a number of fat granules, the number of which gradually disappeared as the structure became more fibrous. The oval corpuscles might be seen, in a very early stage, resembling some of these isolated granules, and gradually becoming larger and larger, caudate, spindle-shaped, and at length fusiform. There was no appearance here of a cancer cell, and we must regard this tumour as one in progress of development. According to the report it was of four months' standing; and yet, in that short period, exudation had been thrown out to the amount of $4\frac{3}{4}$ pounds weight. The time had apparently been occupied more in the exudative process than in the process of development.

OBSERVATION VIII.—*Cancer of Cheek—Excision.*

Jane Johnston, æt. forty-six, admitted May 5th into the Royal Infirmary, under Dr Duncan, with carcinoma of the upper lip and left cheek. About six years ago, it began as a small wart at the angle between the left ala of the nose and the cheek, and has been gradually on the increase ever since. Within the last six months it has grown much more rapidly than it did in the earlier part of its course. It now involves two-thirds of the superior labium, the left ala of the nose, and the skin of the cheek, as far as the malar prominence. The patient suffers little pain from it; her countenance is sallow, and of a harsh, unhealthy appearance. She says that she has, from time to time, experienced lancinating pain in the part, but this is somewhat doubtful. *May 7th.*—To-day the whole of the diseased parts were removed. The wound healed rapidly, and she was discharged cured, May 25th.

Description of the Morbid Growth.—The part removed had been soaking twenty-four hours in water before I saw it, whereby the whole had been perfectly blanched, and the epidermic surface considerably softened. The skin was considerably thickened and indurated, and growing from its surface were several nodules of firm consistence, and varying in size from a pea to an almond-nut. On cutting into one of these, the incised surfaces presented a dead white, and partly fibrous structure, but yielded on pressure a thick, glutinous, milky fluid.

Microscopic Examination.—Several sections were made of these tumours with Valentin's knife, one of which is represented—(Fig. 22). At *a* is the external edge of the section passing through the softened epidermis, the cells of which are enlarged, apparently swollen by steeping in water. They occurred isolated and in groups. A few were round, but they were generally of a somewhat square shape, varying in their longest diameter from 1-50th to 1-30th of a millimetre. Their nucleus was delicate, yet well marked, of round form, measuring about the 1-100th of a millimetre across, with a single nucleolus attached to the wall of the nucleus, about the 1-300th of a millimetre in diameter. Those which occurred in groups were greatly altered in shape, and formed a continuous surface by being agglutinated at their edges (*b*). These epidermic scales were mingled with a great number of fusiform corpuscles, of a flattened form, distinctly nucleated, presenting a narrow edge when turned sideways, and confusedly lying together or crossing one another. The tissue of the dermis was considerably hypertrophied, consisting of curled filaments of elastic tissue, with a fibrous mesh-work, in which numerous loculi or round openings were observed, as at *c*. Some of these were completely filled with

cancer cells, and others were seen infiltrated amongst the meshes of the fibrous structure at *d*. Here and there compound granular masses were visible occurring in the stroma. The milky juice squeezed from the cut surface contained numerous cells, resembling in every particular those described and figured Obs. I.—(See Figs. 3 and 4.)

Fig. 22.

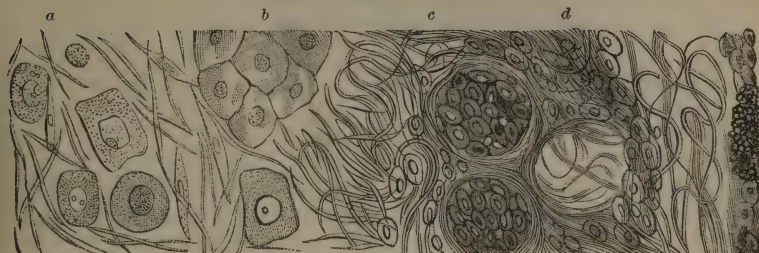


Fig. 22. Appearance of section of cancerous tumour described Obs. VIII. *a*. Epidermic scales and fusiform corpuscles on the external surface. *b*. Group of epidermic scales. *c*. Fibro-elastic tissue of the dermis. *d*. Cancer cells infiltrated into the fibrous tissue, and filling up the loculi of dermis.

Remarks.—The epidermic scales in this case offered a striking resemblance to some forms of cancer cell, represented Fig. 17. One of the nuclei also, it will be seen, has a double nucleolus. I shall afterwards point out how epidermic and epithelial cells, after being steeped in water or a thin serous fluid, may exactly resemble in every essential particular cancer cells. In the present instance, however, they existed rather in the form of flattened scales, which, by their aggregation, formed in some places a contiguous surface, as at *b*. The epidermic cells also were external to the fibrous structure; whereas the true cancer cells were situated within the meshes formed by it, or filled up the loculi in the dermis, where, in a healthy condition, the sebaceous follicles, hair bulbs, and blood-vessels are situated, as at *c*.

OBSERVATION IX.—*Fungoid Tumour of the Bladder.*

On the 24th of April 1846, I visited the Glasgow Infirmary, and was present during the examination of an individual who had died there under the care of Dr W. Thomson. A fungous or medullary tumour, about the size of a hen's egg, of very soft consistence, and of a whitish colour, was attached to the mucous surface of the bladder. A smaller one, about the size of a hazel-nut, was attached to another portion of the mucous surface by a peduncle. I have been unable to obtain any account of the symptoms of the case, but was kindly allowed to take home with me a portion of the larger tumour for examination.

Microscopic Examination.—A portion of the soft medullary tumour contained the structures represented, Fig. 23, composed of, 1st, numerous molecules and granules; 2d, oval and round corpuscles, some of which contained two distinct granules; 3d, oval nucleated bodies; 4th, caudate and spindle-shaped cells; and 5th, fusiform corpuscles with one extremity more or less divided. On the addition of acetic acid, the cell walls in general were rendered more transparent whilst the granules and nuclei remained distinct—Fig. 24.

Fig. 23.

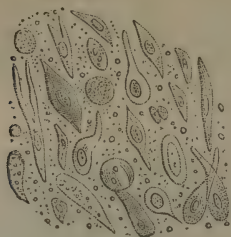


Fig. 24.



Fig. 23. Corpuscles in soft cancerous looking tumour of the bladder, described Obs. IX. Fig. 24. The same acted on by acetic acid.

Remarks.—This is another case where, although the morbid growth exhibited all the appearance of soft cancer, it becomes at first sight very difficult to say whether the corpuscles it contained were or were not epithelial cells. I shall allude more especially to this point hereafter. This is the only cancerous-looking tumour I have met with on the mucous coat of the bladder; and I have therefore considered it worthy of being recorded, notwithstanding the imperfection of its clinical history.

OBSERVATION X.—*Cancer of the Testicle—Excision—Cure.*

James Anderson, a quarryman, æt. forty, was admitted into the Royal Infirmary, Sept. 17, 1842, under the care of Mr Syme, with a large hard swelling of the right testicle. He states, that about two years and a half ago he received a blow on it from a large crowbar, immediately after which the organ became swollen, and felt very painful, especially at two points. In both of these points an abscess formed, was opened and healed. The tumour, however, continued to increase in size, although for a twelvemonth he felt little or no uneasiness in it except what arose from its bulk, which, he says, at the end of that time was as large as a goose egg. At this period he felt a “stounding” pain in the part, passing in the direction of the inguinal canal. This continued for some time, although partially relieved by successive poulticing. The swelling has continued to increase up to the period of admission. On examination, the left testicle was found to be of its natural size and consistence. In place of the right one there is a hard solid tumour, the size of a large cocoa-nut. The cord seems to be implicated as far as the external opening of the canal. The patient is of a strong constitution, although his countenance has a slightly cachectic aspect. *September 21st.*—Mr Syme to-day excised the tumour in the usual way, except that the whole body of the morbid growth was separated from its attachments before the cord was cut. This was done on account of the disease extending so far up towards the groin. Eleven vessels were tied, and the edges of the wound brought together by interrupted sutures. He was discharged cured, *November 8th.*

Description of the Tumour.—On making a section of the morbid growth, it was seen to be composed of two substances, one presenting all the characters of cerebriform cancer, the other a bright yellowish granular matter embedded in it in masses. The first was generally of an opalescent greyish colour, semi-transparent, and of gelatinous consistence, here and there passing into a dull white colour and firmer material. The upper third was more dense than the inferior portions, and of somewhat fibrous texture. This medullary matter was sprinkled here and there with small bloody extravasations, of a deep crimson colour, and yielded on pressure a copious milky fluid. Here and there, in the substance just described, were embedded masses of a light yellow or saffron

colour, varying in size from a hazel-nut to a walnut. They were of soft consistence, friable, and readily broke down under the fingers, very much resembling in general appearance and feel the substance of certain farinaceous puddings.

Microscopic Examination.—The white or medullary substance of the tumour was composed of bands of filaments of great fineness, partly composed of fusiform cells, which crossed each other in various directions. In the meshes so formed, were numerous round and oval cancer cells of great delicacy, varying in size from the 1-50th to the 1-30th of a millimetre in diameter—(Fig. 25.) The nuclei were very distinct, in different stages of development, some containing one nucleolus, and others two. On the addition of acetic acid, the cell was rendered almost invisible, whilst the nucleus was somewhat contracted and rendered darker—(Fig. 26.) The yellow substance was composed of numerous molecules, granules, and loose oil globules, mixed with corpuscles of irregular form. These approached the square or oval form, and in their longest diameter measured from the 1-100th to the 1-80th of a millimetre. In some one, and in others two highly refracting granules could be distinguished, similar to those seen in the nucleus of the cancer cell—(Fig. 27.)

Fig. 25.

Fig. 26.

Fig. 27.

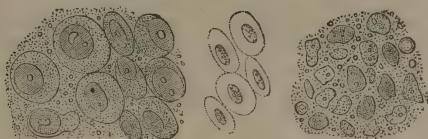


Fig. 25. Cancer cells in tumour of testis described Obs. X. Fig. 26. The same acted on by acetic acid. Fig. 27. Corpuscles and granules in yellow masses of same tumour.

Remarks.—The white or medullary substance of this tumour was evidently composed of the same structure as has been shown to be present in other forms of cancerous growth; consisting of nucleated cells, with double nucleoli, developed in the meshes of fibrous tissue. The yellow substance very much resembled certain forms of tubercle, for which, indeed, it has often been mistaken by pathologists. Its minute structure also has a general resemblance to tubercle, although, on comparison, the corpuscles will be seen to be more like the nuclei of cancer cells. I have always considered, however, that their peculiar appearance was owing to the degeneration or breaking down of the cancer cells first formed, constituting one of the modes in which these cells become abortive. It is the Xanthose of Lebert. We have here also an example of a cancerous growth arising from local injury.

OBSERVATION XI.—*Cancer of the Testicle.—Excision.*

William Steven, æt. thirty-eight, a mason, admitted into the Royal Infirmary September 11, 1847, under Mr Syme. He had a large tumour of the right testicle, which commenced two years previously. For the first two months he experienced stinging pains in it, but subsequently he felt no inconvenience except from the weight, or when pressed upon. It gradually increased in size, and is now of oval form, and measures eight inches in longest diameter. It is firm and solid to the feel; the skin covering it perfectly healthy. *September 24.*—The tumour was excised in the usual manner. A considerable number of arteries were tied at the time, and five more about two hours afterwards, the bleeding having continued. He has done well, but is not yet discharged.—(*October 16.*)

Description of the Tumour.—The tumour was of an oval form, measuring in its longest diameter eight inches, and weighed 2 lbs. 14 oz. On stripping off the tunica vaginalis, its external surface was seen to be slightly lobulated. On section, the cut surfaces generally presented a pale fawn or flesh colour, mottled here and there by crimson extravasations of blood. In two places there was a collection of pale yellow matter, passing gradually into the surrounding substance. One of these measured three inches across in its longest diameter; the other an inch and a half. The consistence of the tumour differed at various places. Towards the circumference, and in the under portion, it was generally firm and fleshy, and presented an evident fibrous arrangement; at one or two spots, however, softening had occurred, over an extent the size of a sixpence, of a greyish colour, and almost diffuent. At the upper portion of the tumour the diffuent softening was much more extensive, occupying about one-fourth of its surface, which, mingled with a considerable quantity of blood extravasated, appeared of a dirty violet colour. The slightest pressure produced copious streams of a milky, reddish or purplish, turbid fluid, from different parts of the surface. The pale yellow matter was of cheesy consistence, somewhat friable, or easily broken down between the fingers, and more dry than the surrounding structures.

Microscopic Examination.—The fluid contained in the softened portions, as well as that squeezed from any part of it, was crowded with cancer cells, compound granular corpuscles, blood globules, and numerous granules. The cancer cells existed in every stage of development; from naked nuclei, containing two nucleoli, to the perfect cell, containing two or even three nuclei. Some were round, others oval, and a few caudate. In size they varied from 1-60th to 1-35th of a millimetre in diameter—Fig. 28. On the addition of acetic acid, the cell walls became more transparent, whilst the nuclei became somewhat contracted, more opaque, and very distinct—Fig. 29. A section with Valentin's knife, showed that those cells were enclosed within the meshes of a fibrous stroma, the filaments of which ran in a waved or straight direction, sometimes crossing each other, at others forming circular or oval curves. On the addition of acetic acid, they presented elongated nuclei, scattered at irregular distances—Fig. 30. The pale yellow cheesy matter was composed of numerous cancer cells, the majority of which, however, had lost their roundness of outline; several appeared indented and shrunk together. In some, nuclei and nucleoli could still be seen; in others, no trace of nucleus could be observed, but only a greater or less number of granules, either confined apparently to the centre of the cell, or diffused throughout its contents. Many irregular shaped bodies, approaching the round or oval, not 1-100th of a millimetre in diameter, could also be observed, resembling tubercle corpuscles, as in the last observation. The compound granular corpuscles were for the most part broken down, presenting no cell wall, but rather a congeries of granules of more or less irregular outline. Several fusiform cells and debris of fibrous texture, together with a multitude of granules and free oil globules, floated among the elements just described.

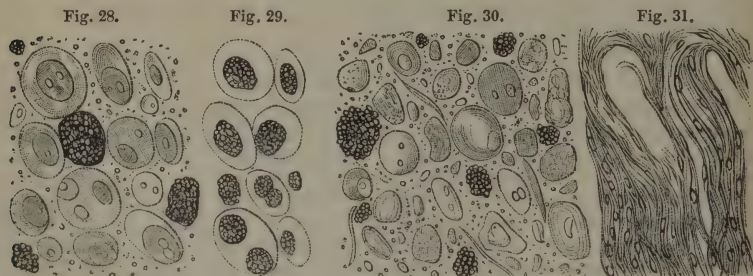


Fig. 28. Cancer cells and granular corpuscles in tumour described Obs. XI. Fig. 29. The same acted on by acetic acid.—(The nuclei have been made too granular.) Fig. 30. Cancer cells and granular corpuscles undergoing disintegration. Fig. 31. Fibrous matrix of the tumour acted on by acetic acid.

Remarks.—In this Observation, the gradual breaking down and disintegration of the cancer cells, to form the yellow material, could be observed in almost every stage: some having lost their contour, others more or less collapsed; the nuclei converted into granules. Many cells were broken down, forming small irregular bodies, resembling tubercle corpuscles. The compound granular corpuscles and fibrous tissue were also broken down. This peculiar appearance also, it must be remembered, was not accompanied with softening,—a fact the more necessary to dwell upon, because it has frequently been considered by pathologists to indicate a yellow softening of cancer. In point of fact, it was more dry than the more perfect cancerous structure surrounding it,—a proof that the fluid it formerly contained had been absorbed.

(*To be continued.*)

ARTICLE IV.—*On an unusual Comatose Affection in Children.*

By JONATHAN TOOGOOD, M.D., Torquay.

DR MARSHALL HALL, in his “Practical Observations and Suggestions in Medicine,” has directed the attention of the profession to a sudden and formidable affection of the head, which occurs independently of any previous scarlatina, of which he had seen two cases in one family, both of which proved fatal; one in six, and the other in twelve hours. The examination exhibited general congestion of the veins within the skull, with a small patch of lymph on the top of the left hemisphere, but no appearance of disease of the heart, lungs, or within the abdomen. I have also met with two cases in the same family, both children having been attacked within a few hours of each other, without any obvious cause. Both cases fortunately recovered, and, as such do not appear to be common, it may not be uninteresting to relate them. These cases were drawn up by the mother, a very intelligent lady, who watched them with the greatest care and most intense anxiety.

“Robert is four years and three-quarters old. He had a cold all the week, but his appetite not falling off, and his appearing quite well, I did not give him any medicine, but, as the weather was fine, sent him out twice the very day he was taken ill. He did not awake during the night, but in the morning of Sunday, the 7th of March 1846, complained to the nurse of feeling tired. His bowels acted as usual, and the nurse gave him a little sal volatile. As this did not appear to revive him, we immediately gave him four grains of grey powder; but, thinking him looking extremely ill, I requested Mr Toogood to see him, who ordered him some castor oil, at eleven o'clock. He continued much the same,—his head hot, his feet with difficulty kept warm,—and so much inclined to sleep, that I had a crib brought down, and laid him on it. He answered when spoken

to, until a little past two, P. M., when, after giving him some gruel, I perceived that he did not speak, and was becoming insensible. Leeches were instantly applied to the temples, four grains of calomel given, and his hair, which was long and thick, was cut off. He continued getting worse, and towards evening some ice was obtained, and applied to his head: his pulse was very rapid. His head was now shaved, and a large blister applied, right across, from one ear to the other. A few spoonful of broth, or gruel, were given, from time to time: he never had any difficulty in swallowing, although we forced the spoon between his teeth. His bowels had now acted several times, and we were desired to throw up some broth with a spoonful of brandy in it. This came away immediately, but the second time it remained. It was now twelve at night. Mustard poultices were applied to the soles of his feet, which appeared to rouse him very much, so that my maid and myself were obliged each to hold a foot; and, when he could bear them no longer, we put them on his knees, which he also resisted very much. A short time before this there had been a good deal of twitching: soon after the poultices were applied, he broke out into a proper perspiration, and began to come to, yawning, licking his lips, and putting out his tongue when desired. Soon after this he threw his arms around the nurse's neck, and began to answer us, and from that time gradually recovered. I omitted to state, that at eight on Sunday evening he had four grains of calomel, which was repeated two hours after, and produced slimy, green, and very offensive evacuations, which continued for some days afterwards, and the water also was extremely high-coloured during the following week. He had some refreshing sleep on Monday, but could retain nothing on his stomach until Tuesday morning."

I met my son in consultation at three in the afternoon, and found the little patient in an extremely critical state. There was great heat, especially in the head, with fever, very rapid pulse, at times intermitting, coma, and threatening of convulsion, and dilated pupils, accompanied with so much sinking as to render the case very difficult to treat. For whilst, on the one hand, it was necessary to relieve the congestion of the brain by local bleeding, and other lowering remedies, the increasing insensibility and sinking, on the other hand, required stimulants. Nourishment, with occasional doses of ammonia and brandy were freely given. I scarcely ever remember a case which excited more lively interest,—and when I left the patient, at twelve o'clock, there appeared but little hope of amendment.

The brother, who was five years and three-quarters old, was attacked a few hours after in a similar manner, and the only apparent cause in his case was unusually severe suffering from cutting teeth, in so much that a medical man at Brighton had recommended, in addition to frequent scarification of the gums, small blisters to be kept open behind the ears alternately. The same treatment was

employed in this as in the former case; but there was much greater difficulty in moving the bowels; and no effectual evacuations were procured until an injection of turpentine had been administered.

In such cases, success depends on the activity, energy, and promptitude of the treatment.

ARTICLE V.—*On the Fœtal Circulation.* By WILLIAM MACDONALD, M.D., F.R.S.E., Lecturer on Anatomy, Edinburgh.

HAVING long doubted the correctness of the received explanation of the circulation of the fœtus in utero, the following is submitted to the consideration of anatomists.

The mode of the circulation of the human adult, being both pulmonic and systemic, has in some measure rendered the explanation of the utero-fœtal circulation confused in its anatomical details; but if it be viewed physiologically in its vascular structure and form, considering the heart and the pulmonic circulation in abeyance, compared with its breathing condition, it will at once be seen to consist of a single system of arteries and veins originating and terminating in the smallest continuous tubes expanding into larger trunks. The utero-fœtal circulation may be physiologically considered as analogous to the aquatic or ichthyo-branchial circulation, where the placenta may functionally represent the analogue of the deciduous branchiæ of the aquatic animals.

The aeration of the blood in the fœtus need not be considered so complete as in the breathing adult; still the blood, after it has been diffused throughout the whole body by the aorta, its branches and capillaries, for the growth and nutrition of the fœtus, and collected by the minute ramuli of the venous system, may be so altered as to require the transmission through some organ to prepare it for renewing and continuing its course through the vascular system. The pulmonic systems within the fœtus being dormant, some other expedient must be found; and this seems to be afforded by the placenta. As in the adult, so in the fœtus, the whole blood distributed by the aortic system is at last collected into the superior and inferior venæ cavæ, to be transmitted through the ærating organ—the placenta. The visceral portion, by its partial circulation being heparised, is then blended in the ductus venosus with the blood from the rest of the body in the united trunk of the venæ cavæ. From this point the circulation may be described as commencing, the heart being considered as a mere receptacle for a portion of the blood, affording, as it were, an elastic apparatus or pump, the blood in the great hemal trunk must find another channel. The flow through the superior cava meets the current of the inferior cava, and by the umbilical vein leaves the fœtus at the umbilicus in its course to the placenta, where it is exposed in sinuses to the maternal blood, in which the loops of the

umbilical vessels are bathed, in the manner pointed out in the Edinburgh Medical and Surgical Journal, vol. 55, by Dr Reid, the eminent physiologist of St Andrews. After this, the ærated or placentized blood is transmitted through the umbilical arteries into the foetal iliacs and aorta, where its course in the great trunk is in a direction contrary to that in the adult; but the current, in all its branches, is the same both in the adult and utero-foetal condition. Thus the whole of the blood, circulating through the foetus, is transmitted through the placenta, whereas in the usual and received description, only a very small portion of it, in the same condition as that employed for the growth of the foetus, is sent through the placenta, while all the impure or altered blood is poured into and mixed with the current in the heart, and sent to nourish the body. In such an intricate subject, so far beyond actual demonstration by examination of the actual circulation in the living human foetus, it is necessary to consider how it may be most rationally explained, and the sketch now submitted seems liable to the fewest objections.

Part Second.

REVIEWS.

Nuisances in Edinburgh, with Suggestions for the Removal thereof, addressed to the General Commissioners of Police. By ALEXANDER MURRAY, Inspector of Lighting and Cleaning. 8vo. pp. 29. Edinburgh, 1847.

Unhealthiness of London, and the Necessity of Remedial Measures. By HECTOR GAVIN, M.D., Fellow of the Royal College of Surgeons of Edinburgh; Lecturer on Forensic Medicine at Charing Cross Hospital. 8vo. pp. 69. London, 1847.

THE reports of the Commissioners for inquiring into the state of the large towns and populous districts in England and Wales, which have now been for two or three years before the public, present a state of things as respects cleanliness hardly credible in a country pretending to civilisation—still less in a country so remarkable for the outward appearance of propriety and comfort as England at large. These reports exhibit in Manchester, Liverpool, Bristol, Leeds, Hull, Sheffield, Birmingham, Norwich, Nottingham, and many other towns of note, such an accumulation of nuisances as one would think Constantinople or Cairo could hardly parallel. But sewerage, polluted streams, stagnant waters, cesspools, dunghills, slaughter-houses, pigstyes, defect of scavenging, damp and crowded

dwellings, and all the concomitant elements of ill-health and epidemic diseases fill these reports with endless repetition.

With the unusually rapid enlargement of great towns, it is much to be feared that the causes of ill-health go on increasing in an accelerated ratio. And though in the United Kingdom it is beyond question that the value of human life has on the whole been much augmented in the last half century, as compared with its value in earlier times, yet it is by no means certain that that improvement is still going on. Nay, there is reason to suspect that at least in some districts a change has already begun in a retrograde direction. At all events it is manifest, that nothing short of a constant attention to the condition of a crowded population can prevent the continual recurrence among them of the numerous causes which deteriorate health. There is one circumstance in particular which deserves the strictest attention at present, as greatly influencing the health of all the large towns of Britain; namely, the continual influx of a numerous Irish population, whose habits, especially as regards crowding together in a narrow space, are beyond measure favourable to the multiplication of nuisances inimical to the public health. Another circumstance equally deserving of attention as continually presenting fresh sources of danger to health, is the unceasing rise of new forms of chemical manufacture. But even the old and well-known causes of disease in towns, the crowding together in a narrow space, and the exhalations from decaying organic substances, cannot be repressed within moderate bounds without a continual vigilance, wherever at least any considerable portion of the population has yet to learn a sense of comfort and habits of cleanliness.

Before applying our utmost efforts to counteract these two fruitful sources of the extension of epidemic diseases in towns, it is needless to await the determination of the question so long agitated among medical writers, whether the mere crowding of human beings in a narrow space, or the concentration of putrid effluvia, be each of itself sufficient to generate anew the poison by which contagious fevers are propagated. In a practical point of view, all the answer wanted is, that where either of these causes prevails, continued fevers spread rapidly—where neither is present, continued fever is little known. The effect of crowding together in a narrow space, is well illustrated in the epidemic at present so widely prevailing among the Irish immigrants into Edinburgh and its neighbourhood. Among the permanent population there has been comparatively little fever; while from the crowded lodging-houses, where often each corner of a small room is occupied by an entire family, numbers have been daily brought to the Infirmary. And, what is singular enough, many of these nests of contagion are in the higher parts of lofty houses; while the Scotch population of the ground floors and lower stories have in many cases remained free from the disease. No doubt, in rooms crowded with people there is not merely a vitiation of the air by the expired air and exhalations from their bodies, but also an accumulation of

effluvia from the natural discharges, often kept for long periods in the room, particularly when high above the street. Every one who has been accustomed to visit the sick in the loftier parts of the high houses of the old town of Edinburgh, must have been often struck with a suffocating close and urinous smell, indicating at once the accumulation of foul water in the apartment, and the little attention to the admission of fresh air. The inmates of houses so ill kept, we have had occasion to remark, seldom escape fever when an epidemic comes round.

Mr Murray, in the little pamphlet before us, mentions among the nuisances of Edinburgh, that the "*Gardez l'eau*," so long a byword of reproach against that city, is still practised with impunity in some of the obscurer closes, wynds, and back courts. Oddly enough, this abomination, though a nuisance to the neighbours, is an advantage to the house from which it is practised, as the difficulty of keeping an apartment sweet to people of indolent habits, is the labour of carrying the foul water down a long stair to the street. This is one of the many inconveniences of the lofty houses of the old town of Edinburgh, now that they are for the most part inhabited by an inferior class of people, whose love of cleanliness is not always strong enough to compel them to the labour necessary to maintain it. And we doubt if it will soon be possible to convince the owners of a property yearly sinking in value, to attach soil-pipes to these high houses, as Mr Murray suggests.

As the "*Gardez l'eau*" cannot be tolerated even in the small extent to which it is still practised, and as the long retention of the foul water in the high apartments is a manifest evil, it is very desirable that steps should be taken to force the introduction of soil-pipes, at least where the property brings any rent at all. It becomes a problem very important for the public health of Edinburgh, whether any steps could be taken under a police or health act to remove some of the inconveniences attending these lofty houses, as the habitations of the labouring class. A great many of these buildings are still in good preservation, and are capable of being rendered sufficiently healthy dwellings, did the means fall within the scope of a legislative measure. But this is rather doubtful. The old town of Edinburgh was in its best days overcrowded with population. To leave out extreme cases, six or seven stories were piled one above another, every story being inhabited by one, two, or several families, and each family occupying from three or four, to six or seven rooms, with a kitchen. Many of these houses still remain as of old, though possessed by families of lower station. Others again, as tenants became scarce for the entire old domicile, were let out in single apartments, and the whole that had before accommodated a single family now received five or six families, each probably as numerous as the one family which possessed it before. This, then, is the second stage of overcrowding. But Mr Murray refers to three houses in the Cowgate, termed the Meal-market Stairs,

which, as we know, underwent this process of division many years ago, without by any means having become unhealthy habitations, the rooms retaining their old names; thus, the members of one family would describe themselves as living in the kitchen, another in the dining-room, a third in the drawing-room, and so on. It appears, however, from Mr Murray's account, that the houses have recently undergone a further change. "These tenements," he says, "are now almost entirely inhabited by the lowest of the population, and within these few weeks have been hot-beds of fever. No district of Edinburgh has more decidedly changed character than this portion of the Cowgate. In 1747, I believe, several of the Scots nobility had flats in the Meal-market Stairs, now occupied as lodging-houses for 'Navies,' who may occasionally be here found crowded by the score into small apartments."

Here then are a set of houses which, a hundred years ago, in the old walled state of Edinburgh, were overcrowded for health and comfort, now occupied by probably at least ten times the former population. We were well acquainted with these houses in the second state of overcrowding, and never remarked that the inmates were often affected with fever. We should say, on the whole, they were rather healthful houses. But, wherever the Irish go, they carry their habits of crowding together with them, and fever follows in their trail. The Grassmarket, and the West Port, have been their favourite streets for many years, and these have been the chief haunts of fever. More recently they have taken possession of the Cowgate and College Wynd, and there fever has taken up its abode. We have already said that this epidemic has taken much less hold of the permanent population than is usual in such epidemics. There can be little doubt that it was imported from Ireland. It prevailed at Glasgow extensively some time before it attracted much attention in Edinburgh. But many of the Irish who have been attacked, had been a considerable time in Scotland, most of them, we should say, at least since last summer. The first of these affected, then, must have caught it by crowding in confined apartments with their countrymen fresh from home. The completely Irish character of this epidemic attacking them at home and in Britain, and hardly spreading to the general population of Britain, almost realises the somewhat fanciful idea of a former age regarding the sweating sickness, the "Seed, or Anglicanus," which was said to attack Englishmen alone in England, and to pursue them over all parts of Europe, without affecting the natives of those countries in which they had taken up their abode.

The following passage enumerates the chief nuisances in Edinburgh referred to by Mr Murray:—

"The more obvious defects in the Police Acts, are connected with slaughter-houses—killing of horses and dealing in carrion—offensive manufactories—feeding and breeding of pigs—imperfect system of drainage—want of public necessaries—ruinous and waste property—disgusting practice of throwing nuisance from windows into closes and back courts—state of the causeways

and pavements in closes and back courts—and the want of power in the General Commission to make regulations from time to time to meet the ever-changing condition of the property and population within the bounds of Police.”—P. 4.

The subject of cesspools is one of much importance in the enactment of regulations for the health of towns. Mr Murray does not mention the depth and other dimensions of the cesspools to which the following observations apply. But if, whatever be their depth beneath the drain of exit, the night-soil and other matters susceptible of putrefaction be carried away, they should not prove so great nuisances as they are commonly supposed to be.

“It is generally observed by those engaged in cleaning drains and cesspools, that nine-tenths of the blackened stuff taken out is sand—the heaviest particles being found nearest the bottom. There are in Edinburgh, parties who hold the opinion that the soil water of the city might be rendered a source of revenue. Were they, however, to examine cesspools formed to collect stuff from some of the leading drains of the city, they would be greatly surprised to find that sand is the chief deposit, and that the stuff obtained, after being allowed to dry, is not worth above 1s. or 1s. 6d. a cart. As proof of the worthlessness of such deposit, the tenant of Lochend farm, one of the most intelligent farmers in the Lothians, has filled up his cesspools, finding it more profitable to have the land in meadow. A company was recently formed in London to transmute by a process of evaporation the soil water of the great metropolis into guano, and the scheme is, I believe, favourably entertained in other quarters; but unless it is managed by concentration, or some other process to render the residuum a hundred times more valuable than the debris collected from the soil water of Edinburgh, the speculation will certainly not pay, to say nothing of the probability of creating an intolerable nuisance. Soil water is indeed highly valuable when used to irrigate land: the most valuable portion, being specifically lighter than water, is rapidly carried along, and grass serving as a filter, arrests and retains the manure. The River Tumble, which at Clock Mill seems a mixture of water and clay, becomes transparent by the time it reaches the sea—fertilizing in its course hundreds of acres, which but a few years since were sand.”—P. 13.

If Dr Gavin, in the second pamphlet on our list, speak correctly in the following passage, the cesspools to which he refers must be different from those referred to by Mr Murray.

“On the subject of cesspools I would observe, that they are pestilential nuisances; that we have abundance of proofs of disease and death being produced by them; and that, with an efficient system of sewerage, and a due supply of water at a cheap rate, they could effectively be abolished, and a proper water apparatus substituted in their stead, at a very trifling cost. For five or six shillings a-year, or for a weekly charge of 1½d. per house, it is stated that water companies could construct, and keep in repair, such an apparatus; this substitution would be the means of remedying one of the greatest abominations which can possibly be suffered to exist in the midst of a large city.

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“It is utterly impossible that any population can be healthy which lives amid cesspools, or upon a soil permeated by decomposing animal and vegetable refuse, ready to give off at all times the most noxious exhalations. Oftentimes we find the walls of houses infiltrated, and constantly wet with fœtid fluid, which destroys the health and lives of those living within its death-dealing influence. Food in such places becomes tainted, and it is impossible to preserve it, even for a single night.

"The springs and wells near cesspools become affected and polluted, and are at last compelled to be abandoned; none ever make use of them but those who do not know their reputation, and their foul produce. One of the noblest public charities in London has, within the last year or two, sunk large cesspools within a few feet of the well which supplies the whole establishment with water, as well for drinking and cooking as for domestic purposes.

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"A few years ago, a public institution, containing from forty to sixty inmates, was the scene of much distress, in consequence of their being attacked by a low form of fever. This occurred in spring, and at first did not excite much attention, until a great number became successively attacked with the disease. The drains were accused of being out of order, underwent repairs, and were trapped. By the time this had been accomplished, autumn had come, and the fever gradually disappeared, with one or two solitary cases; but early next spring the disease again set in, and in a more virulent form; the dormitories became fever wards, and the house a fever hospital. A more complete examination of the premises took place, when it was discovered, that in order to save the expense of removing the night-soil, it had been the custom to remove it from the open necessities and cesspools, and fling it into holes dug in the loose earth at the end of the yard. The earth was sprinkled over the soil, and formed, in dry weather, a sufficient defence against an ocular detection of the decomposing filth contained below; but in rainy weather the soil was washed up, and partly dissolved by the water, which covered the whole of the end of the open yard—there it stagnated, and, in its pasty and semi-fluid state, sent forth the pestilential gases which occasioned the severe and extensive amount of disease which prevailed within-doors. The whole of the night-soil was removed,—the fever abated in severity and frequency, but the inhabitants never in that house had good health. Since the removal of the establishment to a healthier spot, no case of disease has occurred which could not be traced to accidental causes, or agencies in existence previous to the party coming to reside in the establishment; and the duties of the honorary medical officer, in place of being most onerous and oppressive, have become comparatively nominal."—Pp. 36-39.

It has always been believed that fever prevails less, proportionately, in London than in most other great towns. The deaths from typhus in London, according to the tables published by the Registrar-General, amounted in 1843, to 2083; in 1844, to 1696; the mean being 1889. The average annual number of deaths from typhus over England, for the five preceding years—namely, from 1838 to 1842 inclusive—appears, from the same tables, to have been 1080 per million. And thus, since the population of London nearly approaches to two millions, the proportion of deaths from typhus there, in the mean of these two years, is considerably less than that observed over the whole of England for the previous five years. Such a comparative immunity from typhus could hardly be anticipated, from the account given by Dr Gavin, of the insalubrious nuisances which prevail in the metropolis.

"Besides these fertile sources of disease now referred to, London tolerates in its bosom a vast variety of nuisances, alike destructive to the health and comfort of its inhabitants; such as accumulations of filth in nightmen's yards, collections of dung for sale, swine-pens, slaughter-houses, knackers' yards, cow-sheds, pig-styes, gut spinning, tallow boiling, the burning of animal and

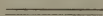
vegetable matters,—such as animal charcoal by the sugar refiners, the desiccation of night-soil, and a thousand others.

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“The inadequacy of the present mode of supply causes the poor to put aside their water for repeated use, and to use but a very small quantity in cooking; it also causes them to boil their vegetables without their being washed, to save them the trouble of fetching more. It, very generally, causes them to neglect every sort of cleanliness, whether of their houses, their persons, or their clothes.

“Another evil consequent on this mode of supply is, that the poor are drawn to the taps common to many houses, where, from the impossibility of all being served at once, and from the fear that the water might be turned off before their turn should come, quarrels are generated—exceedingly injurious to the moral and peaceable disposition of the poorer classes. Mr Rushton, a police magistrate, states that a large proportion of the cases of assault brought before him, are traceable to the disputes engendered by this mode of supply.”—Pp. 44, 45.

The waste of human life annually caused by the innumerable nuisances so general in our great towns—these being even greater in many towns than in London and Edinburgh—is prodigious. Public attention begins to be awakened to the extent of this evil; and it becomes the medical profession in particular to lend the weight of its influence to the speedy application of a remedy.



Klinische Darstellungen der Krankheiten und Bildungsfehler des Menschlichen Auges, der Augenlider und der Thränenwerkzeuge nach eigenen Beobachtungen und Untersuchungen herausgegeben. Von Dr Friederich August von Ammon, Leibarzt S. Majestät des Königs, von Sachsen, &c.—(Clinical Representations of the Diseases and Malformations of the Human Eye, Eyelids, and Secretory Apparatus, based on Original Observations and Researches. By FREDERICK A. VON AMMON. 4 Fasciculi, Folio. Berlin: 1838—1847.)

THE object contemplated by Von Ammon in the extensive and elaborate work now before us, is to give such a representation of the varied diseases of the eye, and the apparatus connected with it, as may not only assist in the self-study of these, but serve as a book of reference, to which the more experienced practitioner may look at times with advantage.

In order to render the work as practical as possible, he has confined it to the delineation of the most striking and remarkable diseases, and of the most important pathological appearances. All rarities, and such disorders as produce only evanescent appearances, or such as never or very rarely produce organic changes, are excluded. In classifying the plates, he follows an anatomical arrangement. The first fasciculus contains twenty-three plates, made up of three hundred and seventy-six coloured figures, representing the diseases affecting the bulb. The second fasciculus has twelve plates,

containing two hundred and ten coloured figures, illustrative of the disease of the eyelids, the cavity of the eye, and of the secretory apparatus. The third fasciculus has twenty plates, with three hundred and seventy-eight figures, showing the congenital and acquired malformations of the eye, the eyelids, and the secretory apparatus. The fourth fasciculus is made up only of descriptive letter-press. We can recommend the work as one of the very best we have seen. The plates are well engraved, well coloured, and are accurate delineations of the diseases they profess to represent.

Part Third.

PERISCOPE.

SURGERY.

SINGULAR CASE OF DISSECTING ANEURISM. By Professor PIRRIE of Aberdeen.

In the body of a man about fifty years of age, who during life had not been supposed to be the subject of any disease, and who died very suddenly, and before any medical man had an opportunity of seeing him, I met with a singular variety of dissecting aneurism. In the arch of the aorta, about three-fourths of an inch to the left side of the origin of the left subclavian artery, there was a rent of the inner and middle coats. From this rent to near the origin of the aorta, on the cardiac side, and for upwards of an inch on the capillary side, the external coat was separated from the middle, and also around nearly two-thirds of the circumference of the artery.

From the aneurism thus formed there was an opening upwards of half an inch in diameter, through which it burst into the pulmonary artery—a little below the part where that vessel gives off its two branches. The aorta was affected with steatomatous deposit in many parts, and, however rare the occurrence, there were, beyond all doubt, patches of the same kind of degeneration in the pulmonary artery. There was also slight hypertrophy of the left side of the heart.

In almost all recorded examples of dissecting aneurism, the heart has been found affected with disease, more especially its left side. In some instances there has been dilatation, with hypertrophy; in others, dilatation with attenuation; and in many there have been evident signs of steatomatous and calcareous deposits in different parts of the vascular system. According to Rokitansky, dissecting aneurism sometimes commences by disease of the *middle* and *internal* coats. In that case the continuity of these coats becomes interrupted, and the separation of the external coat follows as a later event. In other instances, the first deviation from healthy condition is chronic inflammation of the *external* coat, which gives rise to separation of that coat, followed by rupture of the middle and internal coats. In the one class of cases, he considers that the rupture precedes; in the other, that it follows the separation.—*Communicated to Editors of Month. Journ. in a Letter.*

DEATH OF A FEMALE FROM THE ENTRANCE OF AIR INTO A VEIN IN THE OPERATION OF TRACHEOTOMY. By HEYFELDER.

In this case the female was twenty-seven years of age, of good constitution, and in the ninth month of pregnancy. She had a slight enlargement of the thyroid gland. After a restless night she rose one morning with an intense headache,

and, believing herself on the point of delivery, she entered the lying-in ward of our hospital. Two days after she was seized with difficulty of respiration and deglutition, which increased so rapidly that she was placed in a surgical ward. On being examined there on the same day, there was observed on both sides of the neck a swelling almost indolent, as hard as a piece of wood, extending from the chin to the angle of the jaw, immovable, without a distinct border, and slightly convex externally. The skin over the swelling was somewhat tense, and of natural colour and temperature. The interior of the throat could be seen with difficulty, owing to the stiffness of the jaw and the swollen state of the tongue. The tonsils were not enlarged, but the velum and the uvula were much swollen, and had an cedematous aspect. Beneath the tongue there was a pale red swelling, with a callous ring at the inner circumference of the inferior maxilla. This swelling pushed the tongue upwards and backwards, much impeding speech. The dyspnœa was extreme, and nothing could be swallowed. The respiratory murmur could not be discovered over all the left side of the chest; it was very feeble on the right side, and in a short time ceased also—there was a considerable tracheal sound. Percussion offered nothing unusual. The action of the heart was accelerated, and at times tumultuous and irregular. The temperature of the surface was nearly natural. The colour of the face was natural—there was no cyanosis. There was little thirst, no appetite; the voice was hoarse, the speech unintelligible. The dyspnœa was rapidly increasing. Tracheotomy was determined on.

After the first incision was made, the patient insisted on her position being changed, and in the mean time, a venous trunk, the size of a writing quill, became projected forwards in the wound—it was held aside, and the next incision was carried down to the trachea, but during this incision the patient made a sudden bound, with an exclamation; immediately a sound was heard like the escape of air from a tube into which water is entering. It was thought, at first, that the incision had passed into the trachea, but black blood mixed with bubbles of air began to issue from the wound in a uniform stream; the patient fell into a state of extreme agitation, the features altered, the face became of a leaden hue, the pulse ceased, and the action of the heart became enfeebled, and in less than two minutes she expired. The loss of blood did not exceed twelve ounces. Her infant was saved by the Cæsarian section.

The post-mortem examination showed both lungs adherent throughout to the costal pleura, full of blood, cedematous and emphysematous. After tying the great vessels, the heart with the lungs was carefully removed, and, on being placed in a deep vessel of water, the heart did not sink, the right ventricle, which bulged considerably, rising above the water, though several trials were made to turn the left ventricle uppermost. The median cardiac vein showed distinct air-bubbles. A vessel filled with water being inverted over that in which the heart was, the right ventricle was opened under water, when much blood issued, mingled with bubbles of air, which collected in the highest part of the inverted vessel. The left ventricle and auricle were empty. The superior vena cava, the right subclavian vein, the jugular and several branches of the thyroid vein, contained fluid black blood, with a great quantity of air-bubbles. A branch of the thyroid vein, exactly in the median line of the neck, had been divided on its anterior wall, the posterior wall being uninjured. A vein which came off from the trunk immediately below the incision was visibly distended with air. The trachea had not been divided.

The cellular tissue of the submaxillary and sublingual glands on both sides, and in part that of the parotid, was infiltrated with a clear yellowish watery liquid; the periosteum of the inferior maxilla, particularly in the ramus, was much injected and softened. The velum of the palate, the uvula, the glottis, and the epiglottis, were cedematous; the mucous membrane of the larynx, and of the superior part of the trachea, was cedematous, discoloured, and softened; the submucous tissue was injected and softened, and distended with an opaline liquid like whey. The cartilages of the larynx were somewhat placid.

The liver and brain were full of blood; on the internal table of the skull cap, there were very small osteophytes.

The gas collected from the right side of the heart amounted to 7·10 of a cubic inch, and consisted of 0·65 parts of nitrogen, and 0·05 parts of oxygen, and was therefore composed of 0·24 cubic inch of atmospheric air, and 0·46 cubic inch of nitrogen.—*Das Chirurgische und augenkranken Clinicum der Universität Erlangen*, 1845–46.

ON THE TREATMENT OF BURNS WITH TREACLE, OR CLARIFIED HONEY.

By Drs PAYNE AND CARLYON.

DR PAYNE recently recommended the use of treacle as an application to burns, the object being, as it would seem, the exclusion of the air from the affected part. Dr Carlyon has since called attention to the similar utility of clarified honey. The honey, which is more easily applied when clarified, is to be made lukewarm, and smeared over the part, which is then to be covered with cotton, cotton-wadding, or lint. The application need not be repeated oftener than once in two days. He refers to the use of balsam of Peru as being of similar effect in the exclusion of the air, though in many cases it must be too stimulating. He recommends the same practice in cases of wounds and the like, where, without exception, the exclusion of the air is an essential indication.—*Provincial Med. and Surg. Journal*.

ON PARACENTESIS OF THE EYE. By M. DESMARRES.

IN an article in the "Journal des Connaissances Médico-Chirurgicales," by M. Desmarres, on paracentesis of the eye, M. D. recommends puncturing of the eye in cases of acute inflammation, which, without this means of relaxation being employed, so frequently and so rapidly ends in destruction of the organ from purulent matter collecting in the chambers. He relates in this article three cases of internal ophthalmia of the greatest severity coming on after depression of cataract, or laceration of the capsule, and where puncturing of the eye was the means, in the first place, of immediately relieving the intolerable pain which the patient suffered, then of dissipating the inflammatory symptoms, and lastly, of saving the eye from being almost inevitably lost. M. D. performs this little operation in two ways:—1. The cornea at its external circumference is opened by means of a common cataract needle, or the point of a *kératome*, which is made to penetrate into the anterior chamber; a slight movement of rotation of the instrument on its axis separates the edges of the wound, and allows the aqueous fluid to escape. 2. Beer's lancet-shaped knife, or a small trocar, or simply a common lancet, is plunged into the sclerotic, a little below the transverse diameter of the globe, in such a way that one of the cutting edges of the instrument is before, and the other behind. The wound made in the direction of the fibres of the external rectus muscle, should have its anterior angle at a distance of from two to three millimetres from the cornea.—*Gazette Médicale*, August 23, 1847.

ETHERIZATION WITHOUT SPECIAL APPARATUS.

M. GUERSANT, *fils*, in disarticulating the middle finger of a man suffering from caries of the digital bones, used the following method for the inhalation of the ether. A small dish containing about two ounces of ether was placed under the chin of the patient, and the head and face enveloped by a towel. Two minutes and a quarter of inhalation with this apparatus sufficed for the production of insensibility to pain, and the operation was then performed without the knowledge of the patient.—*Journal de Médecine et de Chirurgie*, August 1847.

We are now, to the regret of the instrument-makers, able to dispense with the ingenious, though troublesome and unwieldy articles of apparatus, which, on the first introduction of etherization, encumbered the operator in his proceedings. The use of the common hollow sponge, as proposed by Dr Morton of Boston, and as mentioned by Professor Simpson in the last number but one of this Journal, has, we think, been proved to be the most simple, and at the same time the most quickly effectual apparatus which can be used, when it is not necessary to keep up the state of etherization for above eight or ten minutes; the only objection to its use, when it is necessary to keep the patient for a longer time in a state of insensibility, being the necessarily greater expenditure of ether, which is caused by the large surface of the fluid which is exposed to the atmosphere. The patient in this way is not exposed to the annoyance, or (as we have seen when the first excitement of the etherized state was produced) the supposed indignity of his nose being held. The concavity of the sponge, saturated with about two ounces of ether, is placed before his face, and, after the first three or four inspirations, placed in contact with the face, enveloping the mouth and nose, so that with every inspiration a full stream of ethereal vapour enters the lungs. The convenience of using this method of inhalation will be found, when it is thought advisable (as in children) to produce the state of etherization before they are brought into the operating room. The sponge is retained in contact with the face by an elastic hand or piece of bandage, whilst the patient is carried from bed to the operating table.

TRAUMATIC AXILLARY ANEURISM—OPERATION. By M. GENSOU.

A GENTLEMAN, in fencing, received a deep wound of the axilla from a broken foil. Profuse arterial hemorrhage took place, which was arrested by firm compression. An aneurismal tumour appeared in a short time, which was treated by M. Lisfranc by compression; which, however, had not the effect of diminishing the size of the tumour. M. Gensoul of Lyons, on examining the tumour, found it of the size of a hen's egg, pulsating strongly, and the integuments over it tense and thin. Having determined to try the effect of galvano-puncture, M. Gensoul removed the dressings; but, on doing so, arterial blood issued in a full stream from an ulcerated opening. Ligation of the artery was immediately determined on, and the operation was forthwith performed by M. Gensoul. After a tedious and difficult dissection, a ligature was placed on the artery below the clavicle, immediately above the origin of a very large thoracic branch. The case went on well till the eleventh day, when hemorrhage took place from the wound under the clavicle. Pressure was applied by means of a sponge placed on the wound, and an apparatus by which a heavy weight exerted compression on the sponge. Ten days after this the sponge was removed, when the ligature escaped with the purulent discharge from the wound; and, during the same night, the bleeding was renewed, and the compressing apparatus again applied. On the twentieth day from the operation a small opening took place in the axilla, which at first discharged black blood, and subsequently purulent matter. The tumour gradually diminished in size. Pressure was maintained for about three weeks longer over the wound under the clavicle, after which the wound healed.

Four months afterwards the patient was quite well, and had returned to his business.—*Gazette Médicale de Paris*, September 1847.

DOUBLE HYDROCELE—INJECTIONS OF IODINE AND OF ALCOHOL. By M. ROUX.

THE comparative merits of the iodine and alcoholic injections were tried by M. J. Roux, on a patient suffering from double hydrocele.

Both hydroceles were operated on at once, and the tincture of iodine was injected on one side, and then alcohol on the other. The following remarks were made during the cure of the patient:—"1st, The pain was less from the

iodine injection than from that of the alcohol ; 2*d*, The inflammation caused by the alcoholic injection was more acute, and of longer duration than that produced by the iodine ; 3*d*, The cure was accomplished in the same time by both injections ; 4*th*, In each case the cure was radical."—*Ann. de Therapeutique*, August 1847.

SUBCUTANEOUS EXTRACTION OF A CARTILAGE FROM THE KNEE-JOINT FATAL TO THE PATIENT. By M. PLEINDOUX.

THE following case shows that the above method, as proposed by Mr Syme and M. Goyrand, is not free from danger. A strumous male patient, thirty-five years of age, under the care of M. Pleindoux, in the Hotel Dieu of Nismes, had suffered more or less severely for about two years from a loose cartilage in the left knee-joint, which M. Pleindoux, at the patient's earnest desire, removed by the subcutaneous method. The cartilage, which had attained the size of a flattened chestnut, was made to project to the inner side of the tendon of the triceps ; and, whilst held there between the finger and thumb, a narrow tenotomy knife was introduced, and the synovial membrane freely divided. Much difficulty was experienced in extruding the cartilage from the joint through this opening, and it appears doubtful whether it escaped into the cellular tissue external to the joint or not. A compress was applied over the small wound, and the cartilage retained its situation in the place where it had been pushed to in the operation. Cold was applied to the joint. On the same evening there was heat, pain, and swelling of the parts. These symptoms were much aggravated on the following day, and M. Pleindoux made a free incision over the body, and removed it. Large abscesses formed requiring openings and counter openings. The patient gradually sunk, and died twenty-five days subsequently to the performance of the operation.—*Gazette Médicale*. September 1847.

PATHOLOGY AND PRACTICE OF PHYSIC.

CLINICAL CONTRIBUTIONS BY PROFESSOR PFEUFER, OF HEIDELBERG.

Carcinoma of the Œsophagus communicating with the Aorta.—J. M., aged forty-two, formerly perfectly healthy, was seized in the course of the summer 1840, with a difficulty of deglutition. This act was at first only occasionally impeded, but on his admission into the Zurich Hospital, June 15, 1841, the swallowing of solids had been for some weeks impossible ; they did not pass beyond a spot indicated by the patient opposite to the ensiform process, and were then more or less forcibly regurgitated ; fluids in small quantities were swallowed with difficulty and an audible noise. The patient was wasted, of a dingy colour, had a good appetite, and what food did reach the stomach agreed well. He complained of a constant burning pain, immediately beneath the ensiform process, pressure upon which increased it. On three trials, the probang could never be passed beyond this spot, always producing violent pain ; it was used for the last time on the 26th January ; on the morning of the 27th, the patient suddenly vomited about 3 lb. of red frothy arterial blood ; at 5 P.M. the vomiting returned, and the patient died at 6. Section showed the œsophagus healthy, except opposite the bifurcation of the trachea, where a carcinomatous ulcer, the size of a five franc-piece, existed. Both the aorta and the trachea had been opened into ; the opening in the aorta was one and a half inch in diameter, with irregular swollen edges ; that in the trachea about half as large. The stomach contained about 4 lb. of black blood, which likewise filled the small intestines.

Aneurism of the Aorta communicating with the Œsophagus.—T. F. aged fifty-one, admitted into the Bamberg Hospital, November 10, 1828. For some years he had laboured under pain in the loins, and palpitations; some months ago he had passed blood by stool. This morning, at two A.M., he had been seized with pain along the spine, an agonizing sense of oppression in the epigastric region—violent palpitations and tendency to fainting. About four A.M. he vomited a considerable quantity of blood; the patient was weak, anxious, and restless, and continued so till seven A.M. of the 11th, when he was again seized with violent palpitations, and vomited 2 lb. of blood, partly coagulated and black, partly red, fluid and frothy. About mid-day he again vomited a pound of similar blood, after which the pulse and animal heat fell considerably, and the face became Hippocratic. A clyster given that evening brought away a quantity of black blood, partly fluid, partly coagulated, mixed with fecal matter. In the course of the 12th he complained of anxiety, palpitations, a pain under the sternum, and along the spine, and had frequent eructations, particularly after attempting to take food. That night he was restless, desirous of getting up, drank much, vomited blood several times, and died six A.M., November 13th; no styptic nor analeptic remedies had any effect on the course of the disease. On section, the pericardium was found distended with fluid, the right auricle dilated, the foramen ovale patent, the little finger could be passed through it, the arch of the aorta dilated to double its usual size, the seventh and eighth dorsal vertebræ were carious, and upon them lay an aneurism of the aorta descendens,—its coats were thick, softened, of a brownish red-colour; to this sac the œsophagus adhered just at its passage through the diaphragm, and communicated with it by means of a round opening, three lines in diameter. Immediately beneath this the œsophagus was dilated in form of a sac; the stomach and intestines were filled with black, partially coagulated, blood. In these two cases death was not immediate, although wounds of a much smaller vessel, as the cruralis, prove almost instantly fatal; this, depending probably on individual peculiarities, is most remarkable in the last related, in which, probably, the sac of the œsophagus restrained the bleeding until it was emptied, peristaltically or anti-peristaltically. In a case of rupture of the splenic artery diagnosed by Schönlein, a similar effect was produced by a species of diverticle in the stomach capable of holding about ziv . The arteries of the stomach and intestines are rarely affected with aneurism, and when opened into, this arises most generally from circumscribed inflammation and ulceration of the coats of the stomach and intestines, leading to adhesion and subsequent perforation of the artery. Communication between the aorta and the œsophagus proceeds in general from the former. The first narrated case is a rarity, inasmuch as arteries generally remain uninjured in the midst of gangrene and suppuration. The vomiting of arterial blood is certain proof of some artery having been opened into; but where, from circumstances, the blood has been long exposed to the actions of the acids of the stomach, the case may be mistaken and treated for one of venous hemorrhage (*Melæna*), a matter of small moment compared with the reverse. Arterial hemorrhage beneath the duodenum, and above the sigmoid flexure, cannot be diagnosed on account of the changes undergone by the blood. In certain cases, blood from the lungs may be swallowed and then vomited in considerable quantity, proving a fresh source of error in the diagnosis, only to be corrected by the history. It is, therefore, our duty always to treat the vomiting or passing by stool of black blood as proceeding from venous hemorrhage, and the vomiting of arterial blood as hemoptysis, without paying too much regard to the possibility of both proceeding from the rupture of some large artery.—*Zeitschrift für Rationelle Medizin*, III. Bd. V heft.

SOURCE OF FALLACY IN TESTING THE URINE FOR SUGAR. By Dr REES.

DR REES has pointed out the fact, that the dark colour produced by boiling

the suspected urine with caustic potash (Moore's test), is not satisfactory, unless the purity of the potash be first ascertained. He was led to this knowledge by having failed to detect sugar in a specimen of urine said to be diabetic, when it occurred to him that the dark colour met with by the party sending the urine might be due to the presence of lead in his potash, which was found to be the fact by testing it with the hydro-sulphuret of ammonia.—*Medical Gazette*, April 2, 1847.

ACUTE TUBERCULIZATION OF THE LUNGS. Death in twenty-one days from first symptoms. By MR FIELD.

MR FIELD exhibited portions of both lungs taken from the body of a girl, fifteen years of age; her history is as follows:—Until within a week of the time Mr Field first saw her, which was twelve days before her death, she had enjoyed tolerably good health, though never robust; she had never suffered under any pectoral symptoms, neither cough, short breathing, nor pain of chest. She had never menstruated. On examination she complained of slight pain under the sternum, and in the region of the heart; percussion elicited unsatisfactory sound beneath both clavicles, and beneath the right there was slight crepitation, the respiration was rather hurried, and the expansion of the chest imperfect; her pulse was quick and weak; no expectoration; her general appearance was that of a person labouring under serious disease, which, combined with the physical signs, led Mr Field to consider the case as one of acute tuberculization. The last six days of her life she was very feeble. On dissection, both lungs were found minutely studded with miliary tubercles, all small and of uniform size. The specific gravity of both lungs was so much increased that they sank rapidly in water, and showed but little tendency to float in a saturated solution of alum and nitre; there was slight thickening of the mitral valve, though not sufficient to render it incompetent for its office; the liver was enlarged and fatty; the brain and kidneys were not examined. The period within which the disease proved fatal was, as nearly as could be ascertained, twenty-one days. Mr Field remarked on this case as illustrating a form of disease, rapidly and apparently certainly fatal, and alluded to similar cases that have been placed on record by Louis, especially one case where the duration of the disease was as nearly as possible the same. Mr Field then alluded to the question as one of much interest,—whether this acute form of tuberculization was to be considered as the effect of inflammatory action or not? and gave as one reason against such a view, that in acute tuberculization both lungs are almost invariably affected, whilst double pneumonia is comparatively rare.—*From Transactions of Bath Pathological Society*, Jan. 4, 1847, in *Provincial Med. and Surg. Journal*, June 16, 1847.

CALCAREOUS DEPOSIT IN CEREBELLUM; SUDDEN DEATH WITHOUT PREVIOUS SYMPTOMS OF AFFECTION OF THE NERVOUS SYSTEM. By MR SKINNER.

MR SKINNER exhibited a portion of the cerebellum of a middle-aged woman, in whom there had not appeared any peculiar symptoms referrible to the nervous system; death took place suddenly and unexpectedly. On dissection, the brain was found much congested; the arteries exhibited extensive calcareous deposit; the portion of the cerebellum shown was so loaded with calcareous deposit, that there remained little appearance of nervous matter, except the form.—*Ibid*.

ON THE REACTION OF NITRIC ACID ON THE COLOURING MATTER OF THE BILE. By HEINTZ.

THE well-known reaction of nitric acid on the bile being confined to the colour—
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ing matter, the defect of it shows merely the absence of that matter, while the rest of the biliary constituents may be present. The reaction of sulphuric acid and sugar, discovered by Pettenkofer, relates to another constituent of the bile, namely, the bilin of Berzelius, or the acid of the crystallizable soda-compound. There may be many cases in which the former reaction is wanting, while the latter is distinct. And the contrary may also occur, as is shown by the fact, that Heintz never succeeded in producing any alteration on icteric urine by the new method, whereas the old method never failed of effect. It should be observed, however, that though in a great number of cases nitric acid which had been boiled and cooled again, and which was, therefore, free from nitrous acid, gave rise to the usual changes; yet that it occurred more than once to Heintz, that pure nitric acid failed to produce any change whatever on icteric urine or the bile itself, while the fuming nitric acid did distinctly afford the reaction in question. To obtain perfect certainty, therefore, as to the absence of biliary colouring matter in any fluid, we should use the diluted fuming nitric acid. In one specimen of icteric urine, which, when recent, was not at all changed by pure nitric acid, the usual alterations took place by allowing it to stand for a short time. It is not unlikely that the reaction took place, in this instance, owing to the air replacing the nitrous acid by its power of oxygenation. If this be the truth, it follows that, whenever the colouring matter of recent bile reacts with pure nitric acid, it must first have undergone a metamorphosis by the agency of the air. With a specimen of bile which undergoes no change by pure nitric acid, we are possessed of the means of detecting the slightest trace of nitrous acid in nitric acid.—*Muller's Archiv.* 1846, p. 399.

In reference to the violet red colouring produced, according to Pettenkofer, on the extractive and colouring matter of bile by diluted sulphuric acid, Van den Broek has shown (*Holländ. Beiträge*, Bd. I. H. i. p. 100), that the dilute sulphuric acid is the sole agent, and that the sugar is not concerned.

SULPHURIC ETHER IN INTERMITTENT FEVERS. By CHALLETON OF GANNAT.

DR CHALLETON has had great success in the treatment of intermittent fevers, by administering half a coffee-spoon of sulphuric ether in a glass of sugared water, either at the moment of the first shiver, or at intervals of every four hours on the day preceding that of the attack. The efficacy of this treatment is confirmed by several practitioners in the neighbourhood of Gannat.—*Gazette Méd. de Paris*, 21st August 1847.

ON THE PRESENCE OF FIBRINE IN THE URINE. By DR HEINRICH, Bonn.

THE author describes four conditions in which fibrine occurs in the urine: 1st, The urine coagulates spontaneously, separating into a serous and an elastic solid portion. 2d, Worm or vermicelli like coagula, are evacuated along with the urine, as Seeger, Morgagni, Kelluer, and Thomasius have observed. The author considers that these coagula come from the kidney, and are moulded during their passage through the ureter; they cannot come from the bladder, because exudation within it shows no disposition to put on such forms, probably from the same well-known causes that induce exudation in pleuritis, arachnitis, &c., to assume flattened forms corresponding to the exuding surface. 3d, It is frequently found in a loose aggregate of very fine granules or molecules; this he considers a sign of qualitative disease of the fibrine. 4th, Finally, the author informs us that the microscopic moulds of the Bellinian tubes, occurring in the urine during irritations of the kidneys, are fibrinous, though tubes containing albumen, fat, and uric acid, are likewise found in the urine in similar diseased conditions.—*Schmidt's Jahrbücher.* 1847, No. 8.

DR MADDEN OF TORQUAY ON COD LIVER OIL IN PHTHISIS PULMONALIS.

DR MADDEN's experience of cod liver oil is extremely favourable. He has seen the most marked improvement follow upon its steady employment; the cough becoming less troublesome, and the expectoration less copious; the appetite better, and the emaciation arrested, and even a decided and manifest increase of flesh taking place in many cases. That it will *cure* consumption he does not say, but that it is most efficacious in bringing about a more healthy general condition of body, in which nutrition is carried on more satisfactorily, and the morbid deposition is for a time at least greatly checked, he feels himself warranted in affirming, from the result of many trials, both in his own practice, and in that of his friend and partner, Dr Battersby. Patients soon cease to complain of its nauseous taste; and when care is taken not to give it in large doses at first, he has rarely seen it disagree with the stomach. He prefers, if possible, giving the oil unmixed—simply floating on a little water, or aromatic water; but when there are objections to this, it may be administered as an emulsion, rubbed up with a *pâte de guimauve*, and flavoured in any way that is most palatable. An emulsion with liquor potassæ, is, he thinks, objectionable; the properties of the oil are changed, and it is far from certain that its efficiency is not also damaged.—*Medical Gazette*, September 17, 1847.

CASE OF POISONING, FROM SWALLOWING PERCUSSION CAPS. By T. W. FOSTER, M.D., of Keene, Jessamin County, Ky.

"Not long since, I was called in great haste to attend an infant, *æ*t. 14 months. Upon entering the room, I was informed by the parents that they had observed their child, about two hours previous to my visit, playing with a box of percussion caps, and they supposed she had swallowed some of them, as signs of acute suffering were exhibited soon after.

"The little patient appeared to be sinking very fast. The eyes had a hollow, glazed appearance; there was great heat in the epigastric region, and coldness of the extremities; there had been eight or nine discharges from the bowels in an hour, and her general aspect denoted approaching collapse. Before my arrival free emesis had been produced by some domestic remedy, yet I continued the vomiting by administering ipecac. and large draughts of warm water (of which the patient greedily drank), with the hope of discharging at least a portion of the offending matters. The discharges became so debilitating, however, that I threw up an injection of eight drops of laudanum, suspended in starch mucilage, and immediately afterward gave a large dose of calcined magnesia. An alkaline purgative was selected for the purpose of neutralizing any acid which might be found in the stomach or intestines, and thus prevent any chemical change in the copper. In the course of an hour, the child became perfectly composed, and fell into a pleasant slumber, though it had previously suffered excruciating pain, attended with spasms. Dr Spilman, the family physician, now took charge of the case, and applied counter-irritation to the abdomen. On the next day four caps were discovered in the *fecal* matter, which were found to be devoid of their fulminating powder. The child is now enjoying very good health."—*Philadelphia Medical Examiner*, June 1847.

ASCITES SUCCESSFULLY TREATED BY INJECTIONS CONTAINING IODINE. By Dr LERICHE of Lyons.

THE patient in the following case was a girl of seventeen, of feeble constitution, who had menstruated regularly from the age of fourteen. The abdomen began to swell after a slight affection of the chest, unattended by any constitutional affection. Diuretics and purgatives were employed without effect. She had now been affected with dropsy for fourteen months, when M. Leriche undertook the

treatment. The general health was still unaffected; there was considerable swelling of the abdomen, and the lower extremities were somewhat infiltrated. After drawing off the dropsical fluid, he injected into the cavity of the peritoneum between nine and ten ounces of a fluid containing an ounce of tincture of iodine and a drachm of hydriodate of potassa. Not so much as the half of this fluid could be made to issue again from the abdomen by pressure. During the succeeding night there were some pains of the abdomen, some tympanitic distension and borborgmi; on the third day the urine was abundant, the distension of the belly had diminished. During the next few days there was debility and want of sleep, the evacuation of urine continuing abundant. Eight days after the operation the appetite and strength began to return, and a complete cure was quickly established.—*Annales de Thérapeutique*, August 1847.

ON THE SUPPOSED TUBERCULAR CORPUSCLES FOUND BY GRUBY IN THE SPUTA OF THE PHTHISICAL. By PACINI.

In a work published some years ago by Gruby, entitled "*Morphologia Fluidorum Pathologicorum*," there is a chapter on the sputa of the phthysical. He there describes certain tubercular corpuscles under the name of lenticular spheres (*sphères lenticulaires*), the presence of which he regards as characteristic of the sputa in tubercles of the lungs. Pacini affirms, that these bodies are nothing else but grains of starch derived from bread used at meals. Pacini further states, that what Gruby describes as "*fibræ cylindricæ flavæ linei transversalibus notatæ*," are nothing else than the fibres derived from the meat which the patient has lately eaten. He says, no such fibres can be derived from the lungs; and that the only fibres found occasionally in the sputa in phthisis are cellular and yellow elastic fibres, which result from the destruction of the pulmonary tissue.—*Annali Univ. di Medicina*, and *Archives Générales de Médecine*, August 1847.

ACUTE GLANDERS IN A FEMALE MATTRESS-MAKER. By DULCOS.

A woman, aged forty one, a mattress-maker, was received into the Hospital Necker at Paris, in June last year, who had been ill ten days, affected with the following symptoms. She had been first seized with a neuralgic pain of the right side of the head, which was successfully treated with preparations of belladonna; then with an intercostal neuralgia, quickly followed by an acute sciatic pain on the left side, particularly severe in the calf of the leg. This pain was quickly removed by a blister sprinkled with morphia—then the left knee became painful, the pain being soon accompanied with redness and swelling, and now some delirium arose. On inquiry it was found, that though her husband was a hackney coachman, he was just dead of phthisis, which had prevented him from following his occupation for a year—that he had not been visited during his illness by one coachman, and she herself had never any thing whatever to do with horses. But, as a mattress-maker, she was accustomed to card wool and to prepare horse-hair. The day after her entrance into the hospital she was found in the following state,—fever acute; pulse frequent and rather small; skin dry and hot; face slightly jaundiced; look unsteady; eyes injected; correct answers when the attention is roused; much delirium in the night; redness, tumefaction, and excessive pain of the left knee; a red fluctuating tumour, the size of an olive, on the left hand, near the lower extremity of the second metacarpal bone. On the third day, the twelfth from the commencement of the attack, the delirium continued, the skin was dry and somewhat jaundiced; on the middle of the right cheek there was a vesicle with an inflamed base of a violet-colour, resembling a malignant pustule; some red spots, and some small buttons, like those of small-pox, spread over the thighs and other parts of the body, also some small hard tumours, with a

violet tint of the skin. On the thirteenth day the pustule on the face had a carbuncular appearance, and the skin was gangrenous for a small space around; there were numerous pustules of irregular size on the face, eyelids, and extremities; no restlessness—profound stupor. Next day she sank.

On the post-mortem examination there were found on the mucous membrane of the nasal fossæ some superficial ulcerations; in the knee-joint a large quantity of pus; in the sub-cutaneous tumours pus mixed with blood; the gangrenous spot on the face was but superficial beneath; the cellular tissue was infiltrated with a yellowish serosity to a considerable depth. The other parts of the body were not examined. A lancet was charged with some sanious matter from the gangrenous spot and adjacent abscesses, with which a horse, between eight and nine years old, affected with an ankylosis, but otherwise healthy, was inoculated. This horse died on the seventh day after, of a well-marked acute glanders.—*Journal de Médecine*, July 1847.

On this case it is remarked in the *Archives Générales de Médecine* (August 1847), that this case of glanders must either have arisen spontaneously, or have originated from the horse-hair which the woman was in the custom of preparing. It is there affirmed that the malignant pustule is known to be transmitted to those who work in hair; and that the hair brought from Buenos Ayres to Paris, notwithstanding the length of the voyage, has this effect. And, it is added, that glanders may not improbably be communicated in the same manner.

ATROPHY OF ONE LUNG, WITH CICATRIZED TUBERCULAR CAVITIES.

By M. PAYAN of Aix.

A WOMAN, twenty-six years of age, went into the Hotel Dieu, at Aix, on account of a slight illness. She had been in the same hospital, three years before, with all the symptoms of confirmed tubercular phthisis,—namely, hollow and deep cough; abundant expectoration, at times tubercular; hectic fever, with circumscribed redness of the cheeks, &c. After remaining three months in the hospital, she returned home with no other prospect than of quickly sinking under the disease. Nevertheless the symptoms, after a time, began to subside, and at length she so far recovered her health and strength that she again went to service, having then no remains of her former complaints, but some constraint in breathing when she made an unusual exertion. When she went into the hospital a second time for a complaint of the stomach, there was no symptom of affection of the lungs. A few days after she was seized with influenza, which then (January 1837) prevailed severely in the town. Before the evening of the day on which she was attacked, the respiration had become much oppressed, with sense of approaching suffocation; and, in spite of every means of relief that could be devised, she died in the night with all the indications of asphyxia.

At the post-mortem examination the body presented an ordinary degree of embonpoint. The left side of the chest was perceptibly less prominent than the right. There was nothing unusual within the skull. Between the right and left lungs there was a remarkable disproportion,—the right being well developed, and filling its proper cavity; while the left, reduced to a very small bulk, was lost in the left thoracic cavity, being concealed by much laminated tissue. On minute examination the left lung was found to be much atrophied, and quite impervious to air. Its texture was hard and fibrous-like, somewhat resembling that of the supra-renal capsules. On cutting into it there was found a number of cavities, more or less extensive, appearing to communicate with each other. These were true tubercular cavities, smooth on their inner surface, and altogether empty. The right lung was simply in a state of congestion.

OBSERVATIONS ON HYSTERIA. By M. E. MARCHAND of Sainte-Foy.

THE following are the conclusions to which M. Marchand comes in a memoir on Hysteria, read before the Royal Academy of Medicine at Paris:—

That aglobulia denotes a diminution in the blood globules, as hyperglobulia signifies an increase of the same, or plethora. Aglobulia is a very common morbid state. The blood globules—the medium proportion of which is 125 in a thousand parts of blood—may fall to so low a figure as 24, as Andral has remarked. M. Marchand has found it as low as 43. Aglobulia has for its discriminating mark a disturbance of the nervous system. This disturbance of the nervous system is, in general, the more decided the more complete the aglobulia is. The disorders arising from aglobulia may affect either the nervous system of the functions of relation, or the nervous system of the vegetative functions. Under the first head fall convulsions, paralyses, hysteric lethargies, hysteric aphonia, hysteric asthma; disorders of the organs of sense, as amaurosis and double vision, and tinnitus aurium. Under the second head come various chlorotic phenomena, nervous palpitations of the heart and great vessels, venous and arterial murmurs, failure of the complexion, certain disorders of the uterine function. In general, the diseases termed nervous depend on aglobulia, or at least always coincide with it. Aglobulia is more frequent in women than men. The majority of women have a slight degree of aglobulia, which explains their nervous susceptibility, their exalted sensibility, and their many neuralgias. Sydenham affirmed that two-thirds of the female world suffer from hysteric symptoms. Women have a smaller proportion of blood globules than men; they are more subject to nervous maladies. This smaller proportion of the blood globules in them, may be ascribed to their periodical hemorrhage; for all hemorrhage diminishes the blood globules. The menstruation, then, has the effect of producing in females their peculiar nervous susceptibility.

The nervous symptoms of the first months of pregnancy depend on the diminution of the blood globules. It has been observed, that nervous diseases have a tendency to subside after the critical age. It is equally an observed fact, that after this period of life the blood globules acquire an increased proportion, owing to the cessation of menstruation. In general women bear blood-letting and spare diet ill, because these means favour the diminution of the blood globules, and exalt the nervous tendency. It has been said, with truth, that chlorosis is the basis of pathology in the female constitution. Hysteria is the aglobulia of females from twenty-five to fifty-five years of age; chlorosis is the aglobulia of young girls, and often of young men; hypochondriasis is the aglobulia of grown men. Proceeding on a different train of ideas, Sydenham conceived there is no difference between hysteria and hypochondriasis. A great number of chronic maladies,—but, in particular, syphilis, tubercle, cancer, and intermittent fevers,—give rise to aglobulia, which is then symptomatic. Spare diet too long persevered in, too frequent venesections, produce aglobulia, and consequently nervous excitability. This happened very often at the time when gastralgias were mistaken for forms of gastritis; the means adopted as methods of cure prolonged the disease.

Antispasmodics sometimes calm nervous excitement, but never cure it. To produce a radical and complete cure, the blood globules must be brought back to their normal state; and the only mode of accomplishing this end is by animal diet, wine, iron, bitters, exercise in the open air in the sunshine, residence in the country, sea-bathing, &c. When aglobulia is symptomatic, the same means tend to produce a cure. In the third stage of syphilis, and in old intermittents, it is found beneficial to unite animal diet, wine, iron, and bitters, with the antisyphilitic and febrifuge remedies. If the alteration of tissue be incurable (tubercle and cancer), iron may relieve but cannot cure.

An increase in the proportion of the blood globules (140, 150, 170 per millimetre), or hyperglobulia, impairs the nervous sensibility, and leads to apathy. Those affected with hyperglobulia have diseases in general peculiar to themselves,—gout, gravel, cerebral hemorrhages. Venesection, spare diet, vegetable nourishment, alkaline waters, &c., are the best means of curing hyperglobulia.

To conclude, the functional derangements of the vascular system, and the functional derangements of the nervous system, arise in inverse proportion.—*Gazette des Hôpitaux and Annales Médico-Psychologiques*, September 1847.

ON DORSO-LUMBAR MYELITIS.

DORSO-LUMBAR myelitis is often confounded at its commencement with lumbago, which is a muscular or rheumatic affection usually of no great importance. To avoid this mistake, besides the dorso-lumbar symptoms, attention must be given to the functional state of the lower extremities,—of the bladder, of the rectum, and of the organs of sex. In every case of supposed lumbago, inquiry should be made if there be any debility or tingling of the lower extremities, or any cramps or other muscular disorders, isolated or combined, transitory or permanent, according to the seat and intensity of the myelitis. It is to be remembered that this disease, like pleuritis, may be in one circumscribed spot. The consequence of a failure to detect the disease at its commencement, is the production of mechanical alterations,—thickening, adhesions, congestions, atrophies, and ramollisements. The treatment followed at present in Paris, in this disease, is not energetic except in the first stage. Venesection is practised when there is fever (which is rare), and if the patient be plethoric. More frequently cupping and leeches over the seat of the disease are applied, to an extent and frequency proportioned to what the patient can bear; with these means are conjoined frequent baths, diluent drinks, and restricted diet. After these first remedies, recourse is had to large blisters over the affected part, the baths being continued; and, if these means fail, they recommend the patient to heaven, and make four large cauterizations on his back, while they send him to breathe country air,—the cauterization being, in Parisian practice, the *ne plus ultra*, the last effort of medicine which is to pump the disease out of the osseous canal in which it has fixed itself. To the cauterization there is little limit; it has sometimes been kept up for a quarter of a century with no effect. M. Rayet, who seems to be becoming sceptical as to the benefits of revulsion, prefers the external and internal use of turpentine, to which he adds sulphurous and vapour baths. In our opinion the common Parisian treatment is very incomplete;—it is incomplete, for it is seldom that the patients come into the hospitals at a time when the treatment of acute inflammation can be applied with advantage; and, as to cauterization, it is altogether deceptive and absurd, resting as it does on the chimerical doctrine of revulsion.

The treatment proper for such affections is that in use for many years in the Italian school,—namely, by spinal hyposthenics. Against these there is a great prejudice in France. It is held little short of blasphemy to say to a French physician, that a myelitis, not yet passed into the mechanical state, can be cured by combining with the ordinary antiphlogistics such remedies as the following,—sulphate of quinine in large doses, cantharides internally, iodine, nitrate of potass, and various mineral waters.—*Editorial Remarks—Annales de Thérapeutique*, August 1847.

MIDWIFERY, AND DISEASES PECULIAR TO WOMEN.

ON THE PRIMARY CAUSE OF PUERPERAL FEVER. By Dr SCANZONI of Prague.

DR S. commences by stating that the wound produced on the internal surface of the uterus by the detachment of the placenta during parturition, is universally regarded as the peculiar cause of puerperal fever. He considers this view too exclusive, and in opposition to it mentions two or three cases where the symptoms of puerperal fever had shown themselves before labour had commenced, and continued after it to a fatal termination, and there was no hemorrhage nor putrefaction of the placenta to indicate that that organ had been detached before the proper period of labour. Dr S. then believes that “the seed of the disease is sown before the commencement of labour;” accordingly he seeks for the germ of the disease in the blood, and, as the peculiar condition of the blood of a puerperal

is formed from that of a pregnant female, he conceives that the special predisposing causes of puerperal fever exist in the composition of the blood. In proof of this, the author mentions that in those cases which came under his own observation, where that composition of the blood which is peculiar to pregnant women was totally prevented from taking place, or was modified by the presence of some other dyscrasia, the patients were never attacked with puerperal fever, although exposed in an eminent degree to that combination of circumstances which is supposed to induce it; and on the other hand, when, during pregnancy, females are attacked with any disease depending on hyperinosis of the blood, such patients are very liable to suffer from puerperal inflammations, accompanied by copious fibrinous exudations. The author refers to the writings of Helm, Kiwisch, Rokitsky, and Engel, showing that the blood of puerperal females is in a state of hyperinosis, *i. e.* containing an excess of fibrine. The author saw that strong healthy women, in whom this state of hyperinosis of the blood natural to pregnancy had attained its full development, suffered during a severe epidemic under that form of puerperal disease which gave indications of an excess of fibrine in the blood, *viz.* endometritis or peritonitis, accompanied with copious fibrinous exudation, such as is met with in pleuritis and pneumonia.

Physiologists are as yet unable to explain how this augmentation of fibrine is produced. Fibrinous deposits are frequently found in or upon the placentas of mature children, but very rarely has the same alteration been found in the case of premature fœtuses. These deposits are mostly confined to the two last months of pregnancy, and their existence may, according to our author, be accounted for by the active interchange of material which, toward the end of pregnancy, takes place between the maternal and the fœtal blood; the former abounds in fibrine, and if the consumption of this element on the part of the child be diminished, whilst the supply afforded by the mother remain the same, fibrinous deposits must take place, and these probably on the inner surface of the uterus: this is a fact our author has been unable to ascertain, as the inspection of the bodies of women dying during pregnancy are exceedingly rare. The fibrinous deposits in the placenta, however, according to Dr S., prove that the fœtus does not consume all the fibrine supplied to it, and they lend probability to the idea, that in consequence of the continually increasing supply of fibrine in the circulation of the mother, and the comparatively small consumption of that element on the part of the child, an abnormal accumulation of fibrine must be the result, constituting hyperinosis in the mother.

This fibrinous diathesis in pregnant females may pass into secondary dropsy or purulent deposit; secondary dropsy follows from the anemia gradually developed in consequence of the blood being generally impoverished by copious fibrinous exudation; the dropsical effusion frequently possesses a sero-purulent character.

Purulent deposits may take place in pregnant females, in consequence either of the primary formation of pus in the blood, or of its absorption from an ulcerating surface. According to Mulder, the tritoxyde of protein—or pyin, as some have termed it—contained in the buffy coat of the blood, and which is also more abundant in hyperinotic than in normal blood, has a great tendency to be transformed into pus; which confirms what we might *à priori* have expected, that pus may be formed in the blood, and that it need not necessarily be derived from some ulcerating surface of the body. This is also confirmed by those cases related by the author, in which all the symptoms characteristic of the formation of pus appeared some time before parturition, and were speedily followed thereafter by a fatal termination: it is further corroborated by the fact, that the effects of pyemia are so very frequently observed during puerperal epidemics, and so very rarely in sporadic endometritis or peritonitis; and also by the fact, that the constitution of the atmosphere about the patient exercises such a very powerful influence in originating the disease. To the objection that an impure atmosphere exerts its baneful influence as well upon the raw uterine surface as upon the blood circulating through the lungs, and that its local action upon either the one or the other of these organs may with equal truth be regarded as the primary

cause of the disease, the author replies with the fact—which is not unfrequently observed in lying-in wards—that among, say ten or twelve women in the same ward, one in whom labour has been perfectly normal and easy may speedily show all the indications of puerperal fever, with pyemia, and rapidly die; whilst another, in whom a wound really has been inflicted on the uterus by the forcible detachment of an adherent placenta, recovers, without a bad symptom, in a few days;—thus arguing, that the two females being, as far as regards the atmosphere, *in pari statu*, if the noxious influence residing in the air act only on the raw surface of the uterus, the latter patient should have been attacked with the disease, and not the former; but this not being the case, he regards it as a proof that the noxious properties of the atmosphere act with equal, if not greater power, through the lungs than through the uterus. This cannot be doubted, from what has been lately seen of the immense activity of various vapours administered by inhalation. The author regards pus in the veins as an extremely rare appearance, but as very common in the lymphatics; it is found in them either as a consequence of absorption from a distant part, or of inflammation of their proper tissue (lymphangitis). Among lymphatics of the uterus filled with pus, Dr. S. has found others filled with fibrinous exudation. The latter he regards as a proof that the pus contained in the former does not owe its origin to absorption from a distant ulcerated surface; but that the former also were, at a previous period, filled with fibrinous exudation, which only became purulent secondarily, under the influence of the suppurative process going on in the blood.

The conclusions which Dr Scanzoni arrives at are,—1st. That a rawness of the internal surface of the uterus does not constitute the sole and only predisposing cause of puerperal fever, but that this consists in a peculiar constitution of the blood. 2dly. That this peculiar condition of the blood is indicated by an increase in the quantity of fibrine. 3dly. When this increase becomes excessive in a high degree, it constitutes the primary cause of the disease; or it is, in short, in itself the “essence of genuine puerperal fever.” 4thly. The genuine disease is characterised by the fibrinous type; no other type is genuine, but can only be developed secondarily out of the fibrinous. 5thly. Hypinosis of the blood gives immunity against that form of puerperal disease accompanied by fibrinous exudation; it gives but little protection against the suppurative, and none against the typhoid form. 6thly. The hyperinotic form of the disease frequently, during epidemics, passes into the purulent or into the typhoid form. 7thly. The two last forms may be developed either primarily in the blood, or secondarily by the absorption of pus. 8thly. This absorption can take place from suppuration on the surface in the tissue, or appendages of the uterus, or from the placenta. 9thly. The cases which run the most rapid course are those which are the result of a primary disease of the blood; the less acute are the result of absorption.—*Prager Vierteljahrschrift für die pract. Heilkunde*, 1846. xii. Bd. S. 1.

ON THE INFLUENCE OF PERIODS OF THE DAY UPON BIRTHS AND DEATHS.
By PROFESSOR CASPER of Berlin.

IN a work containing a large amount of statistical data (*Denkwürdigkeiten zur Medicinischen Statistik und Staatsarzneikunde für Criminalisten und Aertze*, Berlin 1846), Professor Casper arrives at the following conclusions:—

1. The greatest number of births takes place between nine o'clock in the evening and six in the morning, whilst the smallest number occurs between nine in the morning and six in the evening.

2. The pains of labour commence most frequently between midnight and three o'clock in the morning, and most rarely between the hours of six and nine in the morning.

3. The influence of night is much more marked on the commencement of labour than on the epoch of complete delivery.

4. Among those births in which the labour has commenced during the day, the greatest number are male children, and the contrary.

5. The medium duration of labour is so much the more prolonged as the pains have commenced in the night.

6. The excess of births in the night over those in the day, is more considerable for still-born than for living children.

7. The maximum mortality occurs in the hours which precede mid-day, and the minimum mortality in the hours which precede midnight.

8. Considered generally, the mortality caused by inflammations, phthisis, and pulmonary apoplexy, is more considerable after mid-day ; that produced by fevers and exanthemata, a little before mid-night ; that caused by cerebral apoplexy, during the day ; and that produced by diseases of the nervous system, in the hours following midnight.—*L'Union Médicale*, 28th September 1847.

SPONTANEOUS RUPTURE OF THE UTERUS FROM THINNESS OF THE PARIETES.

By MR BARRETT of Bath.

Mrs — was taken in labour I believe at her full time. The labour appeared to be going on naturally, when suddenly symptoms of great depression occurred, accompanied with vomiting and extreme tenderness of the abdomen, and the patient sunk rapidly, dying a few hours after the first accession of the dangerous symptoms. On examination after death there did not appear to be any malformation of the pelvis, or disproportionate size of the child. She had borne several children, and her labours had been natural, though rather lingering. The cause of the rupture appeared to be, that one-half of the uterus, as the foetus increased in size, merely dilated, but it did not increase in thickness as it should have done ; so that, whilst the cavity of the uterus was of the size it ought to have been in pregnancy, the substance of the walls of one-half was little thicker than brown paper, the substance of the other half being of the proper thickness. It appeared, therefore, when labour came on, that the part of the uterus which was thus preternaturally alienated had been unable to resist the tension, and had given way. Nothing was discovered to account for this want of nutrition. The patient had generally enjoyed tolerable health. The true nature of the case, though suspected, was not ascertained till after death. So far there is much resemblance between this and the American case (last number of Month. Journ.), and they seem to establish the interesting pathological fact, that one side of the uterus may either congenitally, as probably was the case in the American instance, or from some morbid and subsequent, though unascertained cause, as in the instance I have mentioned, be wanting in that natural and increased degree of nutrition proper to pregnancy ; whilst the proper change goes on in the other half, just as one limb may either congenitally or subsequently be stationary in growth, whilst its fellow properly increases. But what explanation can be given of the want of communication between the vagina and the cavity of the uterus in the American case ? There is nothing in the statement of it which could lead to the belief that the obliteration of the os uteri occurred subsequently to impregnation, but every reason to make us conclude that the appearances described were normal. By what means, then, did that actual contact take place between the semen and ova which is considered essential to impregnation ?

To this question of Mr Barrett, the Editor of the Med. Gazette responds—We think that in the American case, the fact of pregnancy must be taken as affording sufficient proof that there was a communication between the cavity of the uterus and vagina. It may have been abnormally situated, and therefore overlooked.—*Medical Gazette*, October 8th, 1847.

ON TURNING BY THE HEAD. By CARL HELLER.

TURNING by the head is an old operation, which, according to the author, is at present far too seldom performed, as it is applicable in many cases where turning by the feet is practised to the sacrifice of the child.

Turning by the feet is undoubtedly, in a great number of cross positions, the only method which can be chosen, as in very rapid labours, narrowing of the pelvis, when the liquor amnii has been long discharged, complete absence of pains, &c. &c. But in many cases of crossed position, with prolapsus of the cord, or of an arm, or where the head is not found in the neighbourhood of the os uteri, the author has brought the case to a happy conclusion, turning by the head, with less danger to the child. He describes three cases, of which the third only is important as far as the practice he recommends is concerned. There was complete torpidity, and loss of power in the uterus. The child lay completely across, and was already putrid. The constriction of the os uteri rendered spontaneous delivery impossible, and did not allow turning by the feet, from the probability of producing rupture of the uterus.—*Würtemb. Correspond. Bl.* No. 34. 1846.

CASE OF ABDOMINAL PREGNANCY, SUPPURATION, AND EXTRACTION OF THE FŒTUS THROUGH THE ABDOMINAL WALLS. By DR DÜCKERT.

A COUNTRY-WOMAN aged thirty years, was the mother of three healthy children, which were born without artificial assistance, although with difficulty. She again found herself pregnant, when, on the 6th of November 1842, an ox thrust his horns under her clothes, lifted her up, and then let her fall on the ground. Three hours afterwards the author found the woman with a pale countenance; cold skin; small, frequent, and scarcely perceptible pulse; great anxiety, tendency to vomit, and becoming faint. The abdomen was very painful, with tenderness, swollen on the right side, and the fœtus could be very distinctly felt through the abdominal walls. The movements of the child, which had previously been very strong, had not been felt since the accident. A considerable quantity of blood flowed from the vagina. On internal examination he discovered the os uteri to be closed, and the vagina pushed to the right side. The fœtus could not be felt. The patient during her pregnancy had frequently suffered from pain in the abdomen, which she had never before experienced. Under the circumstances internal hemorrhage was supposed to have occurred, and there was ordered an oil emulsion, with cream of tartar and cherry water, and cold applications externally to the abdomen. The following day the pain was increased, and was more limited to a particular spot. Leeches were applied to it, and doses of calomel prescribed. On the third day the patient was much the same, and warm fomentations were ordered. An abdominal pregnancy was now diagnosed. The hemorrhage from the vagina became more and more watery, and continued three weeks; the pain in the abdomen continued to be more or less strong. On the 27th of November, an abscess was discovered in the abdominal walls, below the umbilicus. This was opened, and the incision enlarged, on perceiving that the fœtus presented at the opening. It was now extracted, together with the putrid placenta. The cord still adhered to the abdominal walls, but was afterwards separated by ulceration; no very important hemorrhage occurred. The wound was brought together by sutures. The discharge at first was considerable, ichorous and very foetid, but was soon succeeded by that of a good pus. The accompanying fever, in the weakened state of the patient, was of an adynamic, putrid character, but under the use of stimulants, tonics, and chalybeates, gradually disappeared. By the end of the year the wound was nearly closed, so that the patient could leave her bed, and walk without difficulty.—*Casper's Wochenschrift*, 10th Juli 1847.

Part Fourth.

MEDICAL NEWS.

REMARKS ON EGYPT AS A WINTER RESIDENCE FOR INVALIDS. By WALTER BAIN, M.D., late Physician's Clerk, Royal Infirmary, Glasgow.

OF late years it has become a frequent practice among medical men to recommend to certain classes of invalids a winter residence in Egypt, in preference to other, and formerly fashionable places of resort; and on account of the interest in Eastern affairs, excited by the events of the last decade, and the facility with which it may be reached, this interesting country now forms the chief part of the tour generally performed by the travelling world.

In making choice of Egypt as a residence for a patient during the winter months, the physician is guided almost solely by popular report regarding its climate; for, though numerous works have lately appeared concerning the East, they contain but few observations on the climate and physical geography of the countries described,—certainly none sufficiently extensive or accurate to enable us to institute a comparison with other places.

In the last edition of his excellent treatise on climate, Sir James Clark has stated all that as yet is known with certainty on this subject, and a single glance at the chapter on Egypt in that work, will show how much information is still to be desired on the climatic peculiarities of this country.

In September 1846 I accompanied a young gentleman, in delicate health, to spend the winter in Egypt, and having had my attention directed to the climate, and its influence on disease, I deem it my duty to communicate my observations to the profession, trusting that, though limited, they may add somewhat to what is already known, and induce those who may have better opportunities to extend and confirm them.

In Cairo, lat. 30° N., during October and November the thermometric range was as follows:—

Thermometer.				Diurnal Range.		
Months.	Max.	Min.	Med.	Greatest.	Least.	Med.
October,	78°	67°	73°	10°	4°	7°
November,	74	60	64	11	2	5

During this period rain fell once, a slight shower of a few minutes duration. In October the waters of the inundation were rapidly subsiding, and in consequence there was a considerable quantity of moisture in the air; but by November the river had contracted within its banks, the water had disappeared from the ground, and the air become perfectly dry. Until November there was almost no wind, but with that month northerly breezes set in, and continued without any change during the entire winter. The winds exhibited a good deal of periodicity, blowing strongly during the day, thus moderating the extreme heat, and suffering a diminution of intensity, frequently amounting to complete cessation at sunset. During November the sky was beautifully clear, the air dry and elastic. The heat of the day was considerable, but very agreeable. The nights were mild, and dews of very rare occurrence.

At the end of this month we commenced the voyage up the Nile, and as a general rule, it was observed that in proportion as we sailed farther south the mid-day temperature increased, while that of the night diminished. Rain fell once

during the voyage—of ten weeks' duration—and dews were as unfrequent as at Cairo. The evenings were very mild, but as the night advanced the temperature sunk so much, that we felt the air excessively cold. At Thebes, lat. 26° N., I had an opportunity of observing the thermometer for a week.

Thermometer.			Diurnal Range.		
Max.	Min.	Med.	Greatest.	Least.	Med.
81°	68°	77°	12°	5°	9°

In Nubia the same phenomena were noticed, the only difference being, that the maximum and minimum temperatures were, the one greater, and the other less, than at Thebes.

Our downward voyage commenced January 4th, 1847; and every day we felt the heat less, and the winds colder. At Thebes, for three days, I find that the thermometer ranged as follows:—

Days.	8 A.M.	Noon.	8 P.M.
January 15	64°	76°	66°
„ 16	62°	75°	64°
„ 17	63°	79°	67°

The wind blew strongly against us during the entire voyage down the river, and was so cold that we could not expose ourselves to it without protection; and, when we reached Cairo, the weather was disagreeably cold for several weeks. Rain fell five times during February; the sky was cloudy, and the sun obscured. The thermometric range for the month was as follows:—

Max.	Min.	Med.
76°	57°	61°

The maximum temperature stated in the table, was reached during the last days of the month, about which time the weather began to improve, and in a short while Cairo became as agreeable as before.

The chief peculiarity of the Egyptian climate is its dryness, proved, by the foregoing observations, by the united testimony of travellers, but, above all, by the monuments on the banks of the Nile, the sculptured surfaces of which have retained, during a period of several thousand years, all the sharpness and polish of their finished state, and by the brightness of the colours which adorn the interior of temples and tombs, for ages exposed to the influence of the atmosphere. Moisture is found in the air only during the subsidence of the Nile, and even then it is rapidly abstracted by the arid sands of the desert, which hems in the valley of the river on every side. The heat of the summer months is said to be very great; but during the five months of the year spent by us in the country, it was not only tolerable but very pleasant. In spring, the Egyptians are annoyed by the prevalence of a hot south-east wind, similar to the sirocco of Malta and the Italian coasts, and, like it also, trying both to natives and strangers. It is called by the natives “Khamseen,” or fifty-days wind, and usually sets in about the middle of March; but it is generally

heralded by pilot blasts for several days before its appearance. During this season, the diseases peculiar to Egypt prevail with greatest intensity ; but, if we except it and the period during which the Nile is subsiding—when intermittents abound and plague usually begins—this country is said to be remarkably favourable to health and longevity.

The climate of Egypt is liked by most Europeans, and many have chosen to live in Cairo in preference to any other place, solely on account of the atmosphere. Most of those who sojourned there during last winter, on account of health, derived much benefit, and several, who had formerly wintered in other places, gave it a decided preference. My friend improved very much during his stay. Pulmonary symptoms, which had begun in Scotland, disappeared on his arrival in Egypt, and were never afterwards thought of ; and chronic bronchitis, with a cough which harassed myself incessantly, gradually became less severe, and, ere I left the country, had quite gone.

Egypt offers to the invalid the advantages of a warm and dry winter residence, and, though the temperature is not so equable as that of some other places, it possesses the additional recommendation of being, perhaps, the most interesting country in the world, affording thus, with bodily ease and comfort, mental recreation of the highest order.

The invalid who intends passing the winter in Egypt should not land in that country before the middle or end of November, and, on reaching Alexandria, he ought to start for Cairo without delay. As in almost all cases he must leave England in September, or early in October, he ought to arrange his voyage so as to delay somewhat in a latitude north of the Mediterranean. Any route may be chosen. Through France to Marseilles, or Germany to Trieste, are now common highways to the East ; but, on the whole, I think the best way for the invalid to adopt is to leave Southampton for Corunna, thence down through Spain and Portugal to Lisbon or Cadiz, from either of which ports he can find steamers to transport him to Malta and the Levant. This I deem a very agreeable route. The climate of Spain is finer than that of Germany, and even than that of France during the month of October ; and, as it is customary to return through Europe, a passing glance at the Peninsula may thus be obtained without additional trouble or expense. Once in Cairo he should be in no hurry to proceed up the Nile. The short winter experienced by the Cairenes is during January and February ; so these months ought to be spent in Nubia, the most southerly region to which a voyage can be made with comfort.

Cairo and its environs afford sufficient amusement for a month, and by the end of December, the Nile voyage may be undertaken. Two months are usually spent on the river ; by the invalid the voyage should be prolonged to the middle of February, by which time the weather in Cairo is generally agreeable. The error generally committed by those who undertake the ascent of the Nile is that of starting too soon ; going up into a very warm climate, and then descending to a latitude in which considerable cold is felt in winter. This was the mistake into which we fell, and I cannot too seriously warn all who are ailing, carefully to avoid it.

Before leaving Britain, the invalid should provide himself with clothing of two kinds ; one, the ordinary attire of Britain, the other, such as is worn in hot countries ; every thing else may, and ought to be laid in at Alexandria or Cairo. In either of these towns every article may be provided for making the voyage, not only in comfort, but luxuriously ; and in either place servants may be got without difficulty. To take any thing from England or Malta involves very great expense, and to hire servants in either place is not only very expensive, but is the source of more annoyance than can be conceived ; for though the Egyptian servants are perhaps the basest rascals on the face of the earth, they are not so accomplished in villainy as the Italian or Maltese.

Travelling is thoroughly understood by the Arab dragomen, and it may be performed as comfortably in Egypt as in England ; no fears, therefore, of pri-

vation or roughing need deter any one from making the voyage. The hotels in Cairo and Alexandria are very good, and for a family, or such as study economy and quiet, houses may now be procured in both towns. The English language is alone sufficient for the East, because most of the servants understand it ; but, if Italian be spoken, the traveller's enjoyment will be greatly increased.

Egypt should be left about the beginning or middle of March, circumstances and the route to be taken on leaving, determining the precise time. If it be intended to return to England, the invalid should delay in Cairo or Alexandria till the middle of March, when he may leave for Malta with safety ; but he ought not to proceed further North until the middle of April, unless he goes to Italy, and travels slowly through the Neapolitan and Roman states. Most persons who visit Egypt, however, make the tour across the desert to Syria, and, if circumstances permit, I would strongly advise all to perform it. The desert is full of interest ; very different indeed from the idea usually entertained of it by Europeans ; and as it forms the highway to a land associated with the earliest thoughts of every Christian, a journey through its wild and cheerless wastes, greatly enhances the delight experienced at the first glimpse of the hills of Judea. To the invalid it possesses still other attractions. The climate is finer than that of Egypt, and the change of life there undergone, and the exercise which the traveller is obliged to take, are frequently of far more service in restoring health than any length of mere residence in any climate, and when improvement has already begun there are few places better calculated to advance it.

The journey across the desert may be accomplished as comfortably as the voyage on the Nile, and after a day or two without the slightest fatigue. Last year numerous parties, and among the number many ladies, performed the trip, and all, without exception, bore testimony to the pleasure and benefit they derived from their wanderings.

On leaving Egypt for Syria, there is a choice of routes, one a journey of thirty days, by Mount Sinai, Akaba, and Petra ; the other, of twelve days, by El Arish and Gaza, on the south-east coast of the Mediterranean. To the invalid, I advise the longest, because, if improvement can be effected at all by climate, it must take place under the desert sky, and besides, it is the most interesting route. If he choose it, he ought to leave Cairo by the beginning of March, and thus enter Syria by the end of the month, leaving ample time for making the tour of Palestine, and being ready to sail from Beyrout to Marseilles by the steamer of the last week of April or first of May. By going to Marseilles, voyagers from the Levant now obtain free pratique in twenty-four hours, a comfort which those who have performed quarantine can appreciate. From Marseilles to England is a journey of four or five days, and in a few months, when part of the railways now constructing have been finished, will be much shorter.

In bringing the above detached notices to a close, I would only express a hope, that no imaginary fears may deter medical men in recommending, or patients in adopting the East as a residence and travelling ground during the winter. The annoyances to which the traveller is subjected are not very great, and are every day diminishing, and the expense of living and travelling is not greater than in other countries.

THE INDIAN CHOLERA.

THERE can be little doubt that the Asiatic cholera is once more approaching our shores, and following the same north-west passage across Europe as it did in the epidemic of 1831-2. It is at present at Kiev, a Russian university town, about six hundred miles south-west of Warsaw. The cry has already been pretty extensively raised of what is to be done ? A medical congress, boards of health, accurate reports of past cases, and individual nostrums, have

been proposed. Yet what can we expect from such means? What we require is a knowledge of the disease—its causes—the nature of the changes occurring in the blood—the mode in which these operate upon the secretions—the composition of these latter, and so on. The union together of a number of individuals to constitute a board, all of whom are equally ignorant of these important points, is not likely to furnish us with any thing very positive with regard to the treatment. This machinery was put into very extensive application fifteen years ago, and what do we know of its treatment? Forty-two thousand individuals died of the disease—and what do we know of its pathology? The answer to both these questions is, nothing—absolutely nothing. As an empirical system of investigation and practice, then, has lamentably failed, our efforts must now be directed towards a rational method of observation. Medical science has undoubtedly advanced during the last fifteen years, although not to the extent, perhaps, which is capable of meeting with success the present crisis. Pathological chemistry, for instance, which we ought to expect should clear up many difficulties, is not yet rendered sufficiently exact, or cultivated so extensively, as to render all the services we require from it. A knowledge of histology, and the means of prosecuting morbid changes in the ultimate structure of organs and tissues, is still confined to a few. The chemico-physiological school, however, has some enthusiastic cultivators in this country, and, although not numerous, it is to them we look for the means of first discovering the nature of the disease, and then founding upon this knowledge a rational system of treatment. The next epidemic of Asiatic cholera may find the medical world as unprepared to encounter it as its predecessor; but we predict that it will not again leave us without our knowledge of its nature having been improved, and its treatment, in consequence, rendered more simple and rational.

PRIZE ESSAY ON HYDROPHOBIA.

WE beg leave to direct the attention of our readers to an advertisement on the cover, wherein it will be found that the sum of L.50 is offered for the best Essay on Hydrophobia. We understand that a gentleman who has narrowly escaped the disease, feels so much interest in the subject as to desire a good monograph to be written upon it. We sincerely wish that his liberality may meet with all the success it so well deserves.

BOOKS RECEIVED.

1. A Treatise on Diet and Regimen. By William Henry Robertson, M.D., &c. Fourth Edition. Vol. I. London. 8vo. 1847.

2. Proceedings of the National Medical Conventions held in New York, May 1846, and in Philadelphia, May 1847. Philadelphia. 8vo. 1847.

3. Inquest on Miss Sophia Dallett, held at Putney, &c., with an Appendix. Edited by John Rose Cormack, M.D., &c. London. 8vo. 1847.

4. A System of Practical Surgery, including all the recent Discoveries and Operations, with Forty-nine Illustrative

Plates, &c. Second Edition. By John Lizars, late Professor of Surgery to the Royal College of Surgeons, &c. Edinburgh. 8vo. 1847.

5. On the Nucleus of the Animal and Vegetable "Cell." By Martin Barry, M.D., F.R.S. Edinburgh. 8vo. 1847.

6. The Human Brain, its Structure, Physiology, and Diseases, &c. By Samuel Solly, F.R.S., &c. Second Edition. London. 8vo. 1847.

7. Ueber ein Selbständiges Darmnervensystems, von Dr Robert Remak. Mit 2 Kupfertafeln. Berlin. Folio. 1847.

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No. 18. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Contributions to Pathology and Rational Medicine.*

By JOHN HUGHES BENNETT, M.D., Lecturer on Pathology and the Practice of Physic, Director of the Poly-Clinic at the Royal Dispensary, Edinburgh, &c.

No. XI.—CLINICAL AND HISTOLOGICAL RESEARCHES ON CANCEROUS AND CANCROID GROWTHS.—(*Continued.*)

OBSERVATION XII.—*Cancer of Uterus; Ovaries; Lumbar, Mesenteric, and Epigastric Glands; Liver; Stomach and Colon—Peritonitis—Death.*

MARIAN CHRISTIE, æt. forty-nine, a widow of intemperate habits, was admitted Feb. 26, 1842, into the Royal Infirmary, under the care of Dr Spittal. Her illness was of three years' duration, and first appeared with constant pain in the epigastrium. During the last twelve months vomiting had supervened, generally coming on five or ten minutes after taking food. On admission, the symptoms were of a similar description. The vomiting was frequent. She had acute pain in the left hypocondrium, and wandering pains in the abdomen. There was great prostration of strength, and a sallow complexion. The catamenia had only appeared twice during the last five months. The pain in the abdomen subsequently became more severe, the presence of fluid also was apparent, and she died April 12.

Sectio Cadaveris, April 6, 1842.

Body greatly emaciated.

Abdomen.—The abdominal cavity was greatly distended with fluid, which was of a brownish colour, containing flocculi of lymph. The peritoneum lining the abdominal parietes was coated with a brownish lymph, which in the iliac region was of a black colour and gangrenous odour. The intestines were loosely adherent by soft lymph, of the same unhealthy character as that just mentioned. From the rectum upwards, they were covered with projecting tumours, varying in size from a sixpence to half-a-crown, of whitish colour, slightly depressed in the centre, and more or less vascular. The mucous membrane was healthy, except in the centre of the transverse arch of the colon, where the projection inwards of

one of these tumours had produced ulceration. In the sigmoid flexure of the colon, the tumours were not only larger but more continuous, and the intestinal walls were considerably thickened, and the gut constricted. At the pyloric portion of the duodenum, a large tumour projected from without inwards into the canal, and a few others of smaller size existed around. Several others formed a mass the size of a small orange, which pressed inferiorly and posteriorly inwards on the stomach, producing an hour-glass constriction of that organ. On opening it, the mucous membrane corresponding to the tumour was found deeply ulcerated, with raised edges, extending over a space the size of a five-shilling piece. The liver, at its posterior part, contained three or four white tumours about the size of walnuts, prominent on the surface and depressed in their centres. All the mesenteric and lumbar glands were enlarged and affected with the same disease, which, wherever present, presented on section a whitish colour, in some places very soft, yielding on pressure a copious milky fluid.

The whole fundus of the uterus was infiltrated with the same cancerous deposit, and presented on its surface several prominent tumours, varying in size from a hazel-nut to a walnut. The os uteri was enlarged, its margins rough and ulcerated, yielding on pressure a brownish, fœtid fluid. Both ovaries were enlarged, and infiltrated with white cancerous matter throughout.

The other abdominal organs, the lungs and heart, were healthy.

Microscopic Examination.—On examining a drop of the fluid, squeezed from a section of the tumour in the fundus uteri, at a point where it was somewhat softened, it was found to contain the structures represented—Fig. 32. Numerous corpuscles were observed, varying greatly in size and shape. Some completely round, about the 1-50th of a millimetre in diameter, containing a round nucleus, more or less granular, about the 1-130th of a millimetre in diameter. Some of these had evidently been rendered oval and elongated by pressure. Several also had attained a much larger size, and were of a caudate or spindle shaped form, with one or more nuclei, some of which contained double nucleoli. A few had reached a great magnitude, their nuclei and nucleoli being proportionally augmented. One in the centre of the figure measured nearly 1-10th of a millimetre across. Another corpuscle is seen where the nucleus measured about the 1-33d of a millimetre in diameter, and contained a large nucleolus, with a centre granule the 1-200dth of a millimetre in diameter. Mingled with the cells now described, were numerous granules, several naked nuclei, and some elongated fusiform corpuscles. In the harder portions of the same tumour, the cells varied in size from the 1-80th to the 1-30th of a millimetre in diameter.

The fluid squeezed from one of the tumours attached to the sigmoid flexure of the colon, contained cancer cells of the same general appearance as those in the uterus—(Fig. 33). There were few fusiform corpuscles, however, and a greater number of free nuclei.

Acetic acid produced the same reaction on these cells as has been so frequently described before. The cancerous deposit in the other organs, want of time prevented me from examining.



Fig. 32. Cancer cells and fusiform corpuscles from uterus, described Obs. XII. Fig. 33. Cancer cells from an enlarged lymphatic gland attached to the colon in the same case.

Remarks.—This was an example of very extensive cancerous formation. At the time it occurred, however (1842), not being so much interested in the subject as at present, I only kept notes and drawings of the appearances found in the uterus and glands. It will be observed, that the cancer cells in both had reached a much higher stage of development than any that have been yet noticed. In Fig. 32, we see how, in the same tumour, the cells grow in two different ways. In one increasing by multiplication of the nuclei, in another by one cell rising within another.

OBSERVATION XIII.—*Cancer of the Uterus—Fatty Liver—Bright's Disease—Death.*

ANNE DUFF, aged thirty-seven, admitted into the Royal Infirmary June 19, 1844, under Dr Graham. Has had five children. About four months ago she first perceived a discharge of a yellow colour from the vagina, and has since had constant pain in the lower parts of the abdomen. She has also passed clots of blood at irregular intervals. On admission she complains of constant pain in the region of the uterus, and there is a very copious fetid discharge from the vagina. To the touch the cervix uteri feels smooth, tense, and hard, and the os uteri considerably dilated and irregular, sending down projections into the vagina. The examination causes great pain. The urine is scanty, is mixed with blood, and highly coagulable by heat and nitric acid.—*September 23*, The symptoms, with more or less intromission, have continued up to the present time; the vaginal discharge always mixed with more or less blood. To-day she complains of pain and tightness across the breast, on left side. There is dyspnoea. The heart's action violent—pulse small and jerking.—*September 28*, Much weaker; aspect exsanguine; countenance anxious. No pain or hemorrhagic discharge. *October 7*, Has continued to lose strength. Three days ago the pain and bloody discharge returned. Died at half-past two, A.M.

Section Cadaveris, October 9th.

The body is of cachectic aspect, and unusually loaded with fat.

Head not examined.

Chest.—About a pint of clear amber-coloured serum occupied both cavities of the pleura. Pleura on right side slightly adherent by bands of chronic lymph. Both lungs were gorged, and loaded with yellow serum. *Heart* of normal size. The right auriculo-ventricular orifice so large as readily to admit the whole hand. Valves healthy.

Abdomen.—*Liver* enlarged, weighed four pounds six ounces. It was of a pale-yellowish colour throughout, and had undergone the complete fatty degeneration. The *left kidney* was of normal size, the cortical substance of whitish-yellow colour throughout, and of fatty aspect, presenting the so-called second stage of Bright's disease. The *right kidney* small, about the size of a goose's egg. The cortical substance indurated, and of white colour, with no trace of tubular substance. The pelvis and calyces, distended with urine, presented a series of sacs the size of marbles opening into each other. The *peritoneum* lining the pelvis was covered with shreds of recent lymph, and its cavity occupied by about half a pint of light yellow turbid serum. The *uterus* was much enlarged. The inferior half of the fundus transformed into a fungoid, sanguinolent soft mass, presenting all the characters of encephaloma. The cavity enlarged, presenting a greenish bloody, irregular lining. The os uteri the size of half-a-crown, irregular, nodulated, with projecting masses of fungoid disease. The superior half of the fundus of normal aspect, soft to the feel; on section, allowing a milky cancerous juice to be squeezed from the cut surfaces. Other organs healthy.

Microscopic Examination.—The milky fluid squeezed from the cut fundus of the organ contained corpuscles of a round or oval form, varying in size from the 1-50th to the 1-13th of a millimetre in diameter. Some of the larger contained greatly developed nuclei, containing one or two nucleoli. One is represented (Fig. 34), with a nucleus about the 1-30th of a millimetre in diameter, containing two oval nucleoli. The largest the 1-50th of a millimetre in its longest diameter. These cells were mingled with a few naked nuclei, and numerous granules. The action of acetic acid was the same on these as on other cancer cells. A thin section with a scalpel from the cut surface of the fundus, presented a fibrous mass so dense, that the direction of the filaments or their arrangement could not be discovered. On washing out the cells with water, however, and adding acetic acid, the fibrous matrix was seen to be made up of the elongated nuclei of fusiform corpuscles (Fig. 35), closely crowded together, assuming here and there a looped appearance of an oval form, or a rounded concentric arrangement. In some places concentric circles containing cancer cells were visible, as in Fig. 2.

Fig. 35.

Fig. 34.

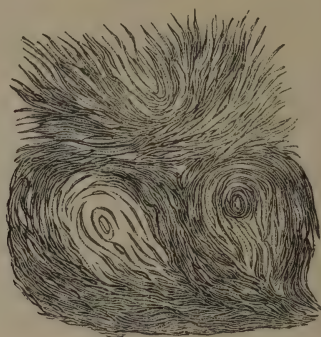
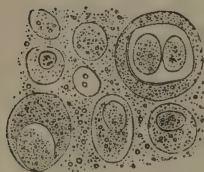


Fig. 34. Cancer cells in juice squeezed from uterus, described Obs. XIII. Fig. 35. Fibrous stroma of the uterus acted on by acetic acid.

Remarks.—In this case also the cancer cells had arrived at a very large size, as in the last case. It would seem that in the uterus these cells multiply very rapidly, and quickly produce great destruction of parts. Thus, in Obs. XII., the large masses of new growth formed could only be traced clinically nine months back, and in the present one only four months; and it is very likely that the increase of the cancerous formation is dependent on the excessive power of development of the cells.

OBSERVATION XIV.—Cancer of Uterus—Peritonitis—Death.

AGNES SUTHERLAND, a fisherwoman, aged thirty-nine, admitted September 15, 1845, into the Royal Infirmary, under Dr Alison. The patient is a widow, and the mother of five children. She has an exsanguine appearance, and states that, nine months ago, she first perceived a leucorrhœal discharge, which she attributed to fatigue and cold. Subsequently discharges of blood appeared, which have continued ever since, mingled with coagula. On admission she complains of pain in the abdomen, where there is considerable tenderness and tympanitis, particularly over the umbilical region. There is also great thirst, hot skin, furred tongue, and rapid pulse. The bowels are constipated,

and she complains of weakness of the back. Notwithstanding the employment of appropriate means to combat the peritonitis, she died on the morning of the 17th.

Sectio Cadaveris, September 18th.

The head was not examined.

Chest.—The cavity of each pleura contained about half a pint of turbid yellowish fluid; but there were no flakes of lymph, nor adhesions. The left lung was puckered at its apex, and contained several masses of old tubercle, passing into cretaceous concretions, and surrounded by a firm fibrous cyst of a black colour. The lungs otherwise were healthy. The pericardium contained three ounces of clear serum. The heart was healthy.

Abdomen.—The peritoneal cavity was much distended with gas and fluid.—The latter, amounting to about two quarts, was of a yellowish, purulent looking character, and contained numerous flakes of soft lymph suspended in it. This lymph formed a coating, in some places a quarter of an inch thick, over the abdominal surface of the diaphragm and liver. It was also diffused over the peritoneal coat of the intestines, but to a much less amount, and gave to the peritoneum a villous look, somewhat like mucous membrane. The intestines, both large and small, were much distended with gas; and they, together with the large omentum, were matted together by soft recent adhesions. The liver was pale and fatty, weighing 3 lbs. 8 oz. The kidneys were pale and flabby. The right one was much atrophied, both cortical and tubular portions being affected; whilst the pelvis was dilated, and capable of containing a large walnut. The pelvis was entirely filled with a firm tumour, which, on removal, and a section being made of it, was found to be owing to an enlarged and cancerous uterus. The superior half of the organ was hypertrophied: its walls an inch in thickness, and on pressure it exuded large drops of a white creamy viscous fluid. The inferior half of the organ, and upper portion of the vagina, were converted into an irregular, fungoid, cancerous mass, of a dirty greenish colour, covered with pus, and mingled with the creamy fluid just noticed. The fundus of the uterus was elevated above the brim of the pelvis, and firmly united to the inferior knuckles of intestine, and margin of the large omentum. These, together with several enlarged cancerous lumbar glands, formed one mass of disease.

Microscopic examination.—On examining the creamy fluid squeezed from the body of the uterus, it was found to contain numerous cancer cells densely crowded together. When separated by means of water, they were found to vary greatly in size and form, several being more or less caudate or spindle shaped. The round and oval corpuscles presented the same appearance described in former observations, and varied in size from the 1-150th to the 1-30th of a millimetre in diameter. The caudate and spindle shaped cells contained a round nucleus about the 1-100th of a millimetre in diameter. The cells of some of the former were cleft at one extremity, or presented two unequal prolongations—Fig. 36. Acetic acid partly dissolved the cell walls, whilst the nuclei remained unaffected.

The milky fluid squeezed from one of the enlarged lumbar glands contained cancer cells, mingled with fusiform and compound granular corpuscles. The first varied in size from the 1-150th to the 1-25th of a millimetre in diameter, with nuclei also varying in size. In one cell figured, Fig. 37, the nucleus is seen greatly enlarged, measuring about the 1-30th of a millimetre across, with one oval nucleolus in its wall containing two granules. The compound granular corpuscles were of great size and beauty, and existed in various stages of development, some only half, and others completely full of granules. One of the latter, 1-20th of a millimetre in diameter, is represented in the figure—Fig. 37.

Fig. 36.

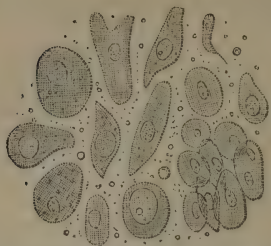


Fig. 37.

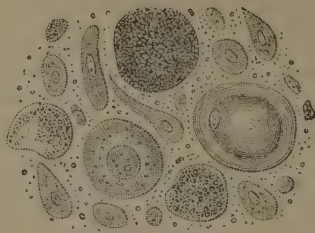


Fig. 36. Cancer cells in the juice squeezed from uterus, described Obs. XIV. Fig. 37. Cancer cells in the juice squeezed from a lumbar gland in the same case.

Remarks.—In this case we observe that the cancer cells from the uterus varied greatly in shape from those which existed in the two former observations, (Obs. XII. and XIII.) The general appearance of the cancerous alteration, however, as well as all the other elements of its structure, remained the same. The disease was of nine months standing, and yet the cancer cells were not so well developed as those in the last observation, where the uterus had apparently only been affected four months. Hence the duration of a disease bears no proportion to the development of the constituent cell elements of which it is composed. On the other hand, the cancerous juice in the lumbar gland presented some peculiarities. It contained numerous compound granular cells, whilst in that squeezed from the uterus not one was to be observed. Are we at liberty to draw any conclusion from this fact respecting the relative age of the cancerous formation? To this point we shall pay especial attention in a subsequent part of this paper.

OBSERVATION XV.—*Cancer of Uterus and Lumbar Glands—Death.*

BETTY BROWN, æt. forty, admitted February 22, 1846, into the Royal Infirmary under the care of Dr Patterson. She is much emaciated, and has a pale sallow complexion. She has had six children, the last fourteen years ago, and all without any bad symptoms. For the last ten years leucorrhœa has been present; menstruation has been regular until last May, when a bloody discharge took place every two or three days, up to October. In that month, considerable hemorrhage occurred at intervals, during a fortnight. It then ceased, but returned eight weeks ago, and continued three weeks. A week ago it occurred again, but only lasted four days, and was slight. During the interval between the first and second flooding, a thin watery discharge of a very offensive odour came away. Each flooding was preceded by a gnawing pain in the uterine region, which, on its commencement, disappeared. At present there is a profuse watery discharge from the vagina, communicating to her person a very offensive odour. She passes urine without difficulty. The appetite is bad.—*March 3d*, A swelling may be detected in the pelvis, above the pubes, the size of which cannot be determined.—*March 16th*, The discharge has much diminished. There is no pain. Appetite improved. Bowels kept regular by medicine. On examination of the os and cervix uteri, they were found ulcerated to a great extent, and a white foetid fluid flowed from the speculum.—*March 31st*, The discharge has again become abundant, and is very fetid. She complains of pain in the back and lower part

of the abdomen, which is increased on pressure. The tumour formerly felt is apparently larger, and can be detected more in the centre of pelvic region.—*April 14th*, Has continued to suffer great pain, and has become very weak from the incessant discharge and absence of appetite. She gradually sunk and died, *April 19th*.

Section.—*Head* not examined.

Chest.—Lungs pale; emphysematous anteriorly, and superiorly much loaded with carbonaceous matter.

Abdomen.—The pelvis was entirely filled up with indurated nodulated masses, which compressed the right ureter below, causing it to be distended superiorly with fluid. On removing the pelvic contents, and subsequently examining them, it was found that, at the upper end of the vagina, a cavity the size of an orange had been formed. The walls were of a dirty green colour, rough, and uneven on the surface, from which a dirty brown opaque fluid could be squeezed. It presented all the characters of cancerous ulceration in its last stage. The os and cervix uteri had completely disappeared; the upper half of the fundus only preserving its healthy appearance. The ulceration was separated from the cavity of the abdomen by the peritoneum, which was united to the ovaries and several enlarged lumbar glands, by bands of chronic lymph. These, together with the distended vagina, formed the mass which filled up the pelvis. The adherent lumbar glands were as large as marbles and pigeon's eggs, and, on section, presented a smooth surface of whitish-yellow colour, and of soft, cheesy consistence. The ovaries, on being cut into, presented several cicatrices loaded with black pigmentary deposit.

The right kidney was diminished to the size of a hen's egg, and anæmic throughout. The cortical substance was much atrophied, being, at its thickest portion, only one-eighth of an inch in depth. The tubular cones were small, short, but healthy in structure. The pelvis was much enlarged; it, as well as the ureter, distended with pale urine,—the mucous surface healthy. Left kidney anæmic, but otherwise healthy. The right ureter was about the size of the little finger, and distended with fluid, evidently from the obstruction it had received in the pelvis. Other abdominal organs healthy.

Microscopic Examination.—On examining a lumbar gland about the size of a pigeon's egg, it was found to be infiltrated with the corpuscles represented Fig. 38. These consisted, for the most part, of variously shaped, somewhat flattened cells; some round and oval; others squarish, triangular, or more or less indented, as if collapsed or pressed upon. A small nucleus could only be seen in a few. For the most part, they contained nothing but molecules and granules, either scattered thinly throughout their interior, or confined in a group to the centre. They varied in size from the 1-100th to the 1-40th of a millimetre in diameter. It was not possible to determine whether the former or smaller ones were free nuclei or not. Acetic acid rendered the cell walls slightly more transparent, but otherwise produced no change upon them. They floated amidst a multitude of loose molecules and granules.

Fig. 38.



Fig. 38. Retrograde cancer cells from a lumbar gland, Obs. XV.

Remarks.—This woman had laboured under leucorrhœa for ten years; yet the functions of the uterus were not seriously deranged until about twelve months previous to her decease. The possibility of forming any precise notion of when the disease commenced, is, in this as in most other cases, impossible. The appearances discovered

on dissection, however, prove it to have been very old. The cancerous formation in the uterus had softened and been discharged by the vagina, leaving a cavity, presenting on its walls the appearance of chronic ulceration. The section of the lumbar glands, one of which only I had it in my power to examine, presented a whitish-yellow colour, and cheesy consistence, without yielding any cancerous juice. It resembled the so-called reticulum of Müller's "Cancer Reticulare," and the yellowish material described in Obs. X. and XI. Its minute structure, however, presented a cell formation very different. The cell walls were here and there partly collapsed, as in Obs. XI.; but in the majority the nucleus had disappeared, and, instead of it, a number of molecules and granules existed. Are we to suppose these to be a disintegration of the nucleus, or the result of endosmosis through the cell wall? However this may be, there can be little doubt that the appearances described constitute one mode of the retrograde course of the cancer cell.

In the four cases of cancer of the uterus now detailed, we find that death was caused by peritonitis in two, by fatty liver and kidney in one, and by exhaustion from the excessive discharges in the fourth. In one case (Obs. XIV.), there were chronic encysted tubercles in the lungs, showing that, at an early age, the tubercular diathesis had existed and been removed, and that the cancerous disease had supervened. The existence of fatty liver and kidney in another case (Obs. XIII.), is also worthy of notice, as indicating that these degenerations may occur with cancerous as well as with the tubercular formations.

I could easily have multiplied cases of uterine cancer; but as it was not in my power to add other histological facts of importance, their publication has appeared to me unnecessary. I may add, however, that I have lately examined a cancerous uterus where the fundus of the organ only was affected. It was, at its deepest part, an inch thick, of firm consistence, and no cancerous juice could be squeezed from the cut surface. There was no ulceration, or softening of any kind. Repeated sections with Valentin's knife exhibited delicate cancer cells developed in the fibrous stroma of the organ, which, on the addition of acetic acid, in some places presented the appearance represented Fig. 2; in others, the loculi figured Fig. 10, whilst the intervening portion consisted of dense fibrous tissue, as depicted Fig. 35.

OBSERVATION XVI.—*Cancer of the Liver, Stomach, Spleen, and Lumbar Glands*
—*Pneumonia—Death.*

JOHN DOHERTY, æt. 50.—A shoemaker from Ireland. Admitted November 11, 1846, into the Royal Infirmary, under Dr Andrew. He entered the army at the age of twenty-three, and was almost immediately sent to India, where he remained seven years. He did not live intemperately, and enjoyed excellent health for five years. He then experienced pains in the limbs and joints, and says he had liver complaint, for which he was actively treated. He did

not recover, however, and was finally sent home and discharged. Since then he has had good health, until thirteen weeks ago. At that time, he began to suffer from vomiting, which was arrested by medical treatment; but he has experienced severe pain in the abdomen, back, and left shoulder, with great tenderness of the epigastrium. He now suffers considerable pain in the bowels and epigastrium, which are acutely tender on pressure. Percussion and auscultation of the lungs are natural. The hepatic dulness is extended about an inch below the ribs, on the right side, and stretches considerably on the left side into the hypocondriac region.—*November 18*, The tenderness of epigastrium is diminished. The liver may be seen to have descended lower, and to be nodulated externally. To the feel, these present hard, irregular masses, the size of a walnut.—*December 5*, Since last report, he has suffered much from pain in the epigastrium, shooting backwards to the spine. There is now a distinct swelling, visible externally, situated in the epigastrium, which, on manipulation, feels hard and non-resistant, without any defined margin. Pressure upon it, causes considerable pain.—*December 20*, Since last report, the general emaciation and weakness have become very great. The tumour in the epigastrium, extending towards the right hypocondriac region, has rapidly augmented in bulk. For the last two days there has been great restlessness and fever, with slight delirium. Died at 3 P.M.

Section Cadaveris, January 1st, 1847.

Chest.—Upper lobe of right lung hepatized over an extent the size of the palm of the hand. Lungs otherwise emphysematous anteriorly, and congested posteriorly. Heart healthy.

Abdomen.—The liver somewhat enlarged, and studded throughout with circumscribed masses of cancer, varying in size from a walnut to that of a small orange. Several of these were prominent on the surface, and depressed, or cup-shaped, in the centre. The intervening tissue of the liver appeared healthy. On section the cancerous masses were of white colour, slightly granular surface, cheesy consistence, and yielded only here and there a milky juice, where softening had taken place. Scattered throughout this substance were small collections of a yellowish fawn-colour, and creamy consistence, reticulated together over the surface of the section. Several of the tumours were very vascular, especially at their circumference, and a few presented infiltrated extravasations of blood. The pyloric extremity of the stomach considerably indurated, extending over the posterior surface of the organ in an oval form, the longest diameter being five inches. Internally a cancerous ulcer extended over a corresponding portion of the mucous membrane, with an elevated, partly everted, edge. Here and there, external to the ulcer, there were a few nodules of indurated cancer, the size of peas, below the mucous membrane. The pylorus was so much constricted as scarcely to permit the entrance of the little finger. The body and cardiac end of the organ, enormously distended. The cancerous portion of the stomach was half an inch thick, the muscular coat, occupying a quarter of an inch of this, being hypertrophied, and exhibiting a striated appearance. The spleen was small, adherent to the stomach, containing at the point of adhesion a cancerous mass the size of an almond. The lymphatic glands surrounding the pylorus were enlarged, and infiltrated throughout with cancerous matter. The lumbar glands also much enlarged from a similar cause. Other organs healthy.

Microscopic examination.—On examining the milky juice squeezed out of one of the nodules of the liver, at a point where it was found somewhat softened, it was found to contain numerous delicate cancer cells. These were of a round or oval form, varying in their longest diameter from the 1-50th to the 1-40th of a millimetre in diameter. The cell walls were exceedingly thin and delicate, requiring in many instances considerable management of the light, in order to see them distinctly. They all contained one or two nuclei, which were generally of oval form, about the 1-100th of a millimetre in their longest diameter, containing one or more granules and molecules. These cells were

mingled with a few naked nuclei, numerous granules, and several compound granular corpuscles and masses—(Fig. 39.) On the addition of weak acetic acid, the cell walls were rendered so exceedingly transparent that they could scarcely be seen, and resembled a faint halo round the nucleus, or a line that could scarcely be traced. This is imperfectly represented—(Fig. 40, *a*.) On adding strong acetic acid, the cell walls were immediately dissolved, whilst the nuclei remained unaffected—(Fig. 40, *b*.) The yellowish creamy matter of the reticulum was composed of minute molecules and granules of oil of various sizes, which refracted light, only some of the latter were 1-200th of a millimetre in diameter. They disappeared on the addition of ether, leaving behind a finely molecular matter only. The intervening substance of the liver was healthy. Want of time prevented my examining the other cancerous tissues in this case.

Fig. 39.

Fig. 40.
a. *b.*

Fig. 41.

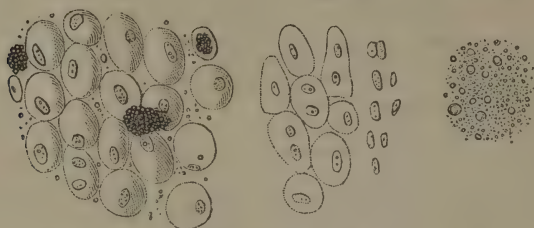


Fig. 39. Young cancer cells from the liver, described Obs. XVI. Fig. 40. The same acted on by acetic acid. Fig. 41. Molecular and granular matter of the reticulum in the cancerous masses.

Remarks.—In this observation we find a well-marked example of the so-called white tubercle of the liver described by Baillie and Farre. Such cases are by no means uncommon, and I have invariably found the cancerous masses to consist of a firm coagulated blastema, in which cancer cells in every stage of development could be discovered. In the observation just detailed, the harder nodules exhibited the morbid growth proceeding slowly in its development. Even where softening had taken place, the cells were exceedingly delicate, exhibiting a very early stage of growth. It is important to remark, that notwithstanding the early stage in development of the cancerous tubera, a section of them presented a well-marked reticulum, with the same structure as we have seen in cancer of the breast.—(Obs. V.) The question as to whether the accumulation of these fatty granules is a contemporaneous or a subsequent process, and its influence on these growths in general, can only be deduced from careful examination of their existence in relation to the cancer cells in a series of observations.

Although I had no time to make careful examinations and drawings of the changes in the stomach, and other organs, I convinced myself, from a few demonstrations, that these changes depended on cancerous growths in different stages of development. From the history of the case it would appear, that in India he suffered from vomiting, epigastric pain, and other symptoms, which are commonly spoken of under the name of liver complaint. It is probable, for

reasons which I shall hereafter allude to in connexion with cancer of the stomach, that that organ was the one primarily affected in this case, and had probably existed for years. It would seem that, notwithstanding the extensive cancerous deposits which existed, his general health was tolerably good, until thirteen weeks previous to admission, and about twenty weeks previous to his death, which, probably, was accelerated by the limited pneumonia which latterly made its appearance.

OBSERVATION XVII.—*Cancer of Liver—Pancreas, and Lymphatic Glands—Death.*

JOHN LAWRIE, æt. fifty, flesher.—Admitted into the Royal Infirmary, March 24th, under Dr Robertson. He has been in prison for the last four years, and has enjoyed good health until three weeks ago, and has since become more and more emaciated. He has also felt pain in the lumbar region, and his legs and feet have become swollen. On admission, the face is sallow and haggard, and there is œdema of the legs. He complains of slight pains in the abdomen, on examining which the liver was found greatly enlarged. It extended two inches below the umbilicus in the middle, but not quite so low on each side. The edge is rounded, and it is easily moved. There is some tenderness on the lower and outer part of right lobe. He has huskiness of the throat, which is dry and red; the velum pendulum is apthous. The appetite is diminished. There is great thirst. Pulse 88, weak. It is stated that before his imprisonment he was very intemperate. Died March 28th.

Sectio cadaveris, March 31st.

Head.—The brain and its membranes were healthy.

Chest.—Pleura on both sides adherent posteriorly by chronic adhesions. On the left side also, there were firm adhesions towards the apex. Both lungs much engorged inferiorly, so much so that the section presented a smooth polished surface, and portions of it sunk in water. The apex of left lung somewhat puckered. The parenchyma below it, indurated from dense carbonaceous deposits. Several of the minute bronchi were obliterated, presenting lines of dense fibrous tissue, which, here and there, were of stony hardness from deposits of earthy matter. *Heart* healthy.

Abdomen.—The liver was greatly enlarged, occupying the whole breadth of the abdomen, and descending inferiorly two inches below the umbilicus. It weighed 13 lbs. 12 oz. Externally it presented a crimson purple ground, scattered all over with white spots and masses varying in size, running together, and giving it a porphyry like appearance. On section it was seen to be infiltrated throughout with soft cancerous matter, which could be squeezed from its surface in the form of a milky juice. It was nowhere gathered together so as to form masses or tumours, but was scattered throughout the organ in minute points, which, by coalescing here and there, formed patches of greater or less extent. In the left extremity of the pancreas there were several tumours, the size of small marbles, of white cancerous deposit. The lymphatic glands in the neighbourhood of the pancreas and stomach, were greatly enlarged from the same soft cancerous infiltration; the largest was about the size of a walnut. The mesenteric and lumbar glands were all similarly diseased. The kidneys presented the second stage of Bright's disease, the external surface exhibiting the roe-like aspect, well marked. On the right side the tubular cones were much encroached upon; on the left, they were unaffected. Other organs healthy.

Microscopic Examination.—The milky juice squeezed from the liver, contained numerous cancer cells of different forms. Some were round or oval, varying in size from the 1-50th to the 1-40th of a millimetre in diameter. Others were caudate, spindle-shaped, or more or less compressed at the sides.

They were nearly all furnished with nuclei, of round or oval shape, varying from the 1-100th to the 1-80th of a millimetre in their longest diameter. Several of these may be seen (Fig. 42) to contain a double nucleolus together with several molecules. One half moon shaped cell in the figure, contains two nuclei of about the same size, both of which possess four nucleoli. On the addition of acetic acid, the cell walls were rendered more transparent, whilst the nucleus appeared somewhat contracted and with a thicker edge. Mingled with the corpuscles were numerous molecules and granules, with a few compound granular corpuscles and masses. The hepatic cells between the cancerous infiltration were carefully examined, and found to present their normal appearance, with the exception of a few brownish irregular masses adhering to some of them, apparently clotted blood.

The milky fluid squeezed from an enlarged lymphatic gland, size of a walnut, below the pancreas, contained the corpuscles represented Fig. 43. Some of these were of an oblong form, with an oval nucleus and two nucleoli. A few were caudate. Several naked nuclei, blood corpuscles, granules, and compound granular masses, were also present. Scattered throughout the substance of this gland was a distinct reticulum, of a fawn colour and cheesy consistence, which was composed of broken down cancer cells, and irregular shaped bodies, exactly like those seen in Obs. X., Fig. 27.

The cancerous masses in the pancreas were not examined.

Remarks.—In this case, the cancer, instead of being disseminated in distinct masses, was infiltrated throughout the liver. The cells were also in a much more advanced stage of development than in the last Observation. In the lymphatic glands, they were evidently in a commencing state of disintegration.

OBSERVATION XVIII.—*Cancer of Liver, Pylorus, Lesser and Greater Omentum and Lumbar Glands—Fever and Dysentery—Death.*

BERNARD PAGAN, æt. thirty, from Ireland. Admitted into the Royal Infirmary, June 23, 1847, under Dr Robert Paterson. On the 21st instant he had rigors with nausea, followed by great heat, and the usual symptoms of fever, to the contagion of which he had been exposed. He is much emaciated, and, previous to admission, has suffered from want of food. Since Monday also, there has been diarrhoea, and at present the stools are frequent, watery, and slimy. The skin is dry and warm, and of a dingy hue.—*June 26th*, To-day the stools are of a light colour, with much mucus tinged with blood.—*July 4th*, The stools are muco-purulent, with some streaks of blood, and their passage is accompanied by pain and much straining. No tympanitis or tenderness of abdomen on pressure.—*July 9th*, Much singultus.—*July 17th*, Hiccough less frequent. Tongue clean and glazed. Takes his food but sparingly. Emaciation continues. Stools still muco-purulent, with occasional masses of mucus tinged with blood.—*August 6th*, The muco-purulent and sanguineous discharges have continued, the strength has lessened gradually, and he sunk this morning.

Sectio cadaveris, August 8th.

The body was much emaciated; abdomen flattened from the retraction of the parietes.

Fig. 42.

Fig. 43.

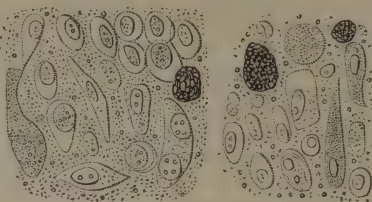


Fig. 42. Cancer cells in juice squeezed from liver, described Obs. XVII. Fig. 43. Cancer cells in juice from a lymphatic gland in the same case.

Chest.—The lungs were congested posteriorly, especially the lower lobe of the left lung, where pressure caused to exude from the cut surface a copious frothy fluid. *Heart* healthy.

Abdomen.—The opposed surfaces of the liver and diaphragm were closely united by old cellular adhesions, which were strongest in the neighbourhood of and to the right side of the insertion of the falciform ligament. Between fifteen and twenty spherical masses of a whitish colour, varying in size from a cherry to a chestnut, were disseminated through the middle third of the right lobe of the liver. Several of these masses were visible on the superior, but none on the inferior surface of the organ. They were all abruptly bounded by healthy hepatic substance, were of somewhat friable consistence, but yielded a milky fluid on pressure. Some of these masses of deposit contained cysts, in some cases occupying the centre, in others situated near the circumference, filled with a turbid whitish fluid. The left lobe of the liver was free of the cancerous deposit, but presented a few round cavities, varying in size from a pea to a hazel-nut, directly bounded by the surface of the organ, and filled with deep black blood of the consistence of mucilage. The small omentum surrounding the hepatic ducts and vessels, contained several cancerous tumours, varying in size from a pea to a hazel-nut. The mucous coat of the stomach was unusually pale, but healthy. The pyloric orifice barely admitted the passage of the little finger; it was constricted by a cancerous mass, situated between the mucous and muscular coat, in size between that of a pigeon and small hen's egg. The mucous and serous coats of the pyloric orifice were healthy, but the muscular coat was much hypertrophied, the sixth of an inch in thickness. On the mucous surface of the duodenum, near its commencement, two cancerous tumours of the size of peas were seen. The whole of the great omentum was studded with cancerous tumours, varying in size from a pea to that of a cherry, and some of these were of a dull, purplish hue. In the situation of the left supra-renal capsule was a mass of cysts of the size of walnuts, containing fully an ounce and a half of thick dark blood, resembling that contained in the cavities in the left lobe of the liver, and one of these cysts was partially filled with a soft cancerous deposit, in part discoloured by blood. The lumbar and mesenteric glands were much enlarged, and of firm consistence. They grated somewhat under the knife on section, were generally of a pinkish white colour, and contained little yellow masses of friable deposit, sparingly scattered through their substance. They were here and there tinged of a deep blue colour by the ramification of vessels loaded with blood.

The descending colon and rectum were contracted in size, and their coats were thickened and corrugated. Their mucous surface was here and there coated with patches of reddish granular lymph, and presented numerous circular ulcerations, occupying the situation of the glandulæ majores simplices. They were of uniform size, about a line or a line and a half in diameter, not exposing the muscular coat. They were encircled by a narrow areola, of a dark livid hue, and their borders were not elevated. They appeared, as it were, punched out of the mucous membrane. Little tumours of the size of small peas, resembling those in the omentum, were seen loosely attached to the serous surface of the rectum, and several of the mesocolic glands were of the size of hazel-nuts, and infiltrated with cancerous deposit.

Microscopic examination.—The cancerous juice squeezed from the liver contained three distinct kinds of corpuscles:—1st, Numerous well-formed cancer cells, with one or two nuclei, and a single or a double nucleolus; 2dly, Compound granular cells; and 3dly, Pus corpuscles, which were mingled with the cancer cells in considerable numbers—(Fig. 44.) On the addition of acetic acid, the distinction between the cancer and pus cells was rendered very apparent. The former had their wells rendered very transparent, whilst the nuclei was either unaffected, or its margin rendered thicker. The latter had the cell walls dissolved, and the characteristic nucleus of two or three granules

rendered apparent—(Fig. 45.) A few naked cancer nuclei and numerous granules were mingled with these corpuscles.

On examining the yellow masses in the mesenteric glands, which grated somewhat on section, they were seen to contain,—1st, Numerous cancer cells in a state of disintegration; the cell walls more or less collapsed, contracted, and so altered in shape as to present no distinctive form. In several, no nucleus could be seen, and its place was occupied by a number of minute molecules; in others, two or more granules could be discerned. 2dly, Small fusiform corpuscles, more or less broken down. 3dly, Numerous laminated crystals of cholestrine. And 4thly, Numerous granules, here and there aggregated into masses. Some of these were evidently fatty; others, from their irregular form, were evidently mineral in their nature—(Fig. 46.) On the addition of acetic acid, the abortive cancer cells became generally more transparent, the fragments of fusiform cells and crystals of cholestrine remained unchanged, whilst the mineral granules and masses were dissolved—(Fig. 47.) One cell in this figure is seen of large size, distinctly nucleated, with its walls collapsed.

Fig. 44.

Fig. 45.

Fig. 46.

Fig. 47.



Fig. 44. Cancer and pus cells in juice squeezed from liver, Obs. XVIII. Fig. 45. The same acted on by acetic acid. Fig. 46. Retrograde cancer cells, crystals of cholestrine, and masses of mineral granules, in the yellow masses of the lymphatic glands in the same case. Fig. 47. The retrograde cancer cells acted on by acetic acid.

Remarks.—In this case there had supervened on the cancerous disease of the liver a recent exudation, which had become transformed into pus. This is an occurrence, I am inclined to think, of considerable rarity. In this also, as in the last Observation, the cancer cells of the lymphatic glands were undergoing disintegration. Conjoined with this we observe, first, an excessive formation of fat, in the form of cholestrine; and secondly, the accumulation of earthy salts, in the form of granules. We shall afterwards see, that this latter occurrence is one of the means whereby cancerous formations are so transformed as to be rendered latent in the system. This must be considered as a very chronic case of cancer, and remarkable from the circumstance that, notwithstanding the great amount of disease present, there were no symptoms which indicated its existence. The man complained of nothing but the fever which was then prevalent in Edinburgh, and of dysentery, which was also epidemic. It may be asked, whether the latter disease was in any way connected with the purulent formation in the liver?

OBSERVATION XIX.—*Cancerous Ulcer of Stomach—Stricture of Pylorus—Cicatrices in the Liver—Calcareous Concretions in Mesenteric Glands.*

MARY ANNE RAMSAY, æt. forty-three, a widow, was admitted into the Royal Infirmary, August 25, 1844, under the care of Dr Alison. She states that two years

ago the catamenia stopped, and that her health has been very indifferent ever since. For ten years she has occasionally experienced pain in the stomach after taking food, which has become more continued and violent since the period referred to. She was treated for this complaint in the Infirmary last October, and was dismissed free from pain. It returned, however, three months ago, and has continued ever since, accompanied by acid eructations and occasional diarrhœa. There has been no hemoptysis or hematemesis. She has had palpitation more or less severe for two years, leucorrhœa for three years, and dysuria for twelve months.

On admission there is severe pain in the epigastric region; very severe half an hour after taking food. There are rather violent pains all over the abdomen, but no tenderness; there is diarrhœa. A fœtid discharge flows from the vagina, and she experiences a sharp pain on passing urine. There is great thirst, sweating at night, temperature of skin natural; appetite somewhat better, she says, than usual. Respiratory and heart sounds natural.—*September 14th*, Since admission has had occasional diarrhœa, which was readily checked. The pain in the stomach, with vomiting, has continued.—*October 25th*, Vomiting of acid matters and of food continues. Emaciation has made great progress. There has been occasional cardialgia, with tenderness on pressing epigastric region. To-day a circumscribed swelling could be detected in region of the pylorus.—*November 16th*, The symptoms have continued, the emaciation has increased, her strength has rapidly diminished, and she sunk this evening at 9 P.M.

Sectio Cadaveris, November 18th.

Chest.—Both lungs were adherent at their apices. On the left side there was a deep cicatrix, with considerable puckering at the apex of the lung, which to the feel resembled an indurated nodule about the size of a walnut. On section it was found to consist partly of cheesy tubercular matter, and partly of gritty calcareous matter. On the right side was also a cicatrix, with puckering at the apex, but of much smaller extent, with induration of the pulmonary tissue, without the traces of tubercular deposit. The lungs were otherwise healthy. The heart was small; the coronary arteries rigid, from calcareous and atheromatous deposits; the lining membrane of the arch of the aorta studded with similar deposits.

Abdomen.—The liver was somewhat atrophied, and presented on its surface several deep puckerings and depressions, which corresponded to indurated yellowish masses in their centres, varying in size from a pea to that of a hazel nut. These masses were of firm cheesy consistence, slightly grating under the knife, and on section presented a somewhat fibrous matrix. The hepatic substance in their immediate vicinity appeared to be healthy. The stomach was firmly adherent posteriorly to the pancreas, and to several lymphatic glands, which were enlarged and of white colour. On being opened, the organ was found to be considerably distended with a semi-fluid brownish matter. The pylorus was considerably constricted, apparently from the formation of cancerous deposit, so as scarcely to admit the forefinger. Extending from it, on the posterior wall of the stomach, corresponding to the firm adhesion between it and the pancreas, was an oval ulcer about two inches in its longest diameter. The edges were raised nearly a quarter of an inch above the level of the mucous membrane, were of soft consistence, easily breaking down between the fingers into a pultaceous dirty white substance. The base of the ulcer was formed by the muscular coat. The mesenteric and epigastric glands considerably enlarged, and of white colour. Some felt hard, from calcareous depositions; others were infiltrated with a putty-like substance; and a few were composed of an external shell of hard calcareous matter, whilst their interior consisted of a semi-fluid, gritty, diffuent material, which flowed out on breaking them. The other organs were healthy. The uterus was not examined.

Microscopic Examination.—On squeezing a little of one of the yellow masses in the liver between glasses, and examining it under the microscope, it

exhibited a substance so dense that its integrant parts could not be made out. Several stellate crystals of margarine, however, could be detected here and there, as seen on the right of Fig. 48. On diluting the mass with a little water, it was seen to be composed, 1st, of round or oval corpuscles, with faint and somewhat flaccid cell walls, containing several molecules and granules, and in a few, one or even two nuclei could be detected. They varied in size from the 1-100th to the 1-35th of a millimetre in diameter; the smaller ones resembled free nuclei; 2d, of margarine, in groups of needle-shaped crystals; 3d, of numerous granules and molecules, which had a disposition to collect in small groups—Fig. 48. On the addition of acetic acid, the walls of the corpuscles became more faint, but no other change was produced. Ether at once dissolved the granules and needle-shaped crystals.

The putty-like substance, and diffuent matter in the mesenteric glands, contained similar corpuscles, though smaller, and varying more in shape. They were mingled with numerous irregular-shaped granules, and masses of mineral matter, which were entirely dissolved in weak nitric acid—Fig. 49.

A thin slice of the strictured portion of the pylorus was composed of a dense fibrous texture, which, on the addition of acetic acid, became more transparent, and exhibited elongated nuclei, such as is represented Fig. 15, and will be again figured when I describe cancer of the stomach. No cancer cells could be squeezed out of it, or were to be seen between the meshes. The surface of the ulcer in the stomach consisted of the debris of epithelium cells, shapeless flakes, and a multitude of molecules and granules, exactly similar to what is found on the surface of most sloughing ulcers. On dissecting below the epithelium, the raised border of the ulcer was found to be continuous with an indurated mass of a flesh colour, which united the stomach firmly to the pancreas. This mass was composed of a fibrous matrix, in the meshes of which numerous cancer cells and free nuclei were crowded together. The fluid scraped from the surface of this growth was of an amber colour, and turbid. It contained cancer cells and nuclei embedded in a finely granular blastema, which assumed various shapes and sizes, according as it was more or less broken up. The number of naked nuclei were in excess. The cells were in general oval, their walls very delicate, and did not exceed the 1-40th of a millimetre in their longest diameter. Several fusiform corpuscles were present, some of which, from their curved form and adhesion to masses of granular blastema, appeared to form the boundary of what seemed a compound cell—Fig. 50.

Fig. 48.

Fig. 49.

Fig. 50.

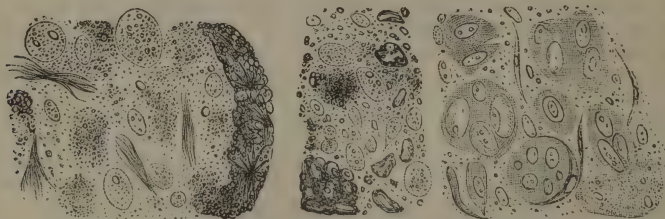


Fig. 48. Retrograde cancer cells, and crystals of margarine in yellow masses of liver, described Obs. XIX. Fig. 49. Retrograde cancer cells with mineral matters from mesenteric glands in the same case. Fig. 50. Cancer cells and nuclei embedded in a granular blastema, with fusiform corpuscles, from the stomach.

Remarks.—This case is an exceedingly interesting one in many points of view. It offers, in my opinion, strong proof of the spontaneous cure of cancer in an organ, and that in two different ways. Not that one case alone would be sufficient to prove this statement,

but that, in conjunction with others which have been previously, and will subsequently be given, little doubt, I believe, can exist as to the true cause of the appearances described. The facts to which I would particularly direct attention in this Observation are, 1st, The retrograde course of cancer in the liver, with excess of fat formation; 2d, The retrograde course of cancer in the mesenteric glands, with excess of mineral deposition; 3d, Chronic stricture of the pylorus; and, 4th, An unequivocal cancerous ulcer in the stomach.

It is by no means uncommon to find puckerings, with yellowish masses, in the liver. This may arise from former depositions of tubercle, chronic abscesses, or cancerous formations. Either of these forms of exudation, after having gone through their ultimate stage of development, may shrivel up, become drier and drier, and cause contraction and puckering of the surrounding tissue. On examining these remains of former exudation, it is exceedingly difficult to determine from their structure what was their former nature, inasmuch as all kinds of retrograde exudation present characters more or less in common. It is only, then, from concomitant circumstances that we can be led to a correct deduction. But here this case presents a great difficulty. This woman had evidently laboured under previous tubercular exudation in the lungs; and it may be argued, that the yellow masses and puckerings in the liver, as well as the calcareous concretions in the mesenteric glands, were rather owing to similar deposits in those organs than to previous cancer. In fact, we have seen in a former observation (Obs. XIV), how cancer may coexist with traces of previous tubercular deposition in the lung. But here we have also stricture of the pylorus, which was evidently chronic, as it was unaccompanied by recent cell formation. Subsequent observations, I think, will render it probable that this was owing to cancerous rather than tubercular exudation, and favours the supposition, that the alterations in the liver and lymphatic glands were owing to cancer also. Besides, the corpuscles in both situations exactly resemble the retrograde cancer cells we have seen previously (Obs. XI. XV. XVIII.), and are in no way like retrograde tubercle corpuscles. Neither did they resemble the hepatic cells in the immediate neighbourhood, which, although somewhat loaded with fat, were otherwise healthy. To these arguments may be added another, derived from the fact, that cancer in an active state of development existed in the stomach at the period of death.

The appearance of the cancer cells in the stomach merits special observation. Most histologists who have paid attention to the structure of cancer, have described compound cancer or mother cells. For a long time I conceived that the structures represented, Fig. 50, were beautiful specimens of this formation. Later observations, however, have convinced me that the appearances described are owing to the imprisonment of nuclei and cells in a finely granular blastema, broken up into various sized masses. I have been

unable to detect any cell wall surrounding them, and, on adding water, the granular matter is separated, and the nuclei and cells float loose. Here and there clefts or divisions may be seen between these masses. This fact has been previously observed by Küss (see Sedillot on Cancer, Planche 7), who has founded upon it a theory of fissiparous multiplication of the cancer cell. Several of the figures given by Bruch¹ (Taf. 1, Fig. 8; Taf. 2, Fig. 4, &c.), it appears to me, present the same structure as I have now described. In one mass, represented at the lower parts of Fig. 50, a curved fusiform corpuscle is seen to form a boundary to it, a circumstance which may also have led to the idea of such masses possessing a distinct envelope.²

ARTICLE II.—*Note on Dr Martin Barry's Views in Embryology.*

By A. G. MELVILLE, M.D., Late Assistant Demonstrator in the University of Edinburgh.

IN an article recently published, "On the Nucleus of the Animal and Vegetable Cell," by Dr Martin Barry, certain doctrines on cell formation, announced in his papers on embryology, are again brought forward.

Anxious to satisfy myself whether the beautiful theory advanced by my esteemed friend was based on accurate and unbiassed investigation, I took part in a series of observations on the ovum of the rabbit, before and after impregnation; and it is with much regret that I am compelled to differ from Dr Barry, regarding the interpretation of appearances which must have been alike in both instances.

My observations corroborate the statements of the accurate and laborious Bischoff on many disputed points; and I the more willingly bear testimony to his accuracy, since I have had occasion to verify his researches in another sphere of anatomical science. So far as this theory is based on the mal-interpretation of phenomena occurring in the ovum, before and after impregnation, it is destitute of even the vestige of a foundation. The removal of error is sometimes as advantageous to the cause of science as the promulgation of new truths, especially when doctrines in some measure baseless have been applied by high authorities to the elucidation of intricate

¹ Die Diagnose Bösartige Geschwülste, an admirable work, which I have received during the past month.

² I shall feel much obliged to any gentleman who will furnish me with *fresh* specimens of malignant melanosis, osteo sarcoma, cancer of bone, fungus medullaris, cancer of brain, or any other rare or doubtful form of tumour. A tolerably thick slice, embracing all the peculiarities of the growth, wrapped up in oil silk, and forwarded by post, with a history of the case, would meet my immediate attention. It is of importance that no alcohol be added to the specimen.

and complex phenomena ; and it is to be wished that new labourers should enter this interesting and important department, and give us their fresh and unbiassed opinions on the various points at issue.

Aided by the best glasses, I have failed to reveal the destiny of the germinal vesicle : that it disappears before the ovum leaves the ovary after impregnation, is all that can be affirmed ; the fissuring of the yelk is due, not to a cell development proceeding from the germinal spot as a centre, and attended by the solution of the primitive yelk, but is produced by the resolution of the primitive yelk, under the stimulus of impregnation, into smaller and smaller segments, in an increasing geometrical ratio, the alternate segments being converted into the cells from which the new being is built up.

The globules figured and described by Bischoff, which float in the fluid surrounding the contracted yelk before and after the fissuring has commenced, frequently shift their place, and might be taken for the nuclei of two cells which originate, according to Barry, from the enlarged and divided germinal spot. These two cells are, however, only the halves of the primitive yelk ; the constitution of the yelk is such as Bischoff has described—it is not composed of distinct and separable cells after impregnation, and we cannot fail to be struck with its resemblance to the parenchyma of the infusoria.

The spermatozoa do not enter the zona pellucida (*membrana vitelli*) by a distinct orifice, an explanation too mechanical to satisfy the physiologist. I believe that the globules above alluded to arise from the solution and attraction inwardly of certain spermatozoa, by vital powers resident in the ovum, and that the final cause of the fissuring of the yelk has been rightly assigned by Dr Barry ; to wit, the imparting to the ultimate segments of the yelk the peculiar influence of impregnation through the material medium of the dissolved spermatozoa, and furnishing thus an element in our investigations regarding the cause or causes which influence the transmission of hereditary peculiarities, bodily and mental, from the parents to the children. The albumen does not arise from the solution of the cells of the disc which accompany the ovum into the Fallopian tube—it is a new formation in the upper segment of the tube, and numerous spermatozoa are interstratified between its successive structureless layers.

I have not seen, in the numerous instances in which the ova have been observed for hours, the rotation of the ovum described by Bischoff ; but I would connect that phenomenon, supposing it correctly reported, with certain appearances in aborted ova of the doris, the knowledge of which I owe to that distinguished anatomist Dr J. Reid ; and it may be regarded as resulting from the pseudo-individuality manifested under certain conditions in aborted ova, before the extinction of their latent vitality.

I have also reason to differ from Dr Barry in various points regarding the origin and maturation of the ovarian ovum and its envelopes, but I hope to make fresh researches on that head.

ARTICLE III.—*Report of the Surgical Operations performed at the Newcastle-upon-Tyne Infirmary, during a period of seventeen years and a half.* By SAMUEL FENWICK, M.D., Lecturer on Pathological Anatomy at the Newcastle-upon-Tyne School of Medicine and Surgery.—(Continued from page 343.)

4.—*The Success of an Amputation is influenced by the duration of the disease for which it is performed.*

THE maxim has been laid down by an eminent surgical authority, that, in amputations for diseased joints, “the shorter the duration of the disease, and the less the system has suffered under it, the greater is the chance of recovery after amputations, and *vice versâ*.” As no facts of any importance are brought forward to verify this assertion, I considered its correctness well worthy of investigation. It would, indeed, seem probable, that if a person labouring under a disease of a scrofulous nature, have less tendency to secondary diseases after an amputation, than an individual in robust health, and the more the strength of a patient be reduced, the less will be his risk from these causes of death, and the only increase of danger he will be exposed to by his debility, will arise from exhaustion. But as in a former table we found that, in practice, but a small proportion died from shock or subsequent weakness, we might, from theory, reasonably feel inclined to call in question the truth of the assertion I have before quoted.

The next table is formed of amputations performed for diseased knee and ankle joints at the Newcastle Infirmary, classified according to the number of years they had existed previous to the operation. Under the head of total, we have the mortality, both inclusive and exclusive of those who died within the first four days after submitting to the amputation.

TABLE XIX.

A view of the Mortality at the Newcastle Infirmary following Amputations performed for Diseased Joints, at different periods of time after the commencement of the disease.

	Knee.			Foot and Ankle.			Total.					
							Including deaths in first four days.			Exclusive of deaths in first four days.		
	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.
One Year,.....	13	4	3.25	11	1	11.	24	5	4.8	23	4	5.75
Two Years,.....	8	9	17	17
Three ".....	2	1	2.	3	5	1	5.	4
Four ".....	2	4	6	6
Five ".....	1	1	1.	1	1	1.	1	1	1.
Above Five Years,.....	9	3	3.	6	15	3	5.	15	3	5.

Instead of the mortality being less in those cases of amputation in which the disease was of only one year's duration, it is greater in them than in those of any subsequent class. Rejecting the deaths occurring during the first four days as probably produced by shock,

we find that 1 in 5·75 died after that period in those whose disease had existed 1 year, whilst amongst those who had been affected between 1 and 4 years, none perished out of 27. Those who had suffered from disease for more than 5 years have been classed together, on the supposition that, as in some the complaint had existed for a very long period, the health would probably be but little affected, and that they had required the removal of the limb, rather on account of its inconvenience or deformity, than for the preservation of life. But as we before observed, that the greater number of deaths of persons submitting to amputation, and occurring after the period of shock, was produced by inflammatory or suppurative diseases, it is evident from this table that such patients as have laboured under diseased joints between 1 and 5 years, are less liable to be attacked by those complaints after the operation, than others who have been affected either within 1 year, or for a greater length of time than 5 years. If these results should be confirmed by other observations, the information to be derived from them will be exceedingly valuable; for it will appear that the impatience of the surgeon will be more generally to be dreaded, than danger from delay. I have seen more than one individual obtain a useful joint, where excellent surgeons had urged the necessity of amputation, and declared recovery without it as impossible; and I have on more than one occasion dissected joints, in which the knife of the operator had interfered, whilst nature was safely accomplishing ankylosis. The number of amputations performed for this class of diseases is every year decreasing as more simple and effectual methods of treatment are employed; and we have good reason to hope that, in a few years, the removal of a limb for a diseased joint will be as rare as it was formerly common.

Dr Lawrie has proved that the success of his predecessors at the Glasgow Infirmary has been greater than his own, and a greater amount of cures is also observable in the Newcastle Infirmary records, as having occurred a few years ago than can be boasted of at the present time. Thus, in the years between 1823 and 1834, omitting 1829 and 1831, in which the operations were not regularly recorded, there were 21 amputations for diseased knee, of which 2 died, or 1 in 10·5; and 22 for diseased foot and ankle, of which only 1 died; whilst, in the time from 1833 to 1842, there were 18 of the former operations and 3 deaths, or 1 in 6, and 16 of the latter amputations, and 2 deaths, or 1 in 8; and, even if we add the operations performed in the two years, in which all have not been recorded, we find in the former period 51 amputations of the lower extremities of which 6 died, or 1 in every 8·5, whilst in the latter there are 34 amputations and 5 deaths, or 1 death in every 6·8 operations. It is difficult to advance a satisfactory reason for this increase in mortality; but the most probable supposition seems to be, that it may have arisen partly from the greater number of persons received into the hospital; for, by the Newcastle Infirmary annual reports, it appears

that during the former more successful period only 7267 in-patients were dismissed cured, whilst the number in the subsequent and more fatal years was 9749.

The time required for recovery after the operation seems to have a certain relation to the amount of death ; thus, in those in whom the disease had existed only for 1 year, the time of cure was 56 days ; in amputations performed upon those who had suffered between 1 and 2 years, it was 44·6 days ; in those the duration of whose disease was between 2 and 5 years, it amounted to 38 days ; and of those who had been invalids above 5 years, it required 53·5 days to complete the cure.

From the above facts the natural conclusion appears to be, that so long as the life of the patient is not placed in immediate danger by his disease, we shall best consult his interest by deferring the operation, since, besides giving him a greater chance of a natural recovery, the amputation will be more successful, and less time will be required to heal the wound in case it be eventually required.

Of 9 cases of amputation for ulceration of the leg, in 3 the disease had existed 2 years or less, and all recovered ; 3 had suffered between 2 and 3 years, and 1 died ; whilst of 3 who had been affected for 10 years or upwards, only 1 recovered after submitting to the operation. The period required for cure in the first class was 55·2 days ; whilst of those whose complaint has existed above 2 years, the time of cure was 67·66 days.

There have been few points in operative surgery more fiercely discussed, than the proper time for amputation after the infliction of an injury. For a considerable period, the main argument used by both parties, was the comparative chance of death in traumatic and pathological amputations. This point we have before examined, and indeed, as it is easily seen, it is one which should be allowed to have but little weight in forming our opinion. Both parties have latterly appealed to the results of practice, and in the following tables all the statements I have been able to collect upon this point are produced :—

TABLE XX.

A comparison of the success following Primary and Secondary Amputations performed at the Newcastle Infirmary.

TRAUMATIC AMPUTATIONS.

	Primary.					Secondary.				
	No.	Dead	Av. Mort.	Cure.	Death.	No.	Dead	Av. Mort.	Cure.	Death
NEWCASTLE INFIRMARY.										
Injuries of Thigh,.....	8	5	1·6	70·33	6·
" Leg,.....	31	12	2·58	66·27	14·16	7	2	3·5	45·25	10·5
" Arm,.....	23	4	5·75	53·33	25·75	1
" Fore-arm,.....	6	1	6·	42·4	8·
Amputation of Shoulder Joint,.....	3	1	3·	72·	2·	2	1	2·	113·	1·
Total,.....	71	23	3·08			10	3	3·33		

TABLE XXI.

Shows the results of Primary and Secondary Amputations performed at various Hospitals.

	Thigh.				Leg.				Shoulder Joint.				Arm.				Fore-arm.			
	Prima.		Second.		Prima.		Second.		Prima.		Second.		Prima.		Second.		Prima.		Second.	
	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.
Liverpool Northern Hospital,	7	7'	3	1'5	21	5'25	6	3'	1	1'	6	3'	4	4'	7
Edinburgh Infirmary,	7	1'	2	1'	1
Glasgow Infirmary,	12	1'09	24	1'5	22	1'46	5	1'66	3	1'5	1	...	23	2'09	13	1'85	15	...	3	...
Glasgow Infirmary,	11	3'66	16	4'	4	4'	10	5'	5
Chester Infirmary,	2	2'	2	1'	3	1'	1	1'	1	...	1
Massachusetts and Pennsylvania Hosp. }	12	1'71	10	3'33	14	14'	15	1'87	1	1'	6	3'	5	...	10	...	6	6'
Mr Alcock,	19	1'46	24	1'71	4	2'	7	3'5	9	9'	1	1'	21	1'75	17	2'42	4	2'	5	2'5
University College Hospital,	2	2'	4	2'	2
Total,	72	1'63	63	1'7	86	2'6	34	2'12	19	3'16	2	2'	67	2'31	40	2'66	43	21'5	14	4'66

TABLE XXII.

Mr Alcock's Table of the results of Primary and Secondary Amputations in Military practice.

	Primary.			Secondary.		
	No.	Dead.	Av. Mort.	No.	Dead.	Av. Mort.
Baron Percy at Newbourg,	92	6	15'3
Baron Larrey,	14	2	7'
New Orleans,	45	7	6'42	7	5	1'4
Naval Action of June 1, 1794,	60	8	7'5
Battle of Aboukir,	11	3	3	1'
Battle of Camperdown, }	7
Dr Wright—Flag Ship }	8
Venerable,
Bombardment of } Upper extremity,	28	7	4'	2	2	1'
Algiers,	29	15	1'9
British Peninsular Army } Upper extrem.,	163	5	32'6	296	116	2'5
in six months,	128	19	6'7	255	149	1'7
Thoulouse—Mr Guthrie's } Upper extrem.,	7	1	7'	16	4	4'
Report,	41	9	4'5	37	18	2'
Navarino—M. Signore,	31	1	31'	38	13	2'8
Total	664	80	8'3	654	310	2'1

At the Newcastle Infirmary, the secondary amputations, where, as in injuries of the leg, we have the means of comparison, appear more favourable than those performed immediately after the occurrence of the accident. It is also worthy of observation, that the time required for the healing of the wound is considerably less in the former than in the latter; thereby confirming the truth of the rule, that the period of cure is less in a person submitting to the removal of a limb for disease, than when the operation is performed upon an individual in perfect health.

The difference in the results furnished by tables 21 and 22 is particularly striking. In the latter table the primary amputations

are exceedingly successful, the average mortality being 1 death in every 8·3 amputations; whilst, in those performed at a subsequent period, 1 death has taken place out of every 2 cases. Or, again, if we look at the statement of the mortality after amputations of the upper extremity, as given in table 22, and which were performed in the British army, we find 1 out of 2·5 died of the secondary, and only 1 of 32·6 of the primary class of operations. But on the other hand, the records of civil practice place the matter in a different light. Here, in some hospitals, as in the Glasgow and Newcastle Infirmaries, the advantage is with the delayed operations; and even taking the general average mortality of the whole of the hospitals, the difference of mortality after amputations of the thigh and arm, between the primary and secondary amputations, is so slight as to be scarcely worthy of notice.

It has always appeared to me, that the mere knowledge of the amount of death following primary and secondary amputations, is not of much importance in enabling us accurately to determine the question of their comparative value. The original point of dispute appears to have been, whether the surgeon would save most lives by amputating a limb immediately after the infliction of an injury, or by deferring the operation to a future period. If, then, we imagine amputation deferred in twenty cases of serious accidents, in which the powers of nature are obviously unable to repair the injury, the question to be decided must be, not so much as to the mortality which will occur amongst those on whom the secondary operation is performed, as the number of the patients who will reach that state in which the limb can be removed. For if a person suffer a compound fracture, or other severe injury, he becomes liable to attacks of phlebitis and visceral diseases, in the same manner as if he had undergone amputation. Some have supposed, that by obtaining the number of deaths which have resulted amongst any number of compound fractures treated without amputation, we might calculate the proportion which would perish out of an equal number of cases deferred for secondary amputation. But it will be observed that, as the cases of compound fracture treated without amputation are selected as likely to terminate well, they can afford us no guide as to the mortality which is likely to occur from diseases amongst accidents of a more severe character.

From the great danger to which every day's experience teaches us, severe accidents are liable before secondary amputation can be undertaken, the conclusion is manifest, that we cannot be justified in deferring the operation, where the powers of nature will be evidently unable to repair the injury. But, on the other hand, the mortality after primary amputations likewise warns us against being, in the first instance, too solicitous to remove a limb, whose injury it is possible nature might repair, under the supposition, that in so doing we place our patient in a state of comparative safety.

We have before seen that different diseases appear at different

periods after the performance of an amputation, and that the occurrence of inflammatory complaints is chiefly to be dreaded in persons whose strength has not been reduced by long-continued disease. Experience also teaches us, that during the first three weeks after the infliction of a severe injury, as a compound fracture of the leg, the same disposition to visceral and inflammatory diseases is manifested, and that the amputations which are performed during that period are exceedingly unsuccessful. After, however, the suppurative stage has commenced, amputation may be resorted to, with a chance of success approaching that of the operation when performed for a long standing disease.

In order to show the difference in mortality following amputations performed at these different periods after an accident, I have added Table XXIII., which is extracted from Mr Alcock's valuable lectures on amputation.

TABLE XXIII.

Shows the difference in Mortality between Intermediary and Secondary Amputations.

	Thigh.			Leg.			Arm.			Fore Arm.		
	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.	No.	Dead	Av. Mort.
INTERMEDIARY.												
Mr Alcock,.....	9	6	1.5	4	2	2.	11	6	1.83	3	2	1.5
Mass. and Penns. Hosp.	4	3	1.33	7	4	1.75
Total,.....	13	9	1.44	11	6	1.83	11	6	1.83	3	2	1.5
SECONDARY.												
Mr Alcock,.....	15	8	1.87	3	6	1	6.	2
Mass and Penns. Hosp...	6	8	4	2.	5	6	1	6.
Total,.....	21	8	2.62	11	4	2.75	11	1	11.	8	1	8.

The difference in mortality is here very striking, and shows most distinctly the greater chance of recovery of a patient after an amputation performed during the suppurative stage. We here find a ready explanation of the discrepancies of various authors, with respect to the amount of death after the delayed operation; for where a surgeon makes it a practice only to amputate in the secondary period, that is, after the 21st day, his success is certain to be vastly greater than another, who attempts to save his patients by having recourse to the operation in the intermediary period, when inflammatory complaints are so apt to occur:—the former may be able to boast of only 1 death in 6 of those in whom he removes the arm, whilst the latter may be compelled to allow an average mortality of 1 in 1.83 after the performance of the same operation.

It is likewise remarkable how slight is the difference in the intermediary period between the mortality after amputations of the thigh, and that occurring after those of the arm; whereas in the secondary

period the difference is very observable; showing that in the former the danger from any operation is so great, that the amount of a limb removed has but comparatively little effect upon the result. The obvious conclusion from such a fact is, that we should avoid the performance of any operation upon a compound fracture during the first three weeks after an accident, providing it can be safely deferred until the suppurative stage has commenced. Table X. shows us the most frequent causes of death after the primary and secondary operations. It is there seen, that whilst in the former the chance of fatal shock, exhaustion, and secondary hemorrhage is less, the liability to secondary inflammations, as phlebitis and erysipelas, is considerably greater. In visceral inflammations, the danger from both is, however, nearly equal, which most probably arises from the operations performed during the intermediary being classed along with those which took place in the secondary period. Gangrene produces death, as might be expected, more frequently in the primary operations.

Mr Alcock has given us the following account of the causes of death, of amputations performed in his own practice, classified according to the period at which the operations took place. In the primary amputations, the average mortality from shock was 1 in 57; in the intermediary period, none died of 27 cases; whilst of the cases of amputation performed after the twentieth day, 1 out of every 8·66 sank from exhaustion or shock. Thus, of all the classes of amputation which we have hitherto examined, that consisting of the operations performed after the twentieth day after the receipt of a severe injury, incurs most risk from shock; and herein is the character which most distinguishes the mortality following secondary amputations, from that observed after amputations for long standing diseases.

Again, whilst, in Mr Alcock's practice, in the primary amputations 1 case in every 9·5 was affected with phlebitis, and 1 in every 3·56 with visceral inflammations or abscesses, and whilst of the intermediary operations 1 in 9 suffered from the former, and 1 in 5·4 from the latter diseases, no death occurred amongst 26 secondary operations from these causes.

On the other hand, according to the same author, 1 in every 6·5 amongst the secondary amputations perished from hectic or exhaustion following some time after the operation, whilst none amongst either the primary or intermediary classes sank from these affections.

These facts most strikingly confirm the view formerly given, with respect to the necessity of antiphlogistic treatment after primary amputations, and show us that the more the patient is reduced previous to the operation, the less danger is there of his becoming affected by those serious secondary inflammations which cause so much of the mortality observed after the performance of this operation on healthy individuals.

But a question has been raised with respect to the time after an accident at which it is proper to perform a primary amputation; whether the limb should be removed whilst the patient is in a state of depression, or the operation be delayed until reaction take place. Common sense, as well as the results of experience, decides the matter at once in favour of the latter plan; and I have seen, I believe, many deaths occur after amputations, from the shock of the operation being inflicted whilst the patient was suffering from that of the accident. Table XIV. shows us that the chief mortality within the first four days after amputations for accidents, is in the thigh operation; it is, therefore, necessary to remember, that in all amputations at this part, on account of the operation itself inflicting a greater shock on the system than when performed elsewhere, a larger interval of time should be allowed to elapse between the receipt of the accident and the application of the knife.

5.—*The Success of an Amputation is influenced by the method by which it is performed.*

There is perhaps no dispute with respect to amputations, in which we find so many theoretical arguments, and so few facts—such sweeping conclusions, and such a paucity of experience brought forward to justify them—as in the controversy regarding the flap and circular operations. I would not, indeed, have thought it worth while to make the following remarks, on account of the few facts I can bring forward, had it not been that the authors of some of our most recent and best works on operative surgery, have written on the question as though it were already settled, and as though the flap operation were so much superior to the more ancient method, that it was almost needless to describe any other. It is not, unfortunately, recorded in the register of the operations performed at the Newcastle Infirmary, in which case the flap and in which the circular method was employed; but I have been able to form an approximate estimate of the comparative success following the employment of each, by separating the operations of each surgeon. For a long series of years, the late Mr Baird was the only operator who employed the flap method, which, as far as my recollection extends, he did on all occasions in the lower extremity, always, however, removing the upper by the circular operation. We have, therefore, to compare the results of his practice with the success of his colleagues.

TABLE XXIV.

A Statement of the comparative success following the Flap and Circular methods of performing Amputations of the Lower Extremity at the Newcastle Infirmary.

	Thigh and Knee,					Leg and Ankle,				
	No.	Dead	Av. Mort.	Cure.	Death,	No.	Dead	Av. Mort.	Cure.	Death.
DISEASED JOINTS & BONES.										
Mr Baird—Flap.....	10	3	3·33	45·7	9·60	9	1	9·	54·16	60·
Other Surgeons—Circular	39	6	6·5	49·85	18·83	40	2	20·	51·67	47·5
TRAUMATIC AMPUTATIONS.										
Mr Baird—Flap.....	1	1	1·	...	2·	8	3	2·66	76·33	5·66
Other Surgeons—Circular	7	4	1·75	70·33	7·	30	11	2·72	57·75	16·66
OTHER PATHOLOGICAL AMPUTATIONS.										
Mr Baird—Flap.....	6	1	6·	46·6	8·
Other Surgeons—Circular	1	50·	...	19	11	1·72	61·28	17·27

This table displays a rate of mortality very different from what I expected to have found before I collected the facts from which it is compiled. In every instance, except after amputations performed for ulcers and malignant diseases, the advantage is on the side of the circular; and this is the more extraordinary, since, from the year at which this report closes, the other surgeons of the hospital have chiefly practised the flap operation, which is, in fact, now as generally used as the other was a few years back. It is, however, but just to observe, that the practice of Mr Baird may not give us a fair means of comparison, as he frequently undertook operations which more cautious surgeons might perhaps have declined; and although, by his boldness, I have seen cases rescued from the grave, yet the greater number of dangerous amputations he performed, may in some degree have contributed to cause the excess of deaths above observed. In the same way, perhaps, we may account for the lengthened time of cure; for I never saw, in the practice of any other individual, so many stumps heal by the first intention.

The conclusions which we might be inclined to draw from Table XXIV. are to a certain extent borne out by the following table:—

TABLE XXV.

A comparison of the Mortality observed after Flap and Circular Amputations by the two under-mentioned Authors.

	Circular.			Flap.		
	No.	Dead.	Aver. Mort.	No.	Dead.	Av. Mort.
THIGH.						
Dr M'Hardy,	100	28	3·57	102	28	3·64
Mr Alcock,	32	20	1·6	11	7	1·57
LEG.						
Dr M'Hardy,	43	7	6·14	12	4	3·
Mr Alcock,	9	3	3·	2	1	2·
ARM.						
Dr M'Hardy,	30	9	3·33	24	5	4·8
Mr Alcock,	34	16	2·1	4	3	1·33
FORE ARM.						
Dr M'Hardy,	26	2	13·	27
Mr Alcock,	7	3	2·3	2	1	2·

Merely looking at the mortality, as thus stated, we find that the success of each method of amputation is about equal in the operations of the thigh and arm; or, at any rate, the advantage which in either case is gained in the statistics of the one author, is lost by those of the other. In the leg amputations, both reports agree in favour of the circular; a considerable excess of mortality being observable when the flap operation has been employed. In the fore-arm, on the contrary, the latter method has been most successful; for although, in Mr Alcock's account of his practice, the average mortality is rather smaller, it is not so much so as to balance the greater number of deaths following the circular operation, as given in Dr M'Hardy's statements.

It has been before seen, that phlebitis and purulent deposits constituted by far the most fatal class of diseases occurring after amputation; the comparison, therefore, of the liability of a number of persons to these affections, after submitting to the different methods of operation, is highly important. Dr M'Hardy, out of 165 flap amputations, gives 12 cases of phlebitis as having occurred, or 1 in 13·75; whilst, of 199 circular, only 9 were affected with it, or 1 in every 22·11. Again, Mr Alcock states, that out of 87 circular operations, 10 died of phlebitis, or purulent deposits, or 1 in every 8·7; whilst, of 24 flap operations, 6 perished, or 1 in every 4, from the same class of diseases. Taking, then, the general amount of death as caused by these diseases, the advantage is vastly in favour of the circular operation; or, if we confine our attention only to the primary amputations, Mr Alcock informs us, that of this class, after the circular operation, 1 in 6·5 died of phlebitis, or purulent deposits; whilst, after the flap amputations, 1 in 3 died of phlebitis, or purulent deposits. If we investigate the frequency of their occurrence, with respect to the site at which the amputation is performed, we find a somewhat different statement. According to Dr M'Hardy, 1 in 24 was attacked by phlebitis when the arm was removed by

the flap, and none out of 27 cases of the fore-arm amputation when the flap method was employed; whilst, after the circular operation, 1 case of phlebitis took place in every 10 when it was performed in the former situation, and 1 in every 26 when in the latter. The comparative liability of the two classes of operation to phlebitis, is reversed in the lower extremity. In the thigh operations, according to the same writer, the employment of the flap was followed by phlebitis once in every 12·75 cases, and, in the leg, once in every 4 operations; whilst, after the circular, 1 in every 33·33 was attacked by the same disease when the amputation took place in the former site, and 1 in every 21·5 when it was performed in the latter.

These are very important statements, as displaying the greater chance of the occurrence of this formidable disease in the most dangerous amputations, when they are performed by the flap operation. The liability to secondary hemorrhage is about equal after both plans of operation. Thus, according to the author last quoted, in the flap amputations 1 in 12·68 was affected by it; whilst, in the circular one, 1 in 13·26 suffered from the same accident; and in Mr Alcock's practice, after the former method 1 in every 6, and, after the latter, 1 in every 5·4 was affected by secondary hemorrhage. As, however, this accident seldom proves fatal, excepting after the thigh operations, we should chiefly direct our attention to the chance of its occurrence after removal of this part. Dr M'Hardy gives 9 cases of it as having occurred out of 100 circular, and 9 out of 102 flap amputations of the thigh. We need not, therefore, consider the liability to secondary hemorrhage as affording any argument against the employment of either the one or the other method of operating.

But if the circular operation is less exposed to the occurrence of phlebitis, there can be no doubt that the advocates of the flap can bring forwards greater rapidity of execution, better stumps, a less liability to necrosis, and a shorter time of cure, for their favourite operation. With respect to the last of these advantages, Dr M'Hardy found that 1 stump in every 4·73 heals by granulation after the employment of the circular; whilst, after the flap operation, only 1 in every 7·85 did not unite by the first intention.

The question, then, which it is necessary to settle, before determining which description of operation we will perform upon any particular case, appears to be, whether the advantages of a better stump, and a quicker recovery, are sufficient to outweigh an increased liability to phlebitis. In amputations for diseased joints, in which phlebitis and secondary inflammations are of comparatively rare occurrence, and in which necrosis and conical stumps are, according to my own observation, most likely to take place, the flap operation appears, in most cases, to be indicated, more especially if the part to be removed be the upper extremity, or the patient be young. On the contrary, in traumatic amputations of the lower extremity, the circular operation would seem most advisable, as, in such cases, plenty of covering can be always obtained for the ends of the bones; whilst, at the same time, we subject the patient to a less danger of

phlebitis and secondary inflammations than by the other method. It seems somewhat strange, that, notwithstanding the long disputes upon the merits of these different plans of amputations, so few facts should have been collected; and I fear we shall have to wait for more detailed and accurate investigations, before we can venture fairly to decide upon their respective merits.—(*To be continued.*)

ARTICLE IV.—*Anæsthetic and other Therapeutical Effects of the Inhalation of Chloroform.* By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh, &c.

AT the first winter meeting of the Edinburgh Medico-Chirurgical Society (10th November), I directed the attention of the members to a new respirable anæsthetic agent which I had discovered a short time previously,—viz., Chloroform, Chloroformyle, or Perchloride of Formyle. In this limited notice, I shall state briefly some of the principal facts pertaining to its history, composition, effects, &c.

Chemical History and Composition.—Chloroform was discovered at nearly the same time by Soubeiran (1831), and Liebig (1832). Its chemical composition was first ascertained by Dumas and Peligot (1835). It consists of 2 atoms of carbon, 1 of hydrogen, and 3 of chlorine; or, to express it otherwise, of 1 atom of formyle, and 3 of chlorine. Hence its chemical formula is C_2HCl_3 ; or $FoCl_3$.

Modes of Preparation.—It may be obtained by various processes. 1. By the distillation of a mixture of diluted spirit, pyroxylic or wood spirit, or acetone, and chloride of lime (bleaching powder); or, 2. By making milk of lime, or an aqueous solution of caustic alkali, act upon chloral; 3. By leading a stream of chlorine gas into a solution of caustic potass in spirit of wine, &c.

Physical and Chemical Properties.—It is a clear limpid liquid, as heavy as 1·480; not inflammable; very volatile; and boils at 141° . It has a fragrant, fruitlike odour; and a sweet saccharine taste.

Therapeutic History.—It has been used internally. Guillot employed it in asthma, diluted with water one hundred times (1844). My friend, Dr Formby of Liverpool, told me, about two years ago, that he used it often in a diluted form as a diffusible stimulant; and I have, since that period, frequently prescribed it instead of valerian, camphor, &c.¹ But I am not aware that any person has used chloroform by inhalation, or discovered its remarkable anæsthetic properties till the date of my own experiments.

Advantages as an Anæsthetic Agent.—In producing insensibility to pain in surgical and obstetric practice, chloroform possesses various important advantages over sulphuric ether. 1. A greatly less quantity of chloroform is required; 2. Its action is much more rapid, more perfect, and generally more persistent; 3. Its exciting or exhilarating stage is far shorter, insensibility commonly supervening in a minute or two, or less; hence, 4. The time of the surgeon is saved; 5. The inhalation and influence of it are more agreeable and pleasant; 6. Its odour is evanescent; 7. No special instrument is required for its employment.

Dose and Mode of Exhibition.—A fluid drachm or two of the liquid, diffused upon the interior of a pocket-handkerchief, arranged in a concave or cuplike form in the hand of the exhibitor, and applied over the nose and mouth of the patient, generally suffices to produce rapid and complete anæsthesia. A few patients may require more, others less. To keep up its action, when that is necessary, the handkerchief must be again besprinkled with the fluid when the first quantity is evaporated. The moistened handkerchief should be at first held at the distance

¹ Since first publishing on the subject of chloroform, Dr Glover of Newcastle has pointed out to me, that, in an essay on Bromine in the 152d number of the Edinburgh Medical and Surgical Journal, he mentions having poisoned several animals with chloroform, by injecting it into their veins, stomach, and the cavity of the peritoneum.

of about half an inch from the face, and gradually approached nearer. The patient should, if possible, be placed easily and upon his back, and advised previously to take full inspirations. All noise and excitement around the patient should be strictly and peremptorily forbidden.

Physiological Effects.—After the first two or three full inspirations, a feeling of warmth and excitation, radiating from the chest to the extremities; followed by whirring noises in the ears; a sensation of vibratory thrilling and benumbing throughout the body; with, betimes, rapid loss of sensation and of motion, and at last of consciousness. Often before total unconsciouness supervenes, the patient, guided by instinct rather than by volition and reason, makes an effort to get rid of the inhaling vapour and handkerchief, as if it interfered with free respiration. During the full anæsthetic sleep produced by chloroform, sometimes no mental action goes on, or at least is remembered; in many others, the mind is active as in dreams. The respiration is usually at first soporose; the pupil sometimes natural, in others slightly contracted, in others dilated; the pulse is usually quickened ten or twenty beats at first, but afterwards falls to its normal rate, and, if the vapour is exhibited very long in very powerful doses, it comes down more and more below the natural standard; muscles of voluntary motion in general relaxed; more rarely cataleptic; still more rarely clonically contracted, as happens also occasionally with ether. In small doses, given slowly, its effects are exhilarating; and exactly like those generally following the inhalation of nitrous oxide gas.

Uses in Surgery.—1. To relax the muscles in reducing dislocations, &c.; 2. To avert the sufferings attendant on deep probing, and other painful but necessary modes of diagnostic examination and dressing; and 3d, and principally, to annul the pain of operations by the caustic, ligature, or knife.

Examples.—I. A child of ten weeks old had a very large nævus behind the ear. Dr Duncan destroyed its internal organization by passing large red-hot needles in different directions through it. While the tumour was hissing and decomposing under their action, the infant lay quietly and placidly asleep on my knee, under the influence of chloroform. This is the youngest subject to whom I have given it.—II. A boy, of four or five, had a necrosed radius cut down upon, and removed by Mr Miller. He slept soundly during the operation; and, without moving, he was carried out of the operation theatre of the hospital still fast asleep. When visited some time afterwards, he was found awake in bed, with a bright merry eye, as if just out of a refreshing sleep. No pain even then.—III. A nervous woman, a patient of Mr Miller's, was to undergo partial amputation of the foot in the hospital—afraid both of the operation, and of being carried in before a crowd of medical men for the purpose. I apathized her with chloroform in the consulting-room of the hospital, had her carried into the operation-room in that state, and did not allow her to awake till the amputation was performed, and she was removed back again to bed. She was thus entirely spared both the moral shock and physical pain which she dreaded.—IV. A boy had his elbow-joint excised by Mr Syme. The operation, which is always a very painful one, was prolonged in consequence of the very diseased state of the parts operated on. He slept soundly, and remained perfectly and passively still during the whole operation.

Uses in Midwifery.—To diminish and annul the physical pains attendant on labour, and more especially those which accompany the passage of the child's head through the pelvic cavity and outlet, (the second stage of Denman.)

Examples.—I. The lady to whom it was first exhibited had been previously delivered in the country by craniotomy after a very long labour. Her second confinement took place a fortnight before the full time. Chloroform was begun to be inhaled when the os uteri was becoming well expanded, and the pains very severe. In twenty-five minutes the child was born. The mother did not awake till after the placenta was removed; and was perfectly unaware that her child was born and alive. She stated her sensations to be those of awaking from "a very comfortable sleep."—II. I exhibited it, with Mr Carmichael, to a patient who had, at her preceding confinement, been in severe labour for twenty hours—

followed by flooding. She began the inhalation when the dilatation of the os uteri was half completed. The child was born in fifty minutes afterwards. She was kept under its influence for a quarter of an hour longer, till the placenta was removed, and the binder, body, and bed-clothes, all adjusted. On awaking, she declared she had been sleeping refreshingly; and was quite unconscious that the child was born. No flooding. An hour afterwards, she declared she felt perfectly unfatigued, and not as if she had borne a child at all.

In labour it does not require to be given in such large doses as in surgery. After the first full dose, a few inhalations before each returning uterine contraction is generally sufficient. It should be made more deep as the head is passing the perineum and vulva. If the state is extremely and unnecessarily deep, it will no doubt diminish and even temporarily stop uterine contractions; and advantage may be taken of this in facilitating the operation of turning, &c. Besides thus, 1. Diminishing or annulling the more severe part of the sufferings attendant on natural labour, it will, 2. Abolish those more agonizing pains which accompany the use of the forceps, and other modes of operative delivery; 3. Enable us to extract the placenta artificially when required, without resistance or suffering; and 4. Give us the power of making an accurate and full examination of the presentation, when necessary early in labour, as in placenta previa, preternatural presentations, &c.

Uses in Medicine.—1. As an *anti-spasmodic*; as in asthma, laryngismus, tetanus, and other spasmodic diseases, &c. I have used successfully the inhalation of ether to arrest the paroxysms of whooping-cough, dysmenorrhœa, colic, and the pains attendant on the passage of biliary calculi. In a case of the most severe, at the same time painful, spasmodic twisting and convulsions of the extremities attending a second attack of chorea, I allowed the patient ether-inhalation; and sometimes she lay under its influence for hours, with relief while its action lasted, but generally without sleep. Latterly the chloroform has both relieved the spasms and their attendant pain, and procured sleep.—2. As an *anodyne* or *narcotic*. In neuralgia, I have seen chloroform stop the fit at once; in two other cases the pain remained absent only while the chloroform acted. A patient suffering under severe delirium tremens had remained awake for about seventy hours; a half ounce of laudanum, given at a single dose, failed to produce rest; ten hours afterwards, the inhalation of chloroform was immediately followed by several hours of critical sleep. What cases of insanity would it benefit? I have exhibited it in full doses in some cases of dementia, combined with excitement and wakefulness. They were all asleep in about a minute—and remained so for some time. In nothing does chloroform differ from ether more than in its soporific effects—when given in full doses, and continued for some time.—3. In small doses as a *diffusible stimulant*; to arrest the first commencement of ague, ephemera, &c.; in hysteria, &c. Perhaps it may be used by inhalation in small quantities when the stomach will not bear wine or other stimulants; in severe vomiting, fevers, &c. I have seen its inhalation at once dispel a sick headach.

Cautions.—The liquid used should be sufficiently strong. Its proper sp. gr. is (as I have said) 1·480. It is certainly far too powerful an agent to be intrusted to nurses or unprofessional individuals. I have given it, up to this date, to above eighty persons, without the slightest bad result of any kind whatever in any one of them. The power, however, which we have with it, of bringing down the pulse, &c., shows that, if exhibited in too strong a dose, and given *uninterruptedly* for too great a length of time, it would doubtless produce serious consequences, and even death. But, certainly, all its *full* anæsthetic and other influences may be perfectly obtained without allowing it to produce such depression as would be in any degree dangerous. Like many other agents, it may be powerful for evil as well as for good. I believe its great potency will be one great safeguard against its abuse.

Its influence upon the blood, &c., the counter-indications to its use, &c. &c., remain still to be ascertained.

Edinburgh, 25th November 1847.

Part Second.

REVIEWS.

The Human Brain, its Structure, Physiology, and Diseases. With a Description of the Typical Forms of Brain in the Animal Kingdom.
By SAMUEL SOLLY, F.R.S., &c. Second Edition. London: 1847. Pp. 688.

THE first edition of this work was published in 1836, in which Mr Solly had the great merit of rendering the anatomy and physiology of the brain much more clear and definite than any previous writer. He pointed out that the method of dissecting this organ practised by Gall, that is, by tracing its fibres from below upwards, was the only method of enabling us to understand the relation of its intricate parts, and the functions performed by each. He satisfactorily showed that the office of the gray portion was to originate nervous power, and that of the white portion was to conduct the influence therein generated. Not, indeed, that he could even then be considered as the originator of these views; but that the facts and arguments he brought to bear upon them, derived from comparative anatomy, physiological experiments, and pathological investigations, caused them to be more readily received by the profession. In a paper read to the Royal Society in May 1846, he was the first to describe certain filaments running from the anterior columns of the spinal cord, below the olivary body, to be expanded in the cerebellum, which fibres, he further showed, constituted one of the organs of the *portio dura* of the seventh pair of nerves. We have always considered that these dissections were of the greatest service to pathology, as explanatory of why partial paralysis of the face should follow the law of crossed action, from lesions of the brain.

The present edition is greatly enlarged, principally by compilations from numerous sources, with the view of causing the work to represent the present state of our knowledge on the anatomy, physiology, and diseases of the brain. An entire section has been very properly added on the structural anatomy of the organ, constituting Part I.; another on its weight, being Part IV.; whilst the last Part, which bore the former modest title of "physiological inferences from pathology," is now converted into a treatise on diseases of the brain, which occupies the goodly space of 330 pages.

After a careful perusal of Mr Solly's volume, we are disappointed to find that he has not made any *personal* endeavours to clear up any of the interesting questions which have arisen, concerning the physiology and pathology of the nervous system, during the last eleven years. Part I., on the structural anatomy of the brain, is a mere compilation from all imaginable sources, authenticated works

as well as anonymous reviews. The illustrations are, for the most part, copied from Todd and Bowman; indeed, only one is drawn from nature, and that with the assistance of another observer.

The Second Part gives one of the best expositions with which we are acquainted of the comparative anatomy of the brain and spinal cord, and is admirably illustrated by numerous wood-cuts.

Part Third is on the protective apparatus of the human brain, and here directions are given for opening the skull and vertebral canal.

The student should place the subject on its face, and, raising the head, rest the chin upon a block, so as to fix it in a horizontal position.

An incision must then be made through the scalp, extending across the vertex from ear to ear. The anterior part of the scalp may then be forcibly torn, instead of being dissected, from the skull over the face, and the posterior over the occiput, which will save much time; but some force is required to effect this reflection of the integuments.

A deep groove must be made with the saw through the outer table and diploe, commencing half an inch above the superciliary ridges anteriorly, and extending round the entire skull to the protuberance of the os occipitis posteriorly.

A small axe should next be used to break the inner table, which is much better than sawing it entirely through, as being less likely to injure the dura mater and brain, and as permitting the skull to be more firmly fixed again when replaced after the dissection is completed.—Pp. 141, 142.

This mode of removing the skull-cap is undoubtedly the best, except in cases of external violence, where fracture of the cranium is to be apprehended. We have seen want of attendance to this rule produce great perplexity, especially in medico-legal cases. A wooden mallet is much preferable to a small axe for the purpose.

The general description of the membranes is good; that of the arachnoid is principally taken from the description of Dr Knox. We are told, in a note, that Mr Rainey has demonstrated the existence of a very abundant supply of branches from the sympathetic nerve to the arachnoid. We would rather have been informed that Mr Solly had taken some pains to ascertain the correctness of Mr Rainey's statements on this matter.—(See our No. for Feb. 1847).

Part Fourth, on the weight of the human brain, is principally made up of lengthy quotations from the papers of Drs Reid and Peacock, published in this Journal for April 1843, and August 1846. It contains all that is known of the subject.

Part Fifth gives an excellent account of the general configuration of the brain; a subject very little understood from its perplexity. We have no hesitation in stating, that in our opinion Mr Solly has rendered this part of the work unusually clear by his able description, and the admirable wood illustrations which accompany the text.

Parts Six, Seven, Eight, and Nine, treat of the descriptive anatomy of the brain and spinal cord, and of the developement of the nervous system. In these chapters Mr Solly evinces a thorough practical knowledge in dissecting the organs described, and his account in

consequence is clear, and easily understood. It is this part of the work which we regard of the greatest value, containing as it does not only a condensation of all that has been made known by other anatomists; but the original views which have resulted from the author's own investigations. The following succinct review of the whole inquiry, given in the form of a recapitulation, is so good that we extract it entire.

In the first place, we have an extensive surface of cineritious neurine, the *hemispherical ganglion*, (speaking merely of one side of the brain,) which, in the higher orders of animals, is convoluted or folded in a peculiar manner.

In apposition to the whole of the vesicular neurine of this ganglion, there are tubular fibres which radiate through it, and are encrusted by its nucleated cells.

These fibres are disposed of in four different ways; 1st, some of them, commencing from the convolutions of the anterior, middle, and posterior lobes, pass through the corpora striata, and, forming the inferior layer of the crus cerebri, pass through the pons Varolii, so as to form the anterior columns of the cord, as previously described—the *motor tract*: 2nd, others, commencing in the nerves of sensation, and after passing through the pons Varolii, and emerging from the substance of the thalamus, terminate in the same neurine that gave origin to the last—this is the *sensory tract*: 3rd, others, passing from one side of the brain to the other, and in apposition to the internal surface of all the convolutions, are those fibres which, collected into a mass, form between the hemispheres that wide bridge, if I may so call it—the *great transverse commissure*, or *corpus callosum*: 4thly and lastly, in contact with all the convolutions are the fibres of the superior and inferior longitudinal commissures, which, connecting together those convolutions which are situated on the same side of the mesial line, or different portions of the same hemispherical ganglion, so far differ from the transverse commissure, which connects those situated on opposite sides, or the two distinct but corresponding ganglia.

The first and second set of fibres, which radiate from the external surface of the two large ganglia of the anterior and posterior columns, as from a common centre, forming however, in their radiation, only half a circle, were designated by Gall and Spurzheim the *diverging fibres*. The third set of fibres, which converge towards the centre of the brain, the transverse commissural, were distinguished as the *converging fibres* by the same author.

The above descriptions demonstrate that the encephalon or brain in the human subject is not a large solid mass of matter, in the interior of which are cavities scooped as it were out of its substance to be appropriately denominated ventricles, but that it really consists of ganglia or collections of cineritious neurine, placed on each side of the mesial line. Some of them being the appropriate ganglia of the nerves of sensation; as, for instance, the olfactory ganglia, the optic ganglia or tubercula quadrigemina, the auditory ganglia or posterior pyramidal bodies, the pneumogastric ganglia or restiforme ganglia, the olivary bodies or lingual ganglia; the others being the motory and sensory ganglia, as the corpora striata and thalami nervorum opticorum. The hemispherical ganglia again, that they might present the greatest possible extent of surface, are folded up into innumerable plaits, and thus cover or surround every other ganglion within the cranium, so that on first removing the skull-cap nothing can be seen but the convoluted surface of these extensive ganglia.

And here let me insist upon this important principle in the study of the brain, which is also one of the first ideas that the student should acquire regarding its composition, namely, that it consists of corresponding or symmetrical parts on each side of the mesial plane, and that instead of regarding the fissures of separation between its different portions as forming ventricles or cavities, he must direct his attention to the ganglia which bound the fissure, and the structures called commissures, which, connecting them together, cross the fissure and necessarily alter its character in different points, masking it, it is true, but not at

any place changing the fissure into a true bag or circumscribed cavity. The third, the iter a tertio ad quartum ventriculum, the fourth, and fifth ventricles, we have already seen, are in truth no more than the successive dilatations from below upwards of the posterior fissure of the cord; difficult enough to be understood when these are viewed in different situations and unconnected one with the other, as in the ordinary mode of dissecting the brain, but which seem necessary and obvious where its parts are traced in connection with one another.—Pp. 280, 282.

With the general physiological views and deductions given in Part Ten, we for the most part concur. Since the publication of the first edition, it would appear that the author has been converted to the doctrine of phrenology. His reasons for this are stated, p. 339; but to us they are exceedingly unsatisfactory, and even unintelligible. We are at a loss, for instance, to understand how the existence of longitudinal commissures in any way supports phrenology. The question of phrenology we consider to be this, not whether the brain be the organ through which the mind is manifested, and which influences the superiority of the latter by means of its more or less perfect development; not whether different portions of its substance perform different functions: but, whether the various faculties of the mind are circumscribed to the particular parts of it pointed out by Gall and Spurzheim or their disciples, and whether their size may be determined by an external examination of the cranium? These two latter points are the only ones peculiar to phrenology; all others having been long admitted by physiologists. Now, we contend that these have never yet been proved, and, did our space permit, could easily demonstrate their fallacy. Pathological facts alone, by showing destruction of the particular organ on *both* sides, whilst the function it is supposed to originate remains intact; and again, the absence of the functions, whilst the corresponding portions of brain on which it is said to depend are well developed, and of normal texture—must at least forbid any pathologist, worthy of the name, from giving in his adhesion to such speculations.

Part Eleven treats of diseases of the brain, and here we think that Mr Solly has placed too much dependence upon the statements of others. The opinions of the latest writer seem to be adopted as a matter of course, without any apparent reflection; and certainly without any pains having been taken to test their accuracy. Moreover, the plan and arrangement of this new treatise seem to us faulty in the extreme. The diseases of the brain are divided into anæmic, hyperæmic, convulsive, and organic affections. Under the head of anæmic affections we find *ramollissement*, which perhaps is as pure a specimen of organic alteration as is to be met with. The idea that convulsive disorders can be separated from anæmic, hyperæmic, or organic changes, is, to say the least of it, very extraordinary—lastly, confining the term organic diseases of the brain, to “those morbid growths from the neurine of the encephalon or its membranes, which are not the result of simple hyperæmia of these structures,” betrays a very limited notion of the term organic.

At the very outset of the pathological part of the work, we are

met with a striking instance of that dependence upon the latest published opinion, of which we complain generally. Thus, Dr Burrowes, having lately brought forward certain views on the cerebral circulation, which he imagined were sufficient to overthrow the careful experiments and well-founded arguments of Monro, Kellie, and Abercrombie, these views are adopted as a matter of course. There has been no examination, no investigation into their merits. But the readers of this Journal, and all who have been educated in the Edinburgh School of Medicine, we hope, are not likely to be led astray so easily, notwithstanding Dr Burrowes' researches are denominated by Mr Solly "valuable and conclusive." In an article published in the August Number of this Journal for 1846, it was clearly proved, that so far from these researches being valuable and conclusive, they were fallacious in the extreme; that Dr Burrowes himself did not understand the doctrine he endeavoured to overthrow; and that his experiments, pathological observations and deductions, were meagre, unsatisfactory, and unsound. That article, we know, has been very extensively read, and has been considered unanswerable by all who have seriously studied the subject. We have lately had occasion to notice how one hospital surgeon, and a lecturer in London, has ventured to criticise Edinburgh practice, from an acquaintance with it only by reading the cover of our Journal (present vol. p. 143). We regret to find that Mr Solly seems to be equally unacquainted with the Edinburgh opinions of Dr Burrowes' work, which ought at least to have merited his attention, when it is considered that the doctrine referred to was established by some of the most able men that city ever produced. We recommend to his careful perusal the article referred to, whilst we again confidently assert, that the existence of the peculiarity of the circulation within the cranium, first pointed out by Monro Secundus, tested experimentally by Dr Kellie, and ably illustrated by the late Dr Abercrombie, is in no way shaken by the attack of Dr Burrowes, or by the opinions of those who have too hastily agreed with him.

Mr Solly next proceeds to speak of the anæmic affections of the brain, among which he places delirium tremens. He very properly points out that some of these cases, however, are allied to inflammations, and require a depleting treatment. He distinguishes these from true delirium tremens by the term *delirium ebriosorum*. He says—

It is impossible to give in words, all that distinguishes these two diseases; they must be seen frequently to be appreciated. But the following will assist in their diagnosis. The head and skin generally is cool and moist in *delirium tremens*, dry and hot in *delirium ebriosorum*. The pupil varies in both according to the stage; in the early stage of both it is generally contracted, in the latter stage dilated. The conjunctiva injected and red in *delirium ebriosorum*; the reverse in *delirium tremens*. The mental derangement in the former is more allied to an exalted, excited state of intellect; in the latter it approaches fatuity and depression. The tongue is generally pale and furred in *delirium tremens*, sometimes unnaturally clean and red; in *delirium ebriosorum* is usually dry, and

sometimes brown, but this is no certain guide. The pulse is most uncertain, for as all inflammatory affections of the brain are depressing in their effects on the heart's action, so do we find that the pulse is not hard and wirey in the hyperæmic affection, which, however, never amounts to one acutely inflammatory. Still, on the whole, there is less power in the beat of the artery, and that more varied in delirium tremens than in delirium ebriosorum.—Pp. 358, 359.

With respect to the treatment of the first, he recommends opium with the accustomed stimulus. When the last is present, he considers the congestion of the brain requires to be removed by a depleting treatment. In such cases he observes:—

It is better to avoid the use of all blood-letting if possible, and never to abstract it except locally. Leeches and the cupping-glass are the best agents. A warm bath for the whole body, and cold to the head, either by means of a stream of cold water or pounded ice, prove much better narcotics than all the varied forms of opium in delirium ebriosorum.—P. 358.

Although we have occasionally seen blood-letting, used cautiously, beneficial in delirium tremens with excited action, it is a remedy very dangerous in the hands of inexperienced practitioners. Tartrate of antimony is a much safer, and even better means of arresting the progress of the cerebral disease, and we are surprised to find that it is not noticed by the author.

The account of cerebral *ramollissement* is carefully collated and brought up to the present state of our knowledge regarding it. The only novelty that has struck us is an expression of belief (p. 374), that long-continued cerebral anæmia will produce white softening. This belief does not appear to be founded on any facts, and we are at a loss to know how it can be supported.

Under the head of hyperæmic affections of the brain, we meet with the following statement:—

I believe, 1st, That inflammation of the hemispherical ganglion may be distinguished from inflammation of the rest of the encephalon. 2ndly, That inflammation of the upper portion of this ganglion may be distinguished from that inflammation which is at the base of the brain. 3rdly, That inflammation of the medullary or tubular substance, though seldom occurring alone, may be distinguished from that of the ganglia. 4thly, That inflammation of the cerebellum has its characteristics.—P. 396.

We have little doubt that careful observation, combined with a clear knowledge of the functions of different parts of the brain, is capable of arriving at such a diagnosis. We should have liked, however, the author to have stated in a condensed form the peculiar characters which would enable us to distinguish one from the other. As it is, he has left this to be inferred from a perusal of that portion of the work which treats of hyperæmic affections and inflammation of the different parts of the brain.

The author divides the appearances observed in inflammation of the hemispherical ganglion, into alterations in consistency and alterations in colour. With respect to the first, there is softening and hardening; and, as regards the latter, it varies from a pale tint to a bright scarlet. The result of his observations is, that a pale condition of the ganglion occurs in patients who had sunk into a state of

mental imbecility, and was generally associated with some serous effusion and thickening of the arachnoid and pia mater. We regret that the observations of Mr Solly at Hanwell, should have been limited to noticing the consistence and colour of the convolutions. Such a mode of investigation can never lead to important results. What the present state of science requires in cases of insanity, is an accurate structural examination of the brain in conjunction with an observation of the case, prosecuted by a trustworthy histologist. It has always seemed surprising to us, that notwithstanding our improved knowledge of the structure of the nervous system, and the magnificent opportunities afforded by our large insane establishments for investigating the true pathology of insanity, we are not aware of any series of observations undertaken for this object with a rational spirit of inquiry. It is certainly reasonable to expect, that if changes exist any where, it must be in the ultimate structure of the organ; and yet it would seem that consistence, colour, and the coarsest kind of observation is still practised, in the vain hope of elucidating the delicate alterations which produce insanity.

Notwithstanding the little light thrown by our author on the pathological states of the nervous system, we have read his observations upon the symptoms and treatment of inflammation of the brain with considerable interest. It is well illustrated by a number of original and well-selected cases. The following is a recapitulation of the treatment he recommends:—

1st. There is no time to be lost—even minutes are of value.

2ndly. That inflammation of the brain is a depressing disease, and that, as a general rule, general blood-letting is not often admissible.

3rdly. That, though general blood-letting may sometimes be attended with benefit at the time, the good derived from it is seldom permanent.

4thly. That local blood-letting, by leeches and cupping, is generally useful, and especially in cases of insomnolence, arising from abnormal action of the brain.

5thly. In cases of insanity, where opium has failed to produce sleep, leeches and cold applications frequently will; and if they do, it is strong evidence that the excitement arises from hyperæmia, and not from anæmia, as in that of delirium tremens.

6thly. That aconite and digitalis are the best sedatives, especially when combined with mercury.

7thly. When it is advisable to salivate rapidly, raise the cuticle by boiling water or a similar escharotic, and dress the surface with the strong mercurial ointment.

8thly. Always commence the treatment with a brisk mercurial purgative.

9thly. Soothe the patient's feelings in every way.

10thly. Never leave anything that is disagreeable to the patient to be done by a nurse or attendant, such as the application of leeches, &c., but *persuade* him to have them applied.

11thly. Never lose your patience in the treatment of a chronic case, or try to hasten the cure by increasing the doses.

12thly. When it is considered necessary to continue the use of mercury for a lengthened period, combine tonics with it.—Pp. 487, 488.

The rest of the volume is a short but judicious summary of the received notions of the more important nervous diseases. At page 648 is a so-called case of cancer of the brain in a child four years

and three months old, the tumour in which, with all deference to the author, we doubt being cancerous at all. It was much more probably tubercular in its nature; but as the observation is exceedingly imperfect, nothing being said as to the constitution of the child, and no remark made as to the appearances of the thoracic and abdominal organs, no certainty can exist on this or any other point regarding it.

On the whole, we consider the work of Mr Solly to be a judicious one, and well worthy of the perusal of our readers. If it do not throw much new information upon the nature and treatment of diseases of the brain, it must be attributed to the still imperfect knowledge of the changes which occur in the minute structure of the organ in its various morbid conditions. None can know better than ourselves the toil and labour, the sacrifice of time, and the extraordinary perseverance required for such investigations. But it is to these alone that we must look for the elucidation of those difficulties and contrarieties which still involve the physiological and pathological conditions of the nervous system.

Recherches sur le Cancer, avec Huit Planches. Par le Docteur SEDILLOT, Professeur a la Faculté de Médecine de Strasbourg, &c. 8vo. Pp. 120. Strasbourg: 1846.—(*Researches on Cancer, with Eight Plates.* By Dr SEDILLOT, Professor of the Faculty of Medicine at Strasburgh, &c.)

Die Diagnose der Bösartigen Geschwülste, nach Eigenen Untersuchungen. Von Dr CARL BRUCH, Privat-Dozenten and Assistenten am Physiologischen Institute in Heidelberg. Mit V. Lithographirten Tafeln. 8vo. Pp. 567. Mainz: 1847.—(*The Diagnosis of Malignant Swellings, from Original Researches.* By CHARLES BRUCH, M.D., &c.)

THE two works, the titles of which we have placed above, are in the highest degree creditable to their authors. The one emanates from the Professor of Clinical Surgery in Strasburgh, the other from a young Pathologist, already favourably known to the profession, in Heidelberg. They both proceed upon the assumption, that to know any thing of a disease it is necessary not only to study it at the bed-side, and by examinations after death, but to investigate the minute changes in the organs affected by means of the microscope. M. Sedillot has been enabled to effect the latter object with the assistance of M. Kuss, and has given thirty cases so investigated, which, in a practical point of view, are of the utmost importance. These are illustrated by eight wood-cuts, which, though not highly finished, are characteristic. We have only to regret that M. Kuss has not figured the effects of re-agents on the structures he describes.

The work of Dr Bruch is more voluminous, and is divided into two parts. The first contains twenty-seven cases of morbid growth, carefully examined as to their symptoms, anatomical characters, and minute structure, with critical observations on each. The second part is a systematic treatise on the nature and means of detecting cancer, in which all that is previously known on the subject is embodied, and compared with the results of his own researches.

Both works are of the highest value; and if we do not notice their contents more at length, it is only because we know that the opinions they contain will be brought under the notice of our readers in Dr Hughes Bennett's paper, now in the course of publication in this Journal. In the mean time, we content ourselves with a quotation from the commencement of M. Sedillot's treatise, which appears to us *apropos* to the present state of our knowledge of cancerous diseases generally:—

We ought to be astonished that in an epoch like our own, when all the powers of the human mind are directed towards the analysis of phenomena, and a demonstration of their nature, characters, and causes, that we have not yet extricated ourselves from the chaos which the history of cancerous diseases offers to our notice. Science and art appear on this point to have undergone the same errors; and whilst the academies avow their want of power to distinguish cancerous tumours, and in vain accumulate pretended differential characters between scirrhus and fibrous tissues, the investigators announce a medical treatment, and specific remedies, of which they declare themselves sole depositaries, and speculate as formerly upon the public ignorance and credulity. It is time, however, to go out of the paths without end, in which men, the most experienced and instructed, arrive only at uncertainty and doubt; and we do not hesitate to declare, that surgeons have not accorded sufficient importance to microscopic investigations.

We trust that this statement of so well known and able a Clinical Professor as M. Sedillot, will have its effect on the surgeons of this country, and stimulate them to follow his praiseworthy example.



A Practical Treatise on the Management and Diseases of Children.
By B. T. EVANSON, M.D., and H. MAUNSELL, M.D., &c.
Fifth Edition, revised and enlarged. 8vo. Pp. 484. Dublin:
1847.

THE work of Drs Evanson and Maunsell, since its first publication in 1836, has enjoyed so large a share of the public patronage as now to have reached a fifth edition. Though revised and enlarged, we nowhere find in it any improvement. Indeed, the direction the work has now taken, in our opinion tends to its depreciation. We are informed that it has met with a very extensive sale among the non-professional public; and accordingly, "to fit it for the better fulfilment of its unexpected destiny," a chapter is now prefixed for the use of non-medical readers. We do not think this a judicious step; and a perusal of the preliminary chapter must convince every

one that it will not materially enlighten the class of persons to whom it is addressed.

With respect to the diseases described, and their treatment, the authors seem to have remained in happy ignorance of the progress of pathology and therapeutics since they first wrote. With them, diptherite or muguet is still only simple inflammation of the mucous membrane, and the curative treatment of scrofula consists in giving iodine and sarsaparilla.

Under the head of *crusta lactea*, we are again told that this name has been variously denominated by *system-mongers*; and under that of *tinea capitis* we are once more informed, that "no words could convey a keener satire upon the imperfection of the medical art, than would a simple enumeration of the names and epithets invented by various authors for the disease of scald head." Statements of this kind, written by members of the profession avowedly for the non-medical public, certainly do not tend to advance the dignity of medicine. The parasitic nature and true pathology of *tinea favosa* seem to be quite unknown to the authors; and the old description of pits or cups filled with pus is repeated, although the most accurate researches have demonstrated, again and again, that there is nothing of the kind. We must therefore be pardoned for thinking, that the real satire upon the present state of medicine, consists in seeing such respectable practitioners as Drs Evanson and Maunsell, so careless in following the progress of the science and art in which they profess to give instructions.

A Practical Treatise on the Causes, Symptoms, and Treatment of Spermatorrhœa. By M. LALLEMAND. Translated and Edited by HENRY J. M'DOUGALL, M.R.C.S., &c. 8vo. Pp. 333. London: 1847.

THE disease of which M. Lallemand treats in the present work, is one of very great prevalence, although for the most part overlooked. A perusal of it when first published, has since led us to detect it in several of our patients, and cure a variety of complaints referred to derangement of the nervous and digestive system. The discovery of spermatozoa in the urine as diagnostic of its existence, would, we feel satisfied, be of very frequent occurrence were the microscope more generally employed by practical men than it is. Indeed, we are certain that the advantages attending its use at the bed-side, would be at once appreciated by any medical man, were the true nature of a perplexing case only once resolved by him, through its means, into one of spermatorrhœa. We cannot, therefore, too highly recommend this work to the attention of our readers. We are aware that many respectable practitioners regard it as of somewhat equivocal character; but in this opinion we have never

agreed. On the contrary we believe, that in publishing this work M. Lallemand conferred a great boon on the profession, and did much towards lessening those secret evils arising from charlatanism which all humane minds must deplore. In no country, perhaps, do these evils exist to a wider extent than in our own; and we think Mr M'Dougall will benefit both the profession and the public by the present translation. We consider this generally to be well accomplished. Much of the verbosity of the original is curtailed; and although we regret that certain disquisitions, which we have always regarded as very interesting and ingenious, have been entirely omitted, we think the work, as it stands, is a highly useful addition to English medical literature.

On Ringworm: its Causes, Pathology, and Treatment. By ERASMUS WILSON, F.R.S., &c. Small 8vo. pp. 102. London: 1847.

MR WILSON has at length turned his attention to the true nature of favus, and has confirmed the observations of Schönlein, Remak, Gruby, Hughes Bennett, and others. We regret to observe, however, that with the exception of Gruby, whose experiments are slightly alluded to, no notice has been taken of the labours of his predecessors in this branch of inquiry. This is the more censurable, as in his previous writings Mr Wilson has proved to us that he is perfectly aware of their researches. At present a student, or one unacquainted with the progress of science, might easily imagine from the present work, that the structures described and figured are now brought forward for the first time. In order to preserve some degree of consistency, however, the author still denies their cryptogamic nature, a circumstance which, in our opinion, can only arise from an unacquaintance with the structure of the cryptogamia generally. Mr Wilson further tells us, that he is exceedingly doubtful of all that has been recorded with regard to the contagiousness of favus, and that his own careful investigations have led him to the conclusion that it is not contagious. These investigations, so far as we can discover, consist in simply observing that a boy affected, did not communicate the disease to his brother, sister, and schoolfellows! (P. 33.) Surely the direct experiments of Remak and Hughes Bennett, both of whom succeeded in inoculating favus, might have saved the public from the dangerous error now brought forward. We are also informed, that notwithstanding his long connexion with the St Pancras Infirmary, the author has only seen six cases; and yet he does not hesitate to recommend ceratum tigllii and local stimulants to the scalp (in our opinion a very injurious practice), and to speak of the treatment generally, with a degree of confidence altogether unwarranted by his avowed experience. We regret that it is not in our power to compliment the author upon his present publication.

Lectures on the Physical Phenomena of Living Beings. By CARLO MATTEUCCI. Translated under the superintendence of JONATHAN PEREIRA, M.D., F.R.S., &c. 12mo. Pp. 485. London: 1847.

THE substance of Professor Matteucci's Lectures is now so well known to the profession in this country, from elaborate reviews in the Quarterly, and lengthy translations in the Weekly medical journals, that it is unnecessary for us to enter upon their analysis. We believe, however, that all the accounts hitherto given of these valuable discourses, will be superseded by the excellent translation now published by Dr Pereira. It is made from a copy furnished to the editor by the author, and contains a large number of additions and corrections. It also embodies his most recent investigations, and cannot therefore be regarded as a mere translation of any of the editions hitherto published. As a work of the greatest interest in every department of medicine, and one eminently qualified to induce reflection and reasoning in the reader, we cannot too highly recommend it. Every practitioner should possess a copy.

Part Third.

PERISCOPE.

PHYSIOLOGY.

ON THE PRINCIPLE OF VITAL AFFINITY, AS ILLUSTRATED BY RECENT OBSERVATIONS IN ORGANIC CHEMISTRY. BY PROFESSOR ALISON, Edinburgh.

IN continuation of his former paper (No. for August 1846), Dr Alison proceeded to review the chief facts known in regard to the formation of the *oils* and the *albuminous* compounds in organized bodies, with the view of illustrating the modification given by vitality to chemical affinities. The formation of fat or oil in vegetables appears to be effected simply by the separation of oxygen from some of the varieties of starch; but in animals, where no oxygen is evolved, and where, nevertheless, oily compounds may certainly be formed from starch (as shown by Liebig, Chevreul, and Milne Edwards), this appears to be effected by an affinity of the greater part of the carbon and hydrogen of the starch, for the small part of the oxygen, to form fat; while a small part of the carbon and hydrogen, with the greater part of the oxygen of the starch, aided by additional oxygen absorbed from the air, passes off as carbonic acid and water. In both cases, the essential characters of that affinity, which appears to be peculiar to the state of life, lies apparently in the attraction of carbon for hydrogen, with a much smaller proportion of oxygen than exists in the compounds of these elements existing in the inorganic world; and as no such compound is formed from starch, under the same circumstances in other respects, without the presence of living cells, he regards the formation of oil in living bodies as an effect of vital affinity; although admitting that, in the course of the decomposition of animal compounds by various chemical agents, oily matters may be formed by simple chemical action, as in the well-known example of the formation of adipocere from fibrin.

The fact, that the formation of fat in the animal body is so notoriously diminished or restrained by exercise, increasing the supply of oxygen, and promoting, therefore, the excretion of carbonic acid and water, he thinks very important, as indicating, along with other facts, the principle that vital affinities do not supersede the usual chemical relations of the elements that are liable to them, but are merely added to these, and allow of a division of those elements between compounds formed by vital and by simple chemical powers.

He next referred to the important question, now warmly disputed among chemists, whether albumen can be formed in the animal body, or only passes into it, directly or indirectly, from vegetables, where it is believed to be formed, by a vital action, from the elements of starch and those of ammonia (in whatever way this last may be supplied), the elements of water and a little oxygen passing off at the same time in the usual exhalations of the plant. He pointed out, that, by the addition of a full supply of oxygen, it is quite possible that the elements of starch with ammonia in the animal body may divide themselves into two portions—the one containing the greater part of the carbon falling by vital affinity into the proportions of the albumen, while the other, absorbing the oxygen, passes off as carbonic acid and water, the constant excretions of animals; and he conjectured that the elements of ammonia, requisite for this action, may be supplied in animals by the air which is continually taken into the stomach, in the water and in the saliva which are habitually swallowed, and which will there be under conditions very similar to those in which air and water are known to form ammonia. He stated likewise, that as it is certain that fat is formed in the animal body from starch, and that gelatin is formed in it probably from albumen, it is certain that the term assimilation, in the physiology of animals, cannot be restricted to the mere selection and appropriation of compounds already formed in vegetables (as Dumas supposes), but must include, also, certain processes of transformation (as maintained by Dr Prout). And admitting that the question, as to the actual formation of albumen in animals, can only be finally decided by ascertaining whether the whole quantity of azote thrown off from an animal in a given time is greater than is introduced into it in the form of albuminous ingesta, he urged various reasons for inclining to the opinion, that a certain quantity of albumen is formed, chiefly from the anylaceous ingesta, in the animal body.

He admitted, however, that a comparison of the proportion of the elements in the azotised aliments, and in the excretions, shows that the formation of albumen in the animal frame must be to a very small extent only; and stated that the general distinction of the azotised or the chief nutritious portion of the aliments, and the non-azotised or chiefly calorific portion of them, and the doctrine of these last protecting the albuminous part of the blood and the textures, from the agency of the oxygen taken into the blood, and that of the wasting of the textures, and death by anæmia in weakening diseases, being due to the action of the oxygen, appear to him to be quite consonant with clinical observations in various diseases, and to be an important addition to pathology; besides giving us more precise ideas as to the nature of the function of digestion, and the law of Prout, of the necessity of mixture of aliments for the support of animal life. This doctrine is the strongest illustration of what was formerly stated as to the use of the oxygen taken into the animal body, viz.,—not to take any direct part in the formation of compounds by strictly vital affinities, but to exert a simply chemical action on all organic compounds capable of yielding to simply chemical affinities, and so to support the excretions.

The formation of gelatin in the living animal body, he considered as certainly owing to the separation of carbon and hydrogen, by help of the oxygen of the air; but pointed out the possibility of this taking place, not merely, as Liebig states, from the action of the oxygen on the elements of albumen, but likewise from its action on the elements of starch with ammonia, provided that a certain quantity of oil or fat is formed at the same time.

He then proceeded to consider the general theory of excretion in the animal

body, beginning this subject with the striking fact, noticed by both Prout and Liebig, that although oxygen from the air must be a main agent in forming all the excretions (because oxygen exists in them all in a larger proportion than in those aliments and those textures of the body from which they must be respectively derived), yet the oxygen taken in at the lungs does not appear to enter into the combinations by which the excretions are formed (particularly, does not form carbonic acid), as long as it is passing along the arteries, but "changes its mode of action" when it reaches the capillaries, where it must meet with the matter absorbed from the textures. He stated it as the general, and apparently first, opinion of physiologists, that the excretions are furnished partly by redundant ingesta, and partly by "effete" matter in the system itself; but the important agency of oxygen in maintaining them had not been so generally recognised, and the term "effete" has in general no very definite idea attached to it. But, combining together all that is known as to the continued interstitial absorption in animal bodies, the continual introduction of oxygen into them, and as to the nature and quantity of the excretions as compared with the ingesta, he stated it as the most general expression of these facts, that throughout all the parts of any living animal where nutrition and absorption are going on, *i. e.*, at the extremities of the capillary vessels, in the more perfect animals, carbon, nitrogen, hydrogen, and oxygen, are continually forming two sets of compounds,—certain portions of these elements, recently introduced in the form of aliments, either separating, in the form of the organic compounds, from the other constituents of the blood, or uniting to form those compounds,—and in either case attaching to themselves particles of earthy and saline and inflammable bodies, taking the form of cells or fibres, and building up the organised frame; while other portions of the same, which have been for some time in the body, rejecting these adventitious matters, and uniting with oxygen from the air, are continually falling into the proportions by which the compounds destined to excretion are formed, which are poisonous to the system if retained, which tend always to the crystalline form, and, in fact, are steps in the process of the gradual restoration of these elements to the inorganic compounds, carbonic acid, water, and ammonia, out of which the agency of vegetable life had originally formed them.

This general fact he considered as the clearest proof of that principle of the modification of chemical affinities, by the contact of living structures, to which the term Vital Affinity is applied; and, at the same time, as an indication of this distinctive peculiarity of the vital affinities, that, like all other actions strictly called vital, they are of *transient duration only*; and that the life of every individual animal is maintained only by the successive life and death of all the atoms of organised matter of which it is composed; every portion, as it dies, being removed from its place by interstitial absorption, becoming liable to the influence of the oxygen (as it would be, if separated from the body, and undergoing decomposition), and serving for the maintenance of the different excretions.

This opinion he stated as having been adopted by several physiologists of late years, in regard to those portions of animal textures which are engaged in active vital operations, particularly muscular and nervous parts; but its importance as a general physiological principle, connecting together the necessity of continued nutrition for the maintenance of animal life (even during the decline of the body), the intention of interstitial absorption, the use of respiration, and the necessity and nature of the excretions, had not, as he thought, been pointed out in any physiological work. He acquiesced, however, in the statement of some recent authors, that this partial death of portions of the animal frame appears to be greatly accelerated by local increase of any of the strictly vital actions; and under this law, he thought we may include not only the fact of the loss of power in a muscular or nervous part from over-exertion (leading to increased interstitial absorption, and thereby in a healthy constitution, and where the exertion is not excessive, to subsequent deposition and hypertrophy), but likewise the increased interstitial absorption, the ulcerative

absorption, the partial sloughing, or general gangrene of a part that has been inflamed,—all these being results of the death of certain particles of matter concerned in the inflammation, and varying in degree as this death is more partial and gradual, or more general and sudden.

On the whole, therefore, he maintained, that not only the general principle may be held to be ascertained of peculiar vital affinities actuating the portions of matter by which the organic principles are formed, and organised structures developed ; but that the facts already known on the subject, justify several important inferences in regard to the peculiarities of these affinities, both as to their nature and duration ; and that we can point out, with at least much probability, the share which these affinities take in all the changes to which the elements composing organised bodies are subjected, from the time of the first separation of carbon from oxygen, which is essential to vegetable life, till the restoration of all the elements thus employed, by the aid of oxygen taken into the body, to the condition of inorganic compounds, in the excretions that are continually thrown off from living animals.—*Proceedings of the Royal Society of Edinburgh*, 1847.

PATHOLOGY AND PRACTICE OF PHYSIC.

ON SUB-ACUTE INFLAMMATION OF THE KIDNEY. By Mr JOHN SIMON ; and, ON THE INFLAMMATORY DISEASES OF THE KIDNEY. By Dr JOHNSON.

[The two papers above referred to are so closely connected in subject, as well as by their being in part the result of conjoint observation, as to make it convenient to analyse them under a common head. That of Dr Johnson is supplementary to his paper on fatty degeneration of the kidney, referred to in the *Periscope* of this Journal for January 1847].

Both authors set out with the description of a condition of the kidney, to which Dr Johnson gives the name of “acute desquamative nephritis,” and which consists essentially in the increased development of the epithelium lining the tubuli uriniferi, which is also thrown off almost as soon as formed into the interior of the tubules, and thence, in part, washed out with the current of urine. This morbid process, Dr Johnson considers to be a modification of inflammation very similar to that state of the skin after scarlatina, and other acute diseases, which results in desquamation of the cuticle. He believes it to be a frequent cause of albuminous urine, and dropsy ; and, in particular, states that the dropsy after scarlatina is dependent on this condition of the kidney. It occurs, however, in many forms of disease as a secondary complication ; and is very apt to recur frequently and insidiously, presenting, according to Mr Simon, few obvious symptoms, and giving rise to changes in the kidney, which, however important, are difficult of recognition, and have been, in the dead body, frequently overlooked.

The condition of the urine in this disease is characteristic. It contains a sediment, which, on microscopic examination, proves to be composed, in part, of nucleated cells similar to those which, in the healthy state, line the tubules of the kidney. These cells are frequently entangled in the cylindrical casts of the urinary tubules, which have been described by Franz Simon, Vogel, Scherer, and others ; they are accompanied generally by blood globules, and not unfrequently by crystalline deposits of lithate of ammonia, or oxalate of lime. The urine which contains them is generally, though not invariably, albuminous.

The acute disease here spoken of, and in the description of which both the authors concur, may disappear without leaving any traces ; or it may verge into a sub-acute or chronic form, in which the cells and accumulated products

of secretion, instead of being removed by the urine, remain in the tubes, and by becoming impacted lead directly to the obstruction of secretion, and the disorganization of the gland. The mode in which these ultimate changes are effected, is differently explained by the authors.

According to Mr Simon, the consequence of this obstruction is rupture and collapse of the tubes, with extravasation of the secreting epithelial cells. These cells, thus deprived of the natural outlet for their contents, assume to themselves a new or *compensating* function; Mr Simon holding them to be closely allied to the elementary cytoblasts, from which the cysts, so often found in the kidneys, are derived. Whether the extravasated epithelium itself *becomes* the cystic cell, or only becomes the matrix of cells capable of this form of development, his language does not indicate with precision; but it is clear that he considers the cystic vesicles to originate in cells almost exactly similar to the secreting cells of the healthy gland.

Once formed, these cysts appear to undergo development, and to usurp the space of the collapsed tubes, which in time disappear, leaving portions of the organ entirely metamorphosed into cystic structures. The progress of this degeneration is twofold: some cysts may become developed (this chiefly happens near the surface) till they reach the enormous size well known in cystic diseases; while others (the majority) become ultimately atrophied and absorbed, leading to great contraction of the kidney, as is commonly seen in the last stages of what is called "Bright's Disease." The development of accidental fibrous tissue appears to Simon to play a very small, if any part in the contraction of the kidney, its apparent excess being altogether due to the removal of the other tissues. He does not, however, deny the formation of new fibre.

The contracted and atrophied kidney, described by Dr Bright, is therefore considered by Simon as invariably the result of cystic disease; while the mottled and perhaps enlarged kidney is connected with the fatty degeneration. The latter he considers to be a serofulous degeneration, and peculiarly the kidney of phthisis; the former is never except by coincidence found in that disorder. The contracted and cystic forms of disease belong to rheumatic, gouty, and intemperate habits.

The only remaining fact of importance in reference to the development of these cysts is, that "they frequently exhibit an endogenous formation of cells, which line them as epithelium;" but this condition does not remain if they grow very large.

Dr Johnson, agreeing with Mr Simon as to the ultimate contraction of the kidney, and the formation, in some cases at least, of cysts, gives a different explanation of the relation of these conditions to the primary diseases. He holds that, even when the tubes have been long affected with the "desquamative nephritis," and have become obstructed as above described, the Malpighian bodies remain unaffected. Adhering to Bowman's view, which represents these bodies as destined to the secretion of the watery part, while the tubular epithelium separates the solid constituents of the urine; he argues, that a large quantity of watery secretion is poured *into the tubes from the healthy Malpighian bodies*, which distends them behind the obstructed portion, and causes them to become permanently though irregularly dilated. Dr Johnson has seen the tubes thus dilated to two or three times the natural size, the walls denuded of epithelium, and thickened. The dilated tubes assume an irregularly varicose form, and Dr Johnson presumes on theoretical grounds (for he does not adduce direct observation in support of this view), that these globular dilatations, becoming isolated from the rest of the structure, assume the character and properties of serous cysts, and acquire the great power of increase known to exist in these textures.

Another result of the desquamative disease of the kidney, is a gradual contraction of the tubes, and a diminished supply of blood, entailing an atrophied and contracted condition of the entire organ. The apparent develop-

ment of fibrous tissue, when a kidney thus affected is examined under the microscope, is in reality nothing more than the change in the relative proportion of this to the tubular and secretory portion of the gland. In this advanced stage of disease, the Malpighian bodies, although hitherto unaffected, become also atrophied, and at last absorbed in consequence of the defective vascular supply. In this condition of the gland there is often a comparatively small quantity of albumen in the urine; the solid constituents also, not being excreted, are retained in the blood, and soon tend to the fatal termination. Dr Johnson, however, thinks that so long as the Malpighian bodies are capable of affording secretion, there is an abundant supply of the watery elements of the urine, as these organs are stimulated into activity by the presence of foreign matter in the tubules. In this way he accounts for the excessive quantity of the urine in the earlier stages of chronic diseases of the kidney, and its abundant quantity and low density even after the disease has made considerable progress.

Finally, it is important to observe, that the coincidence of the fatty and desquamative conditions of the kidney has been repeatedly seen by Dr Johnson.—*Medico-Chirurgical Transactions*. Vol. 30, 1847.

We consider that the explanation of Dr Johnson, with respect to the formation of cysts in the kidney, which has been the long received view held by pathologists, is the correct one. Mr Simon's notions with respect to scrofulous kidney seem rather confused, as we consider that term ought only to be applied when tubercle is present. Fatty degeneration of an organ, although it may coincide with scrofulous or tubercular deposits elsewhere, is not in itself scrofulous. It may occur, for instance, without that complication, as in drunkards, or conjoined with cancer. We do not believe, however, that the contracted kidney is more frequent in drunkards than the fatty one, which is so common in connexion with the same degeneration of the liver.

MORBID SECRETION OF MILK. BY DR BRUCH, Heidelberg.

AFTER extirpation of a carcinoma of the breast, Chassaignae found, in the removed tumour, a distinctly purulent and milky fluid, which he held to be milk. As our author examined the carcinomatous breast of a woman, aged fifty-two, he also found milk, which, as a bluish fluid, dribbled constantly over the cut surface; it was present in all the yet healthy portions of the gland, and could be squeezed from the nipple. Milk globules and colostrum corpuscles, exactly as in normal milk, were found in this fluid. Hence it would appear that milk may be secreted as a consequence of turgescence of the gland from any cause. This deserves attention in forensic cases.—*Zeitschrift für Rationelle Medizin*, V. 3. 1846.

ON THE DISCOVERY OF UREA IN PERSPIRATION. BY PROFESSOR LANDERER, Athens.

THE quantity and condition of the secreted urine depends, as already known, on the quantity and quality of the fluid ingesta, and the greater or less transpiration. The urine consequently becomes more concentrated during summer, in warmer countries, than in winter, and this may be discovered by the increase of the specific gravity. The increased quantity of salt in the urine, dependent on this concentration, is probably the cause of the numerous calculous affections in warm countries; while the vegetable nourishment, and the use of fruit, especially grapes, is probably the cause of the rarity of sand and gravel. Almost all strangers in southern countries become affected with a peculiar exanthema, from the irritating effect produced on the mucous and sebaceous follicles, by a portion of the solid contents of the urine finding their way to the skin. Our author, to convince himself of the presence of these salts, collected the washings of flannels and shirts,—a fluid possessing a striking odour of perspiration, of

a very salt taste, and slightly acid properties. This fluid evaporated to a yellow syrup, deposited, after exposure to cold for some days, a granular mass of phosphates; the remainder, digested in alcohol, formed a yellow solution, which, on spontaneous evaporation, left a substance possessing a strong odour of perspiration, and a sweet taste. This, dissolved in water, and decomposed by diluted oxalic acid, deposited, after thirty-six hours, a precipitate of small crystals of oxalate of urea. To make the presence of urea more evident, these crystals were dissolved in water, and the solution decomposed by carbonate of lime, to separate the oxalic acid from the urea. The supernatant fluid was then evaporated to dryness, and the residue dissolved in boiling alcohol. This was again evaporated, and a few drops of nitric acid added; from this, after cooling in a refrigeratory, crystals of a silky lustre were deposited, which, separated from the mother water, and dissolved in distilled water, possessed a cooling, slightly acid taste, and crackled in the fire. From all these operations, our author believes that urea was present in the perspiration, and that muriate of soda, traces of sulphates, acetates, and lactates, osmazone, and a clear substance, easily soluble in ether, were also present.—*Heller's Archiv für Chemie und Mikroskopie*, IV., Jahrgang 1847. Heft 3.

NATURE OF THE FÆCAL EVACUATIONS IN CHOLERA. By M. ANDRAL.

THE rice water evacuations of cholera consist of a muddy liquid, capable of being rendered transparent by filtration, and within which are suspended, in greater or less abundance, lumps of opaque white matter, perfectly untinged by bile. The liquid portion of this material has been regarded as the serum of the blood; the solid grumous portion as the fibrine. After having filtered this intestinal matter, and thus separated it into two portions—the one liquid, perfectly transparent and colourless, the other solid, consisting of a number of particles agglomerated together—M. Andral treated the liquid portion with alcohol, nitric acid, and heat; but by none of these re-agents was a precipitate formed, consequently there was no albumen. The results of an examination of this liquid portion by M. Favre, also showed that the organic substance contained in it possessed none of the characters of albumen. Hence the serum of the blood does not enter into the materials ejected from the intestines in cholera. On examining the blood in all the stages of cholera, he found that its proportion of albumen is almost unaltered. Neither does fibrine exist in the intestinal secretions in this disease; no trace of filaments could be detected by the microscope. He found the solid masses to be composed of numerous nucleated cells, closely resembling pus globules, and presenting, like them, from one to four granules on the addition of acetic acid. Besides these globules, which were very abundant, the masses contained also numerous particles of epithelium. It would appear, therefore, that the white matter discharged from the intestines in cholera, consists essentially of a mucous secretion in enormous quantity, and so far altered in quality as to contain an abundance of corpuscles in all respects similar to pus globules, and to which the white appearance of the secretion is due.—*Comptes Rendus*, 9 Aout 1847.

We doubt very much the propriety of attaching the idea of fibrine only to that condition of the liquor sanguinis when it coagulates into filaments. This, in the adult, takes place for the most part only on serous, and is exceedingly rare on mucous, membranes. In the latter situation, it is rapidly transformed into pus, and, should the observation of M. Andral be confirmed, we must accept it as an evidence of intense inflammation of the intestines in these cases.

CASE OF HYDROPHOBIA, IN WHICH INHALATION OF ETHER WAS TRIED. By ROBERT ALLAN, Esq., Surgeon, Mauritius.

RAMJAN, an Indian boy, æt. twelve, was bitten on the right ankle by a rabid

dog in May 1846; there were four incisions made by the dog's teeth, and the part was bare when injured. The sesqui-chloride of antimony was applied to the wounds in a few hours after by some druggist, and the dog was shot.

On the 8th of June 1847, he complained of pain in the throat, with difficulty of swallowing liquids, and on the 9th unequivocal symptoms of hydrophobia were manifested. Dr Solesse was called in, who, with Mr Allan and several other practitioners, saw the case. It was unanimously agreed to try inhalation of ether, which was refused by the patient. It was therefore applied to his mouth by means of a sponge in a bottle, with a hole in the bottom, whilst his struggles were restrained by force. He was rendered insensible for six minutes, after which he called for water, with which he rinsed his mouth and washed his hands, without spasm being produced. The inhalation was employed five times during the day of the 10th, always with the effect of temporarily suspending or alleviating the symptoms. Latterly, the bottle having been broken, a sponge was substituted for it, and for this he repeatedly called after ten o'clock at night, and said it relieved him. He expired at half-past one on the morning of the 11th.

The *post mortem* examination presented nothing unusual. In the present instance, sixteen months had elapsed after the bite; and fifty hours after the commencement of the disease, before inhalation was tried. Both the boy and parents, being superstitious and ignorant, did all in their power to thwart every attempt at alleviation. Yet all who witnessed the case (twelve in number) were of opinion that the ether caused great relief.—*Lancet*, October 16, 1847.

Considering the imperfect manner in which the ether was administered in this case, its inhalation seems very encouraging. Those present did not seem to be aware of the fact, that individuals can be kept under its influence for several hours without injury. Had such been done, there might have been a different result. There can be now no doubt, that most of the evil effects attributed to ether arise from the improper mode of its administration, not maintaining quietude in the room, and unnecessarily intermitting its employment. The new preparation of Professor Simpson, announced in the present Number, will remove many of the inconveniences connected with ether; while it opens up a new era in the history of substances capable of safely producing insensibility to pain.

ON THE CONSTITUTIONAL SYPHILIS OF INFANTS. By MM. TROUSSEAU and LASEGUE of Paris.

THE authors consider it established that syphilis may be transmitted either in its primary or secondary form directly from the mother to the infant; but they hold that there is no proof of the transmission of tertiary lesions except as a consequence of the primary or secondary forms.

They are disposed to deny the appearance of syphilis in the infant at birth, or at any period before the second week. They suppose the alleged cases of its earlier appearance to have been founded on misconceptions, either of accidental ulcerations or mucous discharges, which prove nothing with regard to the constitutional affection, or of a general cachectic aspect, which has been described with great confidence, but which MM. Trousseau and Lasègue consider as being too vague in its characters, and too uncertain in its occurrence, to form the basis of a diagnosis.

One of the earliest and most characteristic signs of the appearance of syphilis in the infant is a coryza, which begins at first with mucous secretion, followed by serous and purulent discharges, and by hemorrhage of greater or less frequency, and terminating in caries and deformity of the nasal bones. This affection they believe to be peculiar to syphilitic infants.

Almost equally characteristic is a particular discoloration of the skin, which becomes tarnished and loses its transparency, without, however, any preter-

natural turgescence or shrinking. The colour is unequally diffused over the face and trunk; the greater its diffusion, the less, generally speaking, is its intensity. This tarnished hue of the skin rarely lasts more than a week.

Next in importance and succession are the eruptions. On this point the authors remark, that it is impossible to found a valid diagnosis upon an eruption taken apart from all other symptoms; but that the concurrence of an eruption with other and less variable signs, is sufficient to place the conclusion upon a firmer basis. The different characters supposed to indicate a syphilitic eruption are then discussed, and it is shown that neither the copper-coloured stains, nor the dark hue of the crusts, nor the circular disposition of the eruption, can at all be relied on in the diagnosis of infantile syphilis.

The above are the earliest and most characteristic symptoms; in the subsequent progress of the disease the infant becomes cachectic; accidental wounds assume an unhealthy aspect, and heal with difficulty; the umbilical cicatrix is apt to remain open, and to fungate. Sometimes, though by no means constantly, there is gradual emaciation; the violence of the internal disorder bearing no proportion to the intensity of the external signs. The infant does not take the breast readily; sleep is short and interrupted; it cries frequently, and without appreciable motive; and, concurrently with these symptoms, diarrhoea is established, which it is exceedingly difficult to subdue. The mouth and anus, where the mucous membrane joins the skin, are cracked and fissured, and the discharges by stool are often bloody. Under these circumstances the infant, impoverished and weakened in constitution, falls a victim either to the chronic disorder, to the reigning epidemic, or some accidental acute disease. The fatal sinking is in general extremely rapid, and not proceeded by the usual warning circumstances, and the inspection of the body does not explain the rapidity of the fatal termination. The most constant lesion is serous effusion into all the cavities.—*Archives Générales de Médecine*, October 1847.

TREATMENT OF PERITONITIS FROM INTESTINAL PERFORATION BY REPEATED PARACENTESIS. BY PROFESSOR PIORRY.

A MAN, aged twenty, of a vigorous constitution, was admitted into hospital during the month of March 1835, labouring under well-marked symptoms of typhoid fever. On April 14, the signs of an intestinal perforation showed themselves; a quantity of fluid became effused in the abdomen, gas being at the same time generated in the peritoneal cavity. These circumstances induced M. Piorry, notwithstanding the concomitant acute disorder, to perform paracentesis: three pints of fetid serum were removed; and the liver and heart, pressed upwards in the chest and interfering with respiration before the operation, now descended into their place, thereby causing immediate relief. The peritoneal secretions having rapidly increased, the operation was repeated four times during the ensuing six days, but, after a temporary amelioration, more severe symptoms set in, and the patient died on the 14th of May, one month after the occurrence of the intestinal laceration. On dissection, the anatomical changes due to peritonitis were noticed, *i. e.*, purulent fluids and false membranes; the odour of the effusion was stercoraceous; the intestines were united in the cœcal region by adhesions; and a perforation was found in the neighbourhood of the cœcum, in the centre of an inflamed Peyer's gland. On the liver and in the lungs tubercles were discovered.—*Revue Médico-Chirurgicale*, October 1847, and *Medical Times*, November 6, 1847.

The reporter of this case (Dr M'Carthy) does not agree with M. Piorry in thinking that the exhibition of opium is useless in these cases. He points out, however, "that the success obtained in the treatment of acute hydrophthalmia by puncture of the eye, in pleurisy by paracentesis thoracis, &c., plainly shows that the dangers attending punctures of the large serous membranes are

not in any respect so great as it was generally supposed some years since ; and in the case recorded by Professor Piorry, we find that not only the five-times-repeated paracentesis was not productive of any unpleasant consequences, but that in all probability they delayed the fatal termination of the disease considerably beyond the period at which it might naturally have been expected." In these remarks we fully concur.

ON CHRONIC AMYGDALITIS, AND THE TREATMENT OF INDURATED TONSILS. By DR J. NAUDIN.

THE tonsils, by their situation, are often exposed to attacks of inflammation, which, after repeated occurrence, not unfrequently passes into a chronic state of induration. The disease is generally non-malignant, and affects both tonsils; carcinomatous induration being, on the contrary, much more rare, and affecting usually but one. The seat of this hypertrophy is neither the mucous membrane nor the cellular tissue, though their nutrition may also be altered ; but in the glandular substance itself. The cause of the frequent occurrence of hypertrophy of glandular organs is, that possessing a supply of arterial blood, infinitely greater than is necessary for their nutrition, a large portion of which is destined to supply the material for secretion, any circumstance which produces a suppression of this secretion causes the excess of arterial blood to become expended on the nutrition of the glandular substance, thereby inducing its hypertrophy and induration. Physicians are generally very neglectful of chronic inflammation of the tonsils, too often allowing the case to run on, and finally putting it into the hands of the surgeon for excision. The means, if any, employed with the view of reducing the tumours, are generally insufficient ; and our author, instead of blisters, astringent gargles, iodine, &c., substitutes gentle cauterization, as employed in chronic inflammation of other organs. Instead of producing a slow progressive destruction of the tonsils, he aims at their preservation, and for this purpose employs a solution of nitrate of silver, 3 gr. to ʒj of water, increasing the strength by 3 gr. up to ʒij of the nitrate, in the same quantity of water, and also applying the solid caustic to the surface of those hollows which usually exist in such tonsils, so that all parts may be equally affected. During one sitting the tonsils are painted twice or thrice ; the mouth is then well washed with water. This cauterization must be repeated every two or three weeks, until the tonsils are restored to their normal size, and then gradually discontinued ; it produces no ill consequences, and even children speedily return to their play. Should the parts become accustomed to the caustic, it must either be discontinued for a time, or another substituted, as Lugol's diluted solution of iodine. In two cases related by our author, the nitrate alone was employed. Both, aged thirteen and fourteen, had been affected for years, and were cured in two and a half, to three months ; in a third case, that of a girl aged eleven, the disease was extensive and obstinate, requiring four months' use of the caustic, besides the use of hyd. pot. and iodine internally, and as ointment. In all these cases no return has been observed after the lapse of years, and the previous disposition to inflammation of the tonsils has been extinguished.—*Journ. de Toulouse*, Juin et Juillet, 1846.

* TREATMENT OF CHRONIC ARTHRITIS BY NITRATE OF SILVER OINTMENT.

At the Hotel Dieu and La Charité hospitals in Paris, M. Guerard and Briquet employ in chronic arthritis following acute rheumatism, or any other cause, an ointment containing nitrate of silver. At the Hotel Dieu, five parts of the nitrate are rubbed down with thirty-two parts of lard. The articulation is well rubbed with it once a-day, and a poultice is then applied over it, to favour absorption. This treatment is continued until the disappearance of pain. The epidermis becomes shining, yellowish, or almost bronzed, and sometimes blackened, for some days. At La Charité the ointment is not so strong, and is com-

posed of only one part of the salt to thirty-two of lard ; but the good results are not so well marked. A similar treatment is at present followed by M. Jobert at St Louis, in cases of white swelling.—*Annales de Thérapeutique*, Oct. 1847.

TREATMENT OF CHOREA BY LARGE DOSES OF TARTRATE OF ANTIMONY.

In a case of chorea of five years and a half standing, and which had resisted valerian, oxide of zinc, purgatives, cold baths, and narcotic frictions on the spine, M. Salgues of Dijon had recourse to tartar emetic. Each day, for a week, the child took thirty centigrammes in a drink. The first dose produced strong vomiting and an abundant diarrhoea. The others caused no apparent effect, with the exception of anorexia, and the cessation of the chorea. The fourth day, ten leeches were applied to the neck to diminish a slight cerebral congestion. A permanent cure followed.—*Rév. Méd. de Dijon, and Annales de Thérapeutique*, October 1847.

TREATMENT OF SCABIES BY OLIVE OIL.

M. GRIFFI of Sardinia has treated scabies with great success by the simple application of olive and other fixed oils. The method consists in covering the affected parts with the oil, slightly heated to favour its absorption, three times a-day. The itching completely subsides in three days, the eruption dries up and desquamates, the redness and swelling disappear, and the patient is generally cured in twelve days. The application should be continued a few days, to ensure success. The almond, linseed, nut, and other oils, and even simple lard, will produce the same effect.—*Annales de Thérapeutique*, October 1847.

SURGERY.

A NEW AND CERTAIN METHOD OF CURING FALSE JOINTS, OR PSEUDO-ARTHROSIS. By PROFESSOR DIEFFENBACH, Berlin.

THE most simple method of treating false joints, is by rubbing the ends of the bones strongly together. This deserves the first trial, but is chiefly successful in children and recent cases. The additional use of irritation, or blisters, to the skin, is productive of no benefit. The use of setons drawn through the false joint, has been much recommended ; it is apt, however, to produce violent inflammation and extensive suppuration, and, in the most favourable cases seen by our author, it only strengthened the intervening ligamentous substance, producing in no instance a true firm callus,—the improvement always proving temporary. Resection of the ends of the bones has also been performed in various ways by different surgeons. In three cases so treated by our author, the deformity was only rendered worse, and the extremity became more and more like a flail. Many other remedies have been used, as cauterization of the ends of the bones, &c., several of these being more dangerous than their predecessors, and none more certain of cure.

Instructed by the experimental researches of Flourens, Duhamel, Troja, &c., that, if a bone be bored through transversely, and a wooden pin introduced, or if this be forced longitudinally into the medullary canal, the bone inflames, swells up, and becomes covered with a copious effusion of new bone, our author was led to apply this practically in the case of pseudo-arthritis. He first, however, attempted to gain his end by the simpler method of merely boring through the bone without the introduction of pins. This was successful in the first case.

A healthy girl of nine years old, with a false joint in the right leg, accom-

panied by considerable contraction of the flex. pollicis long., tibialis posticus, and tendo achilles, so that the limb was, at the broken part, bent at an acute angle ; these contractions were first removed by subcutaneous incision, and, a few weeks later, the ends of the bone perforated eight times by a small borer. The limb was, at the end of three months, perfectly firm.

In the second case, that of a girl aged twelve, with a false joint, likewise of the leg, a similar method was pursued ; but, at the end of six months, no callus was produced, and, although the limb was at first tolerably firm, it speedily became again bent. The author, consequently, resolved to repeat the operation, adding the introduction of wooden pins, as the mere boring had not proved sufficiently irritating. He had first, however, an opportunity of employing this method in two other cases, and from its safety, efficacy, and the ease with which it is performed, he is induced to recommend it to the profession.

In performing this operation, the limb must be strongly extended, to bring the ends of the bones in opposition, and there they must be retained. When the false joint is of long standing, and the parts in a manner fixed in a distorted position, a previous treatment is necessary to bring them straight. All rigid and contracted tendons and ligaments must be cut through subcutaneously, and the ends of the bones brought correctly in apposition by careful extension and bandaging. The limb being fixed, and the skin and soft parts made tense at that point where the bones lie nearest the surface, a long narrow, but broad-pointed knife, is then passed down to the bones, about half an inch from the broken ends. Through the opening thus made, a gimlet of the thickness of a quill is passed down, and the bone carefully and slowly perforated, occasionally withdrawing the instrument lest the bone should be split, which is apt to be the case when the perforation is made so near the end of the bone ; yet the irritation would not prove sufficient if placed at a greater distance. Two ivory pins, slightly thinner than the gimlet, are then to be well-oiled and forced through the bone till their ends can be distinctly felt on pressing the opposite side. They should be of such a length that an inch should project above the soft parts. These are to be protected by a handful of charpie, and a bandage and splint then applied, to retain the limb in position. One hole should be bored and the pin introduced before the other is commenced.

In pseudo-arthritis of the patella, the gimlet should be only half the thickness above mentioned, the holes ought not to be bored quite through the bone, and the pins must be drawn together by a twisted suture. After the operation, the limb swells and inflames ; the bandage should then be removed, and suppuration induced by poultices. About the fifth or sixth day, the bones and periosteum begin to swell, and may be felt through the soft parts as round ball-like tumours. Should the violent obtuse pain of inflamed bone occur, the pins must be removed for a few days, the poultices assiduously applied, and then the pins again introduced ; it is seldom necessary to retain them longer than fourteen days, but this period may be extended if the bones show little reaction, and if their swelling be but trifling.

So long as the suppuration continues, the pus must be allowed free vent ; when this, however, is lessened, the bandage may be allowed to remain for a few days, and the cure finally completed by the application of a light splint and bandage. During the whole time, the proper position of the limb must be carefully preserved.—*Casper's Wochenschrift*, Nov. 1846.

ON SUBCUTANEOUS INCISION OF CYSTIC TUMOURS. By Dr A. C. NEWMANN, Graudenz.

THE author believes this method of operating to be unjustly neglected ; the trifling pain to the patient, speedy recovery, absence of cicatrix, its practicability on the scalp, &c., without removing the hair, and also in the immediate

neighbourhood of vital organs, large blood-vessels, or nerves, form its advantages over extirpation. He operates by introducing a long narrow tenotomy knife with a sickle-shaped point under the skin, about half an inch from the cyst, passing it through the subcutaneous cellular tissue to the walls of the tumour, which are then penetrated, and its lining membrane incised in as many places as possible. As much as possible of the contents is now removed by strong pressure; sometimes the whole is evacuated, at others only a portion. The cyst sometimes inflamed, and pus escaped by the wound, causing the cure to be more tedious; in general, a hard portion only continued to be felt for a longer or shorter time under the skin. A simple linen compress retained by stripes of plaster was all the dressing employed. The result has always been favourable. In no case has any return of the tumour been observed, which the author believes to be the result of the careful incision of the lining membrane.—*Casper's Wochenschrift*, Feb. 1847.

ON EXAMINATION OF THE EAR. By MR WILDE OF DUBLIN.

THERE are two methods of examining the ear, which, from their frequency in this country, are not only useless, but in some cases highly injurious. These are, the common resort of syringing, and of probing the ears indiscriminately, without proper inspection of the parts. The former is of daily occurrence. A patient labouring under deafness, or, what perhaps is worse, violent pain in the ear, is examined with the unassisted eye, or by means of some of the old divaricating specula, most probably in a badly-lighted apartment,—at all events, without the membrana tympani being brought into view, a dark cavity being all that the explorer has been able to perceive,—it is deemed advisable to try what might come out by squirting hot water into this dark passage for the ensuing quarter of an hour; but, nothing satisfactory following this operation, the diagnosis that there is no wax in the ear is accordingly made. Now, there may be a collection of cerumen, which may not be got rid of by this operation; while, if the cause of the pain or deafness be owing to an inflammatory condition of the auditory canal and its membranous extremity, a decided increase of the symptoms is produced by this unnecessary and cruel proceeding. Inflammation may be produced by unnecessarily syringing an ear where no wax was present.

The practice of exploring an ear by means of a probe, cannot too strongly be condemned; and yet, that it is frequently resorted to, surgeons are well aware. To introduce a probe down to the membrana tympani, without having that membrane fairly within view, and without a speculum being passed down to it, but merely for the purpose of satisfying the examiner as to whether the membrane is perforate or not, is a most unjustifiable proceeding.—*Dublin Quarterly Journal*, November 1847.

THE LOCAL TREATMENT OF CORNEITIS. By DR JACOB, Dublin.

LOCAL applications, except a sedative stupe or tepid lotion, with the view of allaying pain if present, are not called for, and in the early and acute stage, such stimulating applications as solution of nitrate of silver or the *vinum opii*, sometimes resorted to, should of course be carefully avoided. At a subsequent period, when the increased vascularity and sensibility of the conjunctiva, induced by the inflammatory action, exist, during what is generally considered the chronic stage, remedies of this description may be used with advantage; but as the principle mischief is so much beneath the surface, and so liable to be aggravated by any irritation, great caution must be exercised in their application. For the removal of the diffused opacity of the cornea proper, which I have described, stimulants may be employed, but not until every trace of inflammatory action has long disappeared. From the variety of stimulants used from time

immemorial to remove opacities of the cornea, and the number of them extolled as infallible, it may be presumed that any stimulant will answer the purpose. Solutions of nitrate of silver, sulphate of copper, sulphate of zinc, or the combination called *lapis divinus*, will perhaps answer. I use a solution of iodide of potassium, ten grains to the ounce of water; or, as a substitute for animal bile, said to be effectual, touch the surface with the camel-hair pencil, previously dipped in water and brushed two or three times on soap. The fumes of prussic acid, so much vaunted as a quack remedy, I have not used, being dangerous and troublesome. If this nostrum has any influence at all, it is as any other stimulant. It should be recollected that this diffused opacity, not being a cicatrix from ulceration, disappears in time, and I have no doubt that its disappearance is often delayed by the premature and unnecessary use of stimulants. It is not to be forgotten, that as the inflammation frequently, if not generally or always, extends to the membrane lining the chamber of the aqueous humour, the pupil should therefore be occasionally dilated by belladonna to prevent adhesions. A sedative lotion containing a drachm of good extract of belladonna in eight ounces of fluid, applied with a scrap of old linen occasionally during the day, will answer the purpose, and, if it does not, the outside of the eyelids and the brow must be painted with the softened extract.

It is scarcely necessary to add, that for the removal of this disease, the apparent cause of it, or at least the cause of the predisposition to it, should be removed. Generous diet, pure air, and comfortable clothing, will often do more than any medical treatment. I have more than once seen it disappear in an ill-fed, badly clothed child, after removal from a close room in the city, or a damp cottage in the country, to a comfortable bed in the hospital, without the administration of any medicine.—*Dublin Medical Press, November 3, 1847.*

ON THE TREATMENT OF PENDULOUS TUMOURS. By DR O'FERRALL of Dublin.

IN its simplest form, the pendulous tumour seems to require merely the division of its pedicle by a knife-edged scissors or scalpel; but the point at which this division is to be effected requires consideration. If the section be made too near the bulb, an unsightly projection will remain after the operation; if it be done too near the other extremity of the pedicle, the integument, on retracting, will leave a wound, and consequently a scar, much larger than could have been anticipated. Allowance, then, must be made for the elongation of the pedicle by the weight of the bulb, and for the contraction of the stalk, which always follows its division,—the same process which renders it unnecessary to tie the neck of an uterine polypus close to the mucous surface from which it has grown. The best mode of proceeding is, to poise the tumour on the hand, and allow the surrounding skin to retract and recover its pristine position, and then to make the section of the pedicle a little below its origin. Should the nutritious artery be large enough to deserve attention, the jet of blood may be prevented by previously including the neck of the tumour in a provisional ligature, and, when the section is accomplished, tying the divided artery. The provisional ligature may then be removed altogether. A slight touch of the nitrate of silver, just sufficient to produce a delicate white coating, will not only shorten the duration of the subsequent smarting, but lessen the probability of any reaction, especially of an unhealthy kind. It has appeared to the author, that whatever seals up the cellular tissue, or the orifices of divided vessels of every denomination, diminishes the tendency to diffuse or troublesome inflammation. Simple water dressing will then complete the local treatment.

In operating on the adipose pendulous tumour, the extent of interference with the pedicle will be regulated by the presence or absence of fatty matter in its substance. If the growth extend through the neck into the subcutaneous cellular membrane beyond it, such an incision must be made as will allow of its complete extraction. In such a case, the small cavity then left

should be filled with lint dipped in olive oil, and the integument brought gently over it, to prevent an unnecessarily large cicatrix. The lint is withdrawn when suppuration is established, and the integuments brought together by adhesive plaster.

The proceeding in the case of pendulous nævus is somewhat different, and must be adapted to the peculiar circumstances of the case. It is not usual for the pedicle, in such instances, to be entirely free from all traces of erectile tissue. Should the pedicle be implicated, or should the vessels of the cellular or dermoid tissues beyond it be hypertrophied, a simple section would be inadequate to the cure; hemorrhage of a troublesome nature would be the immediate result, and reproduction of the disease the more remote consequence of such an imperfect procedure. The diseased part must be included in an elliptical incision, and thus freely and completely removed.

It may happen that the erectile formation may extend irregularly, for a considerable distance, beyond the origin of the pedicle. In such cases, the amputation of the pedicle alone would entail the consequences already alluded to; while the excision of the whole of the morbid structure might be forbidden by its extent, or by the importance of the parts in which it is found. The following is the mode recommended under such circumstances, and when the removal of the pendulous tumour is desired on account of the inconvenience it occasions. The tumour being held horizontally and on the stretch, the point of the style or nail cautery, described by Dr Wilmot, should be passed through the cervix in several places, so as to ensure the obliteration of the vessels contained in that place. The whole cervix may be traversed by these punctures at one or several successive operations, according to its breadth. When the vascular character of the cervix is thus changed, its section may be performed without risk of hemorrhage. A series of seton threads would accomplish this object, but in a manner much more tedious and painful to the patient. The mode of obliteration of erectile tissue employed by Dr Wilmot, is a great improvement on the previous practice in such cases. Its adaptation to pendulous tumours previous to their section, will, he trusts, be found available, and is therefore recommended.

When a pendulous tumour is known, or suspected to be malignant, great care must be taken to remove the whole of the morbid parts. If the heterogeneous structure be confined to the bulb of the tumour, and the pedicle or surrounding skin be healthy, there can be no reason for doing more than simple section of the former; but the section should not, for obvious reasons, be made too near the bulb. But should the neck of the tumour be thickened, hardened, or irregular, a free elliptical incision should be made in the integument beyond it, and all suspicious parts satisfactorily removed.—*Dublin Quarterly Journal*. November 1847.

MIDWIFERY, AND DISEASES PECULIAR TO WOMEN.

CASE OF ARRESTMENT OF LABOUR—EXTRACTION OF PART OF THE FÆTUS BY THE OS UTERI—ULCERATION OF UTERUS—FORMATION OF A CYST ENCLOSING BONES.
By DR OLDHAM of London.

Mrs C—, æt. forty-one, who had experienced numerous abortions and floodings, when pregnant was again seized with flooding on the 26th of June 1845. On the following morning she had two pains. On the 30th there was diffused tenderness over the abdomen, which had the ordinary feel of a uterus at the ninth month. The os uteri readily admitted the fingers, and the fetal head, covered by its membranes, could be felt. The mammae were swollen and hard, resembling breasts the third day after delivery, and milk could be freely expressed from them. From an absence of auscultatory signs, the death of the

fœtus was inferred. July 12th.—There has been, and is now a fœtid, coloured discharge from the vagina, with occasional puffs of gas. She is weaker, and there is febrile excitement and anxiety. The os uteri was lower down, and the cranial bones of a decomposing fœtus could be felt. There had been no uterine action, and attempts were made to empty the womb. Decoction of the ergot of rye with subchlorate of soda were tried without effect. Although two fingers could be passed into the os uteri, the cervix could not be further dilated. Galvanism was now tried, but the uterus remained perfectly passive. A great deal of decomposing matter and gas were released from the womb by passing a small hook through the os, breaking up the brain, and injecting a stream of warm water within the cavity, which was frequently repeated. On the 17th of July the placenta, with the umbilical cord, in a loose and putrid state, were removed. There was during this time a constant discharge of an olive-coloured fluid, and the abdominal tumour sensibly diminished. One of the cranial bones was now removed, and the arm drawn into the vagina; no effect was produced by steady and firm traction upon it, and the arm was afterwards cut off. At subsequent visits, several bones, as they became loosened, were removed with a pair of dressing forceps. The abdomen daily diminished in size, and the discharge became puriform. Her strength was kept up by ammonia, bark, and small doses of opium. She took nourishment well, and her strength remained good until the end of September, when she appeared emaciated and feeble, and complained of abdominal pain and dysuria. The size of the abdominal tumour was now that of an orange:—it felt harder, more compact and defined. She now began to sink rapidly, and died 26th of September.

On examination after death, a cyst was opened, when the central line of the abdomen was incised below the umbilicus, which contained a number of bones, closely set together, with a quantity of thick, dark coloured putrilage. It was formed in front by the lower part of the abdominal walls and the bladder; above, by the small intestines and omentum slightly adherent together; and behind, by the posterior wall of the cavity of the uterus. This organ was about four inches in length, and its os was large enough to admit the little finger. The whole of its anterior wall was absent, leaving only the front lip of the cervix to complete the os uteri; and thus the mouth of the womb formed the outlet of the sac which contained the bones. The bladder was so thinned in one part as to be quite translucent, and all but perforated. The left ovary was healthy, but the appendages on the right side were so matted together, as to prevent their being separated. The bones were those of a full grown fœtus, clear of soft parts, and formed a dense, compact, oval body.—*Guy's Hospital Reports*, Vol. V. 1847.

CASE OF COMPLETE ANTERO-VERSION OF THE UTERUS DURING LABOUR. By DR MÜLLER, Homburg.

THE author, having been called to a woman in the country said to have been three days in labour, found the parts of the child unusually distinct on examination *ab externo*, while the most careful examination could discover no os uteri. The woman was small and of a lax habit of body, and the child seemed only to be covered by the abdominal parietes. Our author diagnosed an abdominal pregnancy, which was confirmed by two older and more experienced men called in for consultation. The question of the Cesarean section was already agitated, and anxiety as to what was to be done with the placenta was strongly felt, when one of the advisers recommended that, before proceeding further, the os uteri must be found. After great exertion it was discovered on the fifth day of labour, lying above the promontory of the sacrum, and sufficiently dilated to allow the membranes to protrude, and the child to be distinctly felt. The difficulty was now solved. The woman was placed on her hands and knees, the uterus then raised by a towel, the membranes ruptured, and the child extracted by the feet. Both mother and child did well, and the latter is now a grown woman.—*Casper's Wochenschrift*, Feb. 1847.

BIRTH OF DISPROPORTIONATE TWINS (ABORTUS RETARDATUS)—INEFFICACY OF ERGOTIN. By DR STADLER, Gelnhausen, and DR HEUSINGER, Marburg.

P. K., aged twenty-four, three years after her first delivery of a living boy, arrived at the end of her second pregnancy on the 2d October 1846. This occurred in a second marriage, during which she had suffered much from domestic discomfort, and, on the 18th of March, had narrowly escaped abortion as a consequence of a blow on the right side of the abdomen, received from her husband. A fixed pain in the right hypochondrium, and a constant bearing down of the bladder remained, however, throughout the pregnancy, which otherwise ran its course normally. During the night of the 2d and 3d October, she was delivered of a weakly and asphyxiated male child, which was, however, easily revived. The placenta was retained, and, after several hours violent vomiting, uterine hemorrhage occurred, with pain in the uterus, especially on its right side. Her face was flushed, skin warm and moist, pulse eighty and full, and the uterus was extended to the umbilicus. On examination, the umbilical cord was found lying in a mass of coagulated blood, and on removing this a solid body was felt, which, on examination, was found to be a three months' fetus, measuring $4\frac{1}{2}$ " from the crown of the head to the sole of the foot, and evidently long since dead; the genitals were not sufficiently developed to allow of the sex being determined; it was connected by a withered cord six inches long to an evidently newly separated and normal placenta, measuring in diameter 4", and in thickness $\frac{1}{2}$ ". The placenta of the first child being retained by spasmodic contraction of the neck of the uterus, Ext. sec. coru. aquos. (Ergotin) was administered, eighty grains within ten hours—2—5 grains every half hour, but without effect. The bleeding continued to recur in sudden gushes till four P.M. of the 3d, when the constitutional effects of the hemorrhage becoming alarming, the placenta was removed by the hand; a small portion was, however, retained, but came away spontaneously the next day without further hemorrhage. During the performance of this operation, no adhesion of the placenta was found, nor any duplicity nor valve in the uterus. Metritis occurred, but the mother recovered perfectly, and was able to nurse her child herself. The authors consider both children as the product of one impregnation, 1st, because the history of the case seems to place it beyond doubt; 2d, because, from what we already know, the supposition of an additional impregnation (superfoetation) in the seventh month of pregnancy is untenable. Baudelocque relates a case exactly similar. The authors conclude by stating that this is not the only case, nor the only preparation in which ergot has failed of effect, and that it is a most uncertain medicine.—*Casper's Wochenschrift*, May 1847.

ON PHLEBITIS OF THE BRAIN AND MENINGES IN PUERPERAL WOMEN. By DR F. M. DUCREST.

ACCORDING to the author, this affection in puerperal women is of rare occurrence, only five instances of it having occurred among 259 cases in which the head was examined after death. In one of these instances, the affection was not accompanied by any other cerebral disease; in the others, it occurred in combination with cerebral or meningeal inflammation.

The first case was a woman aged nineteen, presenting the appearance and physical signs of phthisis, who was delivered of a male infant at the eighth month, and afterwards became affected with frequent pulse and a peculiar tremulous motion of the eyelids, lips, and tongue. At first there was no other symptom; but on the seventh day after delivery, there was increase of the affection, with delirium, headach, and some convulsive movements of the limbs. On the ninth day the pulse and respiration were accelerated, jaws locked, tongue dry, articulation imperfect, but intelligence apparently unaffected. There was

great feebleness of all the limbs, and nearly complete paralysis on left side. In a few hours the pulse fell to sixty, the respiration became slow and laboured; soon after she died. On dissection, the meninges were perfectly healthy; the cerebral hemispheres also healthy; but on section of the right side of the pons Varolii, and the cerebral and cerebellar peduncles of the same side, the vessels in the interior of these parts were found distended to the size of a large pin by firm dark clots, which, when extricated, appeared as dark brown cylinders of above a centimetre (nearly half an inch) in length. Around these, the cerebral substance was of natural colour and consistence. The lungs were extensively tuberculated, and contained numerous caverns. On the right side of the uterus was found a vessel full of pus, surrounded by a slate-coloured induration of the substance of the organ, two millimetres (one line) in thickness.

The second case was that of a woman aged twenty-five, who was delivered naturally, but soon after was seized with pains of the hypogastrium and limbs. These were followed by shiverings, fetid diarrhoea, and colic pains. The milk was suppressed, and leech bites on the abdomen suppurated. On the ninth day, the pains being mitigated, she had severe cough with crepitant rale at the posterior and lateral parts of the right lung. From the fourteenth to the seventeenth day the right buttock swelled, and became the seat of lancinating pain; a large quantity of pus, with fetid gases and sloughs of cellular tissue, were evacuated by incision. In the mean time there had been watchfulness and delirium, with gradually increasing loss of intelligence, and moderate cephalalgia. She died on the twenty-seventh day. The pulse varied between 108 and 140, and the respiration between 28 and 48. On dissection, there was considerable subarachnoid effusion; the veins of the pia mater, on the convexity of the right hemisphere, were filled with firm, friable, adherent clots, of a whitish colour, extending in some places into the cortical substance, and being darker in colour there and in the anfractuositities. The cortical substance was somewhat softened, and of a reddish colour; white cerebral substance not altered. The lungs contained some tubercles, and the lower lobe of the right lung was the seat of lobular pneumonia; several of the vessels were filled with partially softened clots. The venous sinuses and lymphatics of the uterus were filled with pus; the broad ligaments were also infiltrated with pus. The cellular tissue between the sacrum and the left great trochanter was gangrenous, and full of pus.

The third case presented successively hypogastric pain and tenderness, with numbness and painful swelling of all the extremities, beginning with the right arm, in which she had been bled. The cerebral symptoms and progress of the case were very similar to the last case, with the additional symptom of vomiting the day before death, which happened on the 17th day from delivery. On examination, the veins of the right arm were swelled, and filled with pus; the left lateral sinus of the dura mater contained a light coloured clot, and the veins of the posterior and inferior parts of the left cerebral hemisphere entering into this sinus, were distended with dark blood firmly coagulated. There was extensive softening of the posterior part of the left hemisphere, which contained in its vessels numerous small clots. The vessels of the uterus contained pus, as in last instance.

The fourth case commenced, two days after delivery, with intense headach, convulsions, and transient stupor. On her removal to the hospital, the headach continued, with slow troubled utterance, tenderness of the abdomen, and fever. She was bled, and the blood presented no buffy coat. An hour afterwards she had a convulsion, in all respects like an epileptic attack, with coma, lasting for a quarter of an hour; and up to the morning of the next day she had twelve nearly similar convulsions. The abdominal pain continued, and she had a shivering. The fourth day after delivery there was immobility, insensibility, and contraction of the right limbs with perpetual agitation of the left; afterwards plaintive cries, coma, stertor, resolution of the right limbs, continued agitation of left; pulse irregular, 140. Death occurred next morning. On examination, there were clots in the sinuses of the dura mater; an ecchymosis

on the surface of the left hemisphere; the vessels of the pia mater in the neighbourhood of this ecchymosis contained reddish clots; the pia mater, both externally and in the ventricles, infiltrated with pus. The lungs contained miliary tubercles. The cavity of the pelvis, and various parts of the peritoneal cavity, contained pus; the uterine tissue was healthy; some clots of blood were in the venous sinuses.

The fifth case occurred in a subject affected with extensive pulmonary disease, who was seized with headach, and most of the symptoms mentioned in the first three cases, while yet undelivered, though at the full term of pregnancy. The morbid changes were in great part similar to those previously described; but the amount of softening of the cerebral substance was greater than in any of the others, and the number of veins occupied by the firm coagula smaller, so that this case seems less important in reference to the peculiarities of the disease than any of the preceding.—*Archives Générales de Médecine*. Nov. 1847.

EXTIRPATION OF BOTH OVARIES WITH SUCCESS. BY DR F. BIRD, London.

DR F. BIRD related, at the Westminster Medical Society, the case of a lady, aged thirty-two, from whom he had lately extirpated both ovaries. The preparations were exhibited, one of which was of nearly spherical form, of compound character, and had weighed about twenty pounds; the other was of remarkably irregular form, consisting of several lobules or distinct cysts, attached to a common base; these several cysts had been filled by different secretions, the larger of them having contained much cholesterine; the weight of this second tumour equalled four pounds. Both tumours were adherent to the abdominal walls, the larger one having been, in its upper portion, covered by the firmly adherent omentum, one of the arteries of which was greatly enlarged, and was ligatured. The patient experienced no untoward symptom, and rapidly recovered, menstruation having since recurred. The author directed the attention of the members chiefly to three features of the case: the probable cause of the disease, which was, in the case of the left ovary at least, to be distinctly traced to antecedent inflammation of that organ, whilst each subsequent change in the character of the disease seemed also to depend upon the accession of inflammatory states. He next spoke of the diagnosis, which in this case was at one period so difficult as to have induced him to defer the operation to the latest period, in the belief that the supervention of additional symptoms might render the propriety of performing it more certain. The chief difficulty arose from the loss of evidence commonly afforded by vaginal exploration, the uterus having been drawn up so far out of the pelvic cavity that it was impossible to make any satisfactory examination. The fact of menstruation having been uninterrupted by the previous destruction of the ovaries by disease, or by their subsequent extirpation, was interesting; and it was observed that, as in the present, so in the majority of such cases, there existed a marked tendency to menorrhagia. He regarded the case as useful, in serving to extend the limits within which the extirpation of ovarian tumours might be successfully employed, and not less so as illustrative of the period at which the operation could alone be sanctioned, when the general health of the patient was beginning to sink beneath the exhausting influence of the disease.—*London Medical Gazette*, Nov. 5, 1847.

RETROFLEXION OF THE UTERUS. BY PROFESSOR BEATTIE OF DUBLIN.

IN consequence of this affection of the uterus being almost always, both by foreign and British authors, described under the head of retroversion of the organ, it has escaped the notice of practitioners, and hence, as Dr Ashwell says, "Many question whether the uterus is ever antiverted, antiflexed, or retroflexed."—In retroversion of the uterus, the long axis of the organ becomes nearly parallel to the

plane of the brim; the womb, as it were, rotates upon its horizontal axis; hence the fundus uteri sinks backward into the cavity of the sacrum, and the os and cervix rise to the symphysis pubis. In retroflexion of the womb, no such movement occurs, but a bend takes place in the uterus at the point where the neck and the body of the organ join, and the fundus uteri sinks down between the cervix uteri and rectum; the womb is in short doubled upon itself, the cervix uteri remaining in its natural position, but on a somewhat higher level than the fundus. This position of the uterus constitutes retroflexion of the organ; by some of the French writers it is termed incurvation of the uterus, and by Meissner, Busch, and others, the retort-shaped or horseshoe-shaped curvature of the womb. This is the affection to which Professor Beattie directs attention in the short notice before us. "The angle at which the bend takes place is sometimes very acute, at others more obtuse. The displacement is commonly the result of pregnancy; it could scarcely if at all occur during gestation, owing to the fulness and tension of the uterus in that state; but in all the instances that have come within my knowledge, or that I find recorded, the occurrence of the accident was subsequent to delivery. Velpéau saw fifteen cases in which it occurred in the unimpregnated uterus, but after parturition. Retroflexion is peculiar to the unimpregnated uterus subsequent to delivery. Retroversion takes place when the uterus is distended by pregnancy. The latter may compromise the safety of the patient; but the former is productive only of distressing local symptoms, unaccompanied by danger. The time at which retroflexion takes place, is most probably immediately after delivery, when the uterus is still large, but soft and pliable; it is, however, very likely to be overlooked at this period, because the urgent symptoms do not manifest themselves until the patient rises from bed. She then probably, for a considerable time, endures these distressing sensations, under the delusive hope that they will gradually disappear as she regains strength. The organ by this delay becomes fixed in its unnatural position, a certain amount of chronic inflammation alters its tissue, and moulds it into a new shape, rendering all attempts at restoration by mechanical means alone fruitless. The curative measures to be adopted, are those we employ to restore a retroverted uterus, and, besides the mechanical means, the altered condition of the tissue and texture, as well as of the shape of the uterus, must be attended to. The chronic inflammation, which is the effect of the alteration in shape, and the cause of alteration in tissue, must be combated by appropriate treatment before we can expect success in our attempts to restore the organ to its natural shape." Professor Beattie relates three cases in proof of what he has above advanced; these cases are so similar, that it is unnecessary to quote them individually. The three patients suffered all more or less from menstrual derangement from leucorrhœa, and from a sensation of dragging weight and debility in the loins, groins, and back. The most characteristic symptom in all of them was pain and difficulty in defæcation; two of them described their sensations as of something blocking up, or arrested in the bowels, preventing the exit of the fæces. On vaginal examination, the uterus was found retroflexed, with the fundus pressing into the rectum. Dr B. could pass his finger into the angle formed between the body and cervix; there was also congestion and ulceration of the os and cervix uteri; the ulceration was successfully treated with nitrate of silver, and the congestion with leeching. By a combination of these means, together with a prolonged employment of the recumbent position, the congestion of the uterus gradually disappeared, and in one case the organ becoming by degrees smaller, could be at length pushed into its natural position, still, however, with a tendency to fall back again; this was remedied by means of a sponge pessary, and the patient perfectly recovered. In the second case, the condition of the patient was very much improved, but the uterus could not be artificially restored to its natural position; nature at length, after a subsequent delivery, effected a cure, care being taken that she should not be permitted to rise from bed too soon after parturition. The third case underwent considerable improvement; but no cure was effected in consequence of the patient's refusal to submit to the necessary treatment.—*Dublin Journal of Med. Science*, November 1847.

[We are of opinion that Dr Beattie has quite under-stated the frequency, &c., of retroversion and retroflexion of the unimpregnated uterus. For the last two years our Edinburgh accoucheurs have become convinced that it is an exceedingly common displacement—easily detected by a means to which Dr Beattie does not allude, viz., the uterine sound—and in its more marked forms requiring the use, for some time, of a wire pessary introduced into the cavity of the uterus.]

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVII.

MEETING I.—*Wednesday, November 10th, 1847.*—DR ALISON, V.P., in the Chair.

[We never remember having seen a more crowded meeting of the Society, which was occasioned by the desire to hear the two following important communications by Professors Syme and Simpson. The first of these we give entire.]

CASE OF TUMOUR OF THE NECK, SIMULATING ANEURISM OF THE CAROTID ARTERY.

By JAMES SYME, Esq., Professor of Clinical Surgery in the University of Edinburgh.

ABOUT a month ago a young man called upon me, to get my opinion of a swelling in his neck. It was seated on the right side, and occupied the upper triangular space. It was of an oval form, quite circumscribed, and obviously consisted of a bag containing fluid. Upon more particular examination, I found a distinct pulsation of the kind which I had been accustomed to regard as characteristic of aneurism, being an expansive impulse, not limited to a portion of the tumour, but felt equally at every accessible point, even from the mouth, and more especially in a lateral direction. The patient stated that the swelling had commenced in the beginning of the present year—that is, about nine months ago—and had progressively enlarged without any cause that had been ascertained. He also stated, that when he worked hard or walked fast, the tumour increased in size, and had a strong beating in it. I felt satisfied that there was an aneurism of the carotid artery, but expressed no opinion at the time, and desired the patient to call again for further examination. When he did so, I varied the process by placing him in different positions—by trying the effect of pressure on the tumour and artery—and by listening to the sounds of the tumour. There was no distinct aneurismal “bruit,” but a very strong loud pulsation, that implied the action of the heart upon an extensive surface. Finding my impression thus confirmed, I informed the patient of my apprehension; but, before giving a decided opinion, requested that he would call once more. He did so a few days afterwards, and I then felt fully warranted to inform him that an operation would be requisite for his relief. Next day he placed himself in my hands for this purpose, and was admitted into the Minto House private hospital. After he had been confined to bed for a few days, I tied the artery below the crossing of the omo-hyoideus, as the tumour prevented this from being done higher up. The textures of the neck were more than usually adherent, and the vessel was not exposed to view with nearly the same facility as upon the former occasions which have required me to perform the operation. I nevertheless succeeded without any tearing, or undue disturbance of the parts, in passing and tying the ligature, so as to relieve me from the slightest apprehension of any bad

consequences. The tumour immediately sustained a very distinct diminution of bulk, which was remarked not only by the gentlemen present, but by the patient. He went on most favourably after the operation until the fifth day, when hemorrhage took place from the wound, and, notwithstanding every effort to effect prevention, recurred from time to time until the evening of the twelfth day, when it proved fatal.

The parts concerned were examined next day in the presence of Drs Scott, Duncan, Mackenzie, Peddie, Brown, Gillespie, and Ballingall. We found a tumour extending from the ear to the extremity of the omo-hyoideus, and completely occupying the upper triangular space of the neck. At the lower part it seemed to terminate in the sheath of the vessels, which looked like a prolongation of it downwards, but was found to be merely enveloped by the bag, which I dissected out entire from the coats of the vessels to which it had intimately adhered. The cyst, when opened, was found to possess a tough consistence, and to contain a fluid like thin gruel. At the posterior part, when viewed internally, it displayed a sacculated or honeycomb looking structure.

I have deemed it right to communicate this case to the Society without delay; in the first place, because the names of several gentlemen have been mentioned in connexion with its previous treatment, and it is but due to them that the true state of matters should be made known. And secondly, because I wish to discharge the duty which devolves upon every member of the profession who is so unfortunate as to commit a serious error of practice, by faithfully stating the circumstances which misled me, so that they may be rendered less likely to cause similar mistakes in future.

I have had extensive opportunities of observing tumours of an aneurismal kind, and it was impossible for me to bestow more care in any instance than in the case just related. But it appears that the character which I have hitherto regarded as the most certain indication of aneurism, may be presented by a peculiar state of things of an entirely different kind. I allude to the general expansive pulsation, especially when felt in a lateral direction. This was most distinctly recognised not only by myself, but by several gentlemen well practised in the diagnosis of aneurismal tumours, who, in consequence, agreed with me in believing that an aneurism existed. Having come to this conclusion, and being requested by the patient to do what seemed requisite for his relief, I had no course but to tie the artery. The low situation to which the ligature was restricted, with the consequent proximity of the innominata, and the intimate adhesion of parts, no doubt led to the hemorrhage.

The painful feelings excited by such an occurrence, are on the present occasion somewhat relieved by the reflection, that I did not err from any want of care, and that I proceeded on the ground of my most deliberate judgment, in accordance with what I believed to be a well-established principle of surgical diagnosis. I also derive comfort from the persuasion, that a fatal issue must, sooner or later, have resulted from the case, whether it had been left to itself, or been subjected to surgical treatment. It is plain that no sort of external application or internal remedy could have had any beneficial effect on the tumour. The patient had used liniments and blisters, and taken large quantities of cod liver oil, under the impression, I suppose, that the swelling depended upon an abscess; but with no check to the enlargement, which had progressively increased with accelerating rapidity. In such cases I have repeatedly dissected out the cyst, and did so with success upon one occasion in the hospital, where the tumour extended from the mastoid process to the clavicle. But in the present instance it would have been impossible to effect removal in the living body, since it was only by cutting, without regard to the integrity of the great vessels, that the bag was separated in an entire state from their coats. The only remaining mode of procedure, therefore, was to lay the sac freely open, so as to induce suppuration and granulating contraction of the cavity. The last case of the kind that occurred in my

practice, was treated in this way, as Dr Richard Mackenzie, who assisted me, may remember. It is between two and three months since the operation was performed, by removing the anterior or superficial part of the cyst; and the patient is now nearly, if not quite well. But it is well known that when a large artery lies in close proximity to the suppurating surface of a sinus or open cavity, ulcerative absorption of the septum between them is apt to occur, and occasion hemorrhage. I was asked by Dr Douglas to see a man under his care in the hospital last winter, on the lower part of whose neck, and upper part of the sternum, a large swelling had taken place. In laying my hand upon it, I felt a strong pulsation, which ceased during inspiration; and I therefore concluded that the tumour proceeded not from an aneurism but an abscess. As there was a blush on the most prominent part of the swelling, and as the patient did not seem to suffer any particular inconvenience from its presence, I advised that the matter should be allowed to find vent spontaneously—since it seemed very probable that, after evacuation was accomplished, ulceration of the arterial coats adjoining to the cavity would ensue. In the course of a few days the matter was discharged by the natural process, and a few days afterwards a gush of blood proved instantly fatal to the patient. Now, I think it extremely probable, or rather, almost certain, that if a cyst so intimately connected with the carotid artery had been converted into a suppurating sinus, hemorrhage no less profuse than uncontrollable would have been the consequence. If, again, the disease had been left to pursue its course, there can be little doubt that the swelling must, before long, have seriously interfered not only with the circulation of the head, but also with deglutition and respiration—and that its spontaneous conversion into a sinus would have been attended with no less risk of hemorrhage than if this change had resulted from surgical interference. These considerations of course afford no excuse or apology for the mistake that was committed; but, as already remarked, tend to lessen the painful feelings excited by its consequences.

Professor Alison remarked, that he was sure there could be only one feeling as to the manly and open manner in which Mr Syme had made known the details of this case. It was important also to remark, that even external aneurisms could not be diagnosed with certainty by experienced practitioners. With regard to internal aneurisms, he had long been convinced that in many cases their detection was impossible. Not long ago he had a case of pulsating tumour in the epigastrium, with an expansive swelling and impulsive bruit, which however, after death, was ascertained to be owing to a mass of enlarged cancerous glands, which received an impulse from the aorta below.

HISTORICAL RESEARCHES REGARDING THE SUPERINDUCTION OF INSENSIBILITY TO PAIN
IN SURGICAL OPERATIONS; AND ANNOUNCEMENT OF A NEW ANÆSTHETIC AGENT.
By PROFESSOR SIMPSON.

DR SIMPSON first entered at some length into the chemical and therapeutic history of sulphuric ether. He traced it from Lully, Hollander, Valentine, &c., in the 13th and 14th centuries, down to Valerius Cordus, who described its formation accurately in the 16th century; and Frobenius, who first designated it ether, and published an account of it in the *Philosophical Transactions* of 1730. He then pointed out its therapeutic history, from the first mention of it as a medical agent by Valerius Cordus, down to the works of Hoffman, Cullen, Alston, Lewis, Monro, and the other writers on the *materia medica* during the last century. The history of its use by inhalation commences with the pamphlet of Richard Pearson, published upon it in 1795. Since his time most therapeutic authors mention, more or less fully, its employment by inhalation in asthma, &c., as Duncan, Murray, Brande, Christison, Thomson, Nysten, Barbier, Wendt, Vogt, Sundelin, &c. Its power of producing, by inhalation, effects like intoxication, or like the influence of nitrous oxide gas, he showed to have been stated by various American authors, as by Professor

Samuel Jackson (1833), Wood and Bache (1834), Miller (1846), before it was so fortunately adopted by Dr Morton as an anæsthetic agent. His belief was, that Professor Charles Jackson improperly claimed the merit pertaining to its recent happy application to surgery, &c. Perhaps the idea of relieving patients from the pains of surgery by some such means, or rather the restoration of that idea in recent times (for it was an old one), belonged justly to Horace Wells.

Dr Simpson then proceeded to show, that the idea was not one entirely of our own times, by taken a retrospective glance at the history of *painless* operations. Among external measures proposed with this view, he mentioned the idea of Moore (1784), to compress and obtund previously the nerves of the implicated part; the alleged ligature or compression of the larger vessels of the neck mentioned by Hoffman, Valverdi, Morgagni, &c. He then showed that various internal agents, introduced by inhalation and otherwise, had been at different times suggested and employed; as 1. In 1828, the previous inspiration of carbonic acid (?) gas proposed by Dr Hickman; 2. In 1800, the hint of Davy regarding the application of nitrous oxide for this purpose; 3. The inhalation of vapour arising from the watery extract of mandragora, opium, &c. He read at length the following remarkable passage, (which he had found in the old surgical treatise of Theodoric, who died in 1298,) as an odd forestalling of the practices of the present day. In the rubric of the work the receipt is described as "*Spongia somnifera*."

"The preparation of a scent for performing surgical operations, according to Master Hugo; it is made thus:—Take of opium and of the juice of the unripe mulberry, of hyoscyamus, of the juice of the hemlock, of the juice of the leaves of mandragore, of the juice of the woody ivy, of the juice of the forest mulberry, of the seeds of lettuce, of the seed of the burdock which has large and round apples, and of the water hemlock, each 3 i; mix the whole of these together in a brazen vessel, and then in it place a new sponge, and let the whole boil; and as long as the sun on the dog-days, till it (the sponge) consumes it all, and let it be boiled away in it [or, in other words, let a watery extract be so formed]. As often as there is need of it, place this same sponge into warm water for one hour, and let it be applied to the nostrils till he who is to be operated on (*qui incidendus est*) has fallen asleep, and in this state let the surgery [or operation] be performed [*et sic fiat chirurgia*]. When this is finished, in order to rouse him, place another sponge dipped in vinegar frequently to his nose; or let the juice of the roots of fenigreek be squirted into his nostrils. Presently he awakens."

The above remarkable anæsthetic means was recommended in the thirteenth century. Mandragore was used even much earlier for the same purpose. In proof of this Dr Simpson adduced various extracts from the works of authors of the Augustine age, as Dioscorides, Pliny, and Apuleius; as, for example, the following from Dioscorides' chapter on mandragore—

1. "Some boil down the roots in wine to a third part, and preserve the juice thus procured, and give one cyathus of it in sleeplessness, and severe pains of whatever part; also (it is given) *to cause the insensibility* (*ποιεῖν ἀναίσθησιαν*) *of those who are to be cut or cauterized.*"

2. "There is prepared also, besides the decoction, a wine from the bark of the root, three *minæ* being thrown into a cask of sweet wine; *and of this three cyathi are given to those who are to be cut or cauterized, as aforesaid; for, being thrown into a deep sleep, they do not perceive pain.*"

3. Again, of another mandragore called "*Morion*," Dioscorides observes, "medical men use it also for those who are to be cut or cauterized."

When treating of mandragore, Apuleius in the same way remarks, "If any one is to have a member mutilated, burned, or sawed [*mutilandum, comburendum, vel serrandum*], let him drink half an ounce with wine, and let him sleep till the member is cut away without any pain or sensation" (*et tantum dormiet, quousque abscindatur membrum aliquo sine dolore et sensu*).

In describing the leaves of circeius or mandragore, Pliny remarks, "It has a soporific power on the faculties of those drinking it. An ordinary potion is half a cup. It is drunk against serpents, and before cuttings and puncturings, lest they be felt. (*Bibitur et contra serpentes, et ante sectiones punctionesque, ne sentiantur*)." The same author mentions that the seeds of rocket (*eruca*), when drunk infused in wine by criminals about to undergo the lash, &c., produce a certain callousness or hardihood of feeling (*duritiam quandam contra sensum induere*). Dr Simpson referred to Bang or Indian hemp being used in India with the same object at the present day, and cited various authorities to show that the gall and vinegar, or myrrhed wine offered to our Saviour before his crucifixion, was probably at that time generally employed with the same object. Dr Simpson referred to the inhalation of the intoxicating vapours of hemp-seed, as having been practised by the Scythians long before the Christian era, as described by Herodotus, &c.

After some further similar observations, Dr Simpson proceeded to draw the attention of the Society to the use of Chloroform, the powerful anæsthetic agent which he had lately discovered, in making a series of experiments on different respirable substances. The substance of these observations has been published in a separate pamphlet. It is given, with considerable additions, in a former part of this Number.

Dr Bennett stated that he had inhaled the chloroform, the taste of which was exceedingly pleasant. It had produced a warm glow throughout the chest, which seemed rapidly to affect the brain. After a few inspirations, he felt that one more would render him perfectly insensible, and therefore discontinued it. He had never inhaled ether, and could not speak as to its comparative merits. But the absence of all apparatus, the simple pocket-handkerchief, and agreeable odour, had induced him to try the new preparation, and he had not experienced any unpleasant effects.

Dr Robertson stated that he also had inhaled the chloroform. He had frequently employed ether to remove a severe tic in the foot, and could assure the society that the new preparation was infinitely more agreeable and certain in its effects than ether. In a young lady labouring under dysmenorrhœa, and affected with suppuration of the ankle joint, whose cries were distinctly heard in the street, he had lately seen the best effects from ether inhalation, which at once removed the pain, and enabled her to enjoy a tranquil sleep. Four menstrual periods have since passed without pain. He had also seen it employed during the operation of firing in horses, which produced no pain, nor even the slightest perspiration. In another case of lock-jaw in a horse, occasioned by a fall into a quarry, and which had prevented deglutition for four days, ether inhalation had completely and permanently removed it.

Dr Simpson requested that the President would nominate a committee to investigate experimentally the properties of the new substance, and report at the next meeting. This was agreed to, and the following gentlemen were appointed for that purpose. *Dr Hughes Bennett, Convener*—Drs Alison, Christison, D. MacLagan, Taylor, Duncan, and Roberts, with power to add to their number.

The Society then adjourned, when a very singular scene took place. Dr Simpson having stated that any gentleman who felt inclined might try the effects of chloroform on his own person, Mr Young, cutler, at once presented himself. About a tea-spoonful of the preparation was placed on a silk pocket-handkerchief, folded together in the hand. After a few inspirations, complete insensibility was produced, on recovering from which, he said that he had experienced exactly the same sensations as when he had taken ether. Dr Roberts then inspired from the handkerchief, with the same result. Several other gentlemen followed in succession. In one (Mr Hunter) some excitement was produced. He stood up, and on being held made some resistance. He afterwards declared that the effect produced on him was similar to that

occasioned by the nitrous oxide gas. Whilst the bustle attendant on this case was in progress, the handkerchief seemed to have circulated among the members, who applied it to their faces, and were unexpectedly surprised by the effects produced on them ; so that, when we looked round, we saw more than one gentleman insensible, and several others in various stages of apparent intoxication. They all however rapidly recovered, and declared that they had experienced no unpleasant symptom ; whilst a few stated their sensations to have been so delightful, that they should have no objection to their repetition.

ARE SCOTCH PRACTITIONERS TO REMAIN UNREMUNERATED FOR DANGEROUS SERVICES ?

OUR medical contemporaries of the sister isle have very properly been loud in their denunciations against the system which calls upon the profession to relieve the destitute poor without sufficient reward. The public press has seconded their efforts, and we have, in consequence, heard an immense deal about Irish sufferings, Irish sacrifices, and Irish dangers. The subject has been forced upon the attention of the local boards, formally brought under the notice of government, and the sympathy of the public largely awakened. Meetings have been held, petitions numerously signed, and every death affecting one of the body medical has been represented as a new wrong done to Ireland. Now, whilst our brethren on the other side of the Channel have undoubtedly much to complain of, the hardships they set before us are in no way confined to themselves. Indeed, the loud declaimers in Ireland occupy a much better position than the forbearing and uncomplaining practitioners in Scotland. There, it seems, they obtain five shillings a-day, which, though a miserable pittance, is still L.91, 5s. a-year. But here we have yet to learn that any thing whatever is gained but the eminent risk of disease and death. Moreover, we have not only to bear the burthen of attending our own countrymen, but are overwhelmed with crowds of sick Irishmen, whose ignorance is so great, and squalid habits so rooted and unchangeable, as to set at defiance every effort made for their own and the public safety.

In the midst of these discouraging circumstances, Scotch practitioners have toiled on ; they have gratuitously attended fever wards and fever sheds ; they have been stricken down on the bed of sickness ; they have died ; their families have been left destitute ; and, up to the present hour, no voice has been raised in their behalf. No sooner was Dr Hughes Bennett appointed to the Fever Hospital than he was attacked by the disease, and nearly lost his life. Drs Coldstream and Sellar have just narrowly escaped the same danger, from the same cause. Dr Tait, surgeon to the police force, has lately died of fever in the performance of his duties. Every year we have to deplore the loss of one or more promising young men, swept off in a similar way. In short, medical men in Edinburgh look forward to being wounded or killed by the fever, with much greater certainty than soldiers who enter upon the most bloody campaign. Thus, fever may be regarded as a destructive enemy, which is constantly combated with the utmost bravery and devotion by the medical army, without the hope of pay, reward, or distinction.

Now, we believe that this great grievance is entirely owing to a want of discipline and tactics in the said army. It requires a little of the drilling and manœuvring of our Irish allies. Why should not we meet together, sign petitions, and represent our wrongs ? Are there no local boards, is there no government, no public sympathy for us ? Do we undergo no sufferings, make no sacrifices, and meet no dangers ? Why should a medical man in Scotland get nothing, whilst one in Ireland obtains five shillings a-day, and yet complains loudly that he does not get more ? We seriously ask our brethren what they can be about, to suffer such things ? Will they go on subject to these injuries, or seeing them inflicted, and make no effort as a body to re-

move them? We cannot believe it—Passive endurance has reached its limits, and the time has arrived when Scotch as well as Irish practitioners ought to come forward and make their voices heard upon this all-important subject. We live in the midst of one pestilence, and another is said to be rapidly approaching. If medical practitioners are required to perform highly important and dangerous duties for the public benefit, they have every right to obtain from that public a fair reward for their professional knowledge, loss of time, and increased labour. We see that in Ireland the system of agitation has done something, though not perhaps so much as is desirable. There, during the cholera period, they succeeded in extracting from government to pay physicians from one to two guineas daily, and it is argued that the same should be obtained in the time of fever. Yet Scotch practitioners attended cholera patients in times past, as fever patients now, and neither then, nor at present, have ever received one farthing, or even thanks for their exertions. Moreover, they pay in the form of taxes the very sums which are given to their Irish brethren, whilst they themselves, so far from being benefited, are in every way losers by their philanthropic endeavours.

It must be clear, therefore, that however bad the case of Irish medical men may be, that of Scotch practitioners is infinitely worse; and that, if government think it right to remunerate the one class, they are bound, by every principle of equity, to remunerate the other. We must remind our brethren in Scotland, that *the remedy is entirely in their own hands*. Let the profession be only true to itself—let all its members unite for mutual advantage and the common good, and there can be little doubt regarding the result.

INHALATION OF CHLOROFORM.

In the present Number will be found a full and condensed account, by Professor Simpson, of the new anæsthetic agent he has discovered, and which, from the numerous trials now made of it, there can be little doubt, will wholly supersede the use of ether for annulling pain. The simplicity of the apparatus employed (a common handkerchief), the agreeable odour, the pleasant sensations produced, the rapid action, and the small quantity necessary, will at once bring it into universal request. When manufactured largely, also, we have little doubt it will be made cheaper than at present, and this especially if a new process be invented for its formation, which at present seems to be a desideratum.

Our readers will remember, that when ether was first introduced, we pointed out that dangerous effects sometimes resulted from its use in certain individuals. These effects, we now know, were entirely owing to the imperfect apparatus and want of skill employed in its administration. They were not, however, on that account, less dangerous; and a want of proper precautions with the new agent, inasmuch as its power is much more intense, will certainly lead to even greater mischief. In many persons, however, we are bound to say, very grave symptoms have been only hazardous in appearance. We have seen it produce stertorous breathing, foaming at the mouth, tetanic rigidity, and rapid and alarming fall of the pulse; and yet such individuals have recovered as well, and with as little inconvenience, as those who have been thrown into a tranquil sleep. Whilst, therefore, such symptoms should induce extreme caution, observation hitherto has not demonstrated that they are really dangerous.

A committee, appointed by the Medico-Chirurgical Society, is closely investigating the effects of chloroform on man and animals. The report will probably be presented at the next meeting. As our January Number, however, will not appear for nearly a month afterwards, we may state, for the information of our readers, that its effects on the lower animals are precisely the same as in man, producing profound sleep and insensibility, from which they gradually recover. A large dose (3ij), however, continued from three to six minutes, has in pigeons and small dogs proved fatal.

We have heard of operations having been performed under the influence of chloroform in London, Glasgow, and other places, and every where with the same favourable and satisfactory results. In Edinburgh it has been used publicly by all the surgeons of the Royal Infirmary, and its employment in midwifery practice is almost universal. Ether seems to be already abandoned.

BOOKS RECEIVED.

1. Observations on some of the Parts of Surgical Practice, &c. By John P. Vincent, late Senior Surgeon to Bartholomew's Hospital. London. 8vo. 1847.

2. A Practical Treatise on the Causes, Symptoms, and Treatment, of Spermatorrhœa. By M. Lallemand. Translated and Edited by Henry J. M'Dougall, M.R.C.S., &c. London. 8vo. 1847.

3. Guy's Hospital Reports. Second Series. Vol. V. London. 8vo. 1847.

4. Elements of Chemistry. By the late Edward Turner, M.D., &c. Eighth Edition. Edited by Baron Liebig and William Gregory, M.D., &c. Part II. Organic Chemistry. London. 8vo. 1847.

5. On the Inhalation of the Vapour of Ether in Surgical Operations, &c. By John Snow, M.D., &c. London. 8vo. 1847.

6. Hassall's Microscopic Anatomy. Part II.

7. Lectures on the Physical Phenomena of Living Beings. By Carlo Matteucci, &c. Translated under the superintendence of Jonathan Pereira, M.D. F.R.S., &c. London. 12mo. 1847.

8. Deafness Practically Illustrated, &c. By James Yearseley, M.R.C.S.E., &c. London. 8vo. 1847.

9. An Introductory Lecture, read to the Medical Classes in King's College London, October 1st, 1847. By George Budd, M.D., F.R.S., &c. London. 8vo. 1847.

10. On Ringworm, its Causes, Pathology, and Treatment. By Erasmus Wilson, F.R.S., &c. London. Small 8vo. 1847.

11. Three Tables for Students. By William E. C. Nourse, M.R.C.S. to. London.

12. An Essay on the Diseases of the Jaws, and their Treatment. By Leonard Koecker, Surgeon-Dentist, &c. New Edition, with Notes and an Appendix, &c. By J. B. Mitchell, M.D., Surgeon-Dentist. London. 8vo. 1847.

13. Account of a New Anæsthetic Agent as a substitute for Sulphuric Ether in Surgery and Midwifery. By J. Y. Simpson, M.D., F.R.S.E., &c. Edinburgh. 8vo. 1847.

14. Observations on Pleuritis and Empyema in Children. By F. Battersby, M.D., &c. Dublin. 8vo. 1847.

15. On the Contractibility and Irritability of the Muscles of Paralyzed Limbs, &c. By R. B. Todd, M.D., &c. 8vo.

16. Physiology of the Nervous System. By Robert B. Todd, M.D., &c. London. Royal 8vo. 1847.

17. Die Krankheiten der Arbeiter in den Phosphorzündholzfabriken, &c. Von Dr. Freiherrn Ernst v. Bibra und Dr. Lor. Geist. Mit neun gemalten Kupfertafeln. Erlangen. 8vo. Atlas. 1847.

18. Untersuchungen über die Galle und ein Wort über Protein. Von G. J. Mulder, &c. Unter des Verfassers Mitwirkung übersetzt. Von Dr. A. Völcker. Frankfurt, A. M. 8vo. 1847.

19. Zur Pest, und Quarantainefrage. Von Dr. Carl Ludwig Sigmund, in Wien, &c. 8vo.

20. Die Krankheiten des Orients vom Standpunkte der Vergleichenden Nosologie betrachtet. Von Dr. F. Premer. Erlangen. 8vo. 1847.

21. Der religiöse Wahnsinn. Ein Beitrag zur Geschichte der religiösen Wirren der Gegenwart. Von Dr. Karl Wilhelm Ideler, &c. Halle. 8vo. 1847.

22. Traité Théorique et Pratique de la Folie. Par M. Parchappe, M.D., &c. Paris. 8vo. 1841.

23. Recherches Historiques et Critiques sur la Démonologie et la Sorcellerie. Par M. Parchappe, &c. Rouen. 8vo. 1843.

24. Notice Statistique sur l'Asile des Aliénés de la Seine, inférieure, &c. Par MM. L. Deboutteville et M. Parchappe, &c. Rouen. 8vo. 1845.

25. Du Cœur, de sa Structure et de ses Mouvements. Par M. Parchappe, &c. Paris. 8vo. 1844. Atlas de 9 Planches. Paris. 4to. 1846.

TO CORRESPONDENTS.

Communications have been received from Dr Turner of Keith, and Mr Frazer of Glasgow.

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No. 19. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Nature, Cause, and Prevention of Scurvy.* By
ALFRED B. GARROD, M.D., Assistant Physician to University
College Hospital, London.

HAVING for many years paid considerable attention to the application of chemistry in the investigation of disease, the subject of scurvy has frequently presented itself to my mind; and, as there is but little doubt that the disease arises from some alteration in the animal fluids and solids from error of diet, I have always considered that it was in the power of chemistry to unravel the mystery. During the early part of the present year, many cases came under my care at University College Hospital: this led me again to pay particular attention to the subject, and to examine the peculiarities of the food, and other causes, which could have produced the disease at that time; for until then I had scarcely seen a case of scurvy in London.

It is not my intention to detail the symptoms which presented themselves in my scorbutic patients. I have however noticed, that the solid effusions in the ankles, with petechiæ and discoloration of the lower extremities, have, in several cases, preceded the spongy condition of the gums; there has also been the sallow skin, great muscular weakness, with rheumatic pains; but these have been so recently and ably described in *The Monthly Journal* by Drs Christison, Ritchie, Lonsdale, &c., and in many other recent periodicals, that it is unnecessary to recapitulate them.

Causes of Scurvy.—From the different writers on scurvy it appears, that although impure air, cold, moisture, age, and condition of

habit, may favour the occurrence of this disease; yet no one of them can be regarded as the real cause, which must be sought for in the nature of the food. This is fully proved in the writings of Lind, Trotter, Budd, Christison, Curran, Ritchie, &c. It appears also, that it is due to the *absence* of some essential ingredient in the food, and not from the *presence* of any noxious substance; for the use of salt in large quantities is certainly not a cause, many of the most severe cases of disease having occurred where no such diet had been used, and sea water has never been found to aggravate the symptoms of the sufferers. The causes of the disease are thus reduced to one of the two following, viz. :—

1st, To the absence or deficiency of some *organic* substance in the food.

2d, To the absence or deficiency of some *inorganic* constituent.

There are many facts to support the opinion, that it is some organic constituent that is deficient in the food, and this is usually believed to be of an acid nature; for it is generally found that scurvy has appeared when there has been a want of succulent vegetables, and that the disease is easily cured whenever they can be supplied in abundance: these succulent vegetables mostly contain some organic acid. It has also been found that fruits, from the order Aurantiaceæ, containing much of such acids, are highly anti-scorbutic. But there are many objections to this view; for although fruits and vegetables containing these acids are exceedingly useful, yet the acids themselves, when separated, are not so, and I am informed, on good authority, that citric acid has been used and found not to be anti-scorbutic; the same remark applies to acetic acid. Again, carnivorous animals live entirely on meat without suffering from such disease, and infants will sometimes live for eighteen months on milk and not show any scorbutic symptoms. Milk is also found to act as an excellent remedy in scurvy, although, when fresh, it contains no organic acid. These facts are therefore sufficient to show, that it is not to the absence or deficiency of organic acids in the food that scurvy is due.

Dr Christison thinks that the main peculiarity in scorbutic diet is the deficiency in the quantity of animal nitrogenized principles, and that this may be effectually counteracted by milk, and other nitrogenized articles of food from the animal world. He also thinks that potatoes may owe their anti-scorbutic properties to the albumen contained in them; but if we only examine the diets under which some patients become scorbutic, and others under which they remain in a healthy condition, we must be at once convinced that it is not from the want of nitrogenized principles that the disease arises; for we observe in the diet of sailors who become scorbutic, abundance of these principles, and in many of our union workhouse and prison dietaries, they are very much reduced in quantity; yet no such disease arises when a few pounds of potatoes are added per week, although the amount of albuminous matters contained in them is far

from sufficient to make the total quantity of such principles equal to that found in many scorbutic diets. Again, if this theory were true, why should so very small an amount of fruits, and succulent vegetables, act in so surprising a manner in curing the disease?

Some have supposed that vegetable acids are not required, but that there exists a *something* in fresh vegetables which acts by a kind of catalytic power, and which is necessary to cause certain changes to take place in the body by its presence; but as there is no proof of such an hypothesis, we will not stop to consider it.

We have not, then, found that the absence or deficiency of any organic constituent of the food can be proved to be the real cause of scurvy.

The analyses of blood hitherto made, have not thrown any light on the cause or nature of this disease. Recent examinations have shown that the blood is not in a dissolved state as was formerly supposed, but that the globules are normal in appearance, and the clot firm, and frequently buffed and cupped. The fibrin has been found in excess by Busk, Becquerel and Rodier. I have also observed a small firm clot in scorbutus, sometimes also it has been cupped and buffed. The red globules and organic matters of serum have been generally observed to be deficient, so that the blood appears to be *impoverished* in its nature.

The saline matters in the serum have been found in about the normal proportions. In the analyses which I made—one of healthy, the other of scorbutic blood—I found in the former 100 parts of dried serum gave 7·609 of inorganic matters—in the latter, 8·125; but if the density of the serum in scurvy is less than in health, the 100 parts of dried matters corresponded to a larger amount of serum.

Finding that all the theories of scurvy hitherto advanced were imperfect, I was led to examine more minutely the composition of food under the use of which scurvy was capable of occurring, and also of such substances as had been proved beyond doubt to be anti-scorbutic, and afterwards to seek for the absence or deficiency of certain normal substances in the blood; and from such examinations I was led to the following conclusions:—

1st, That in all scorbutic diets, *Potash* exists in much smaller quantities than in those which are capable of maintaining health.

2nd, That all substances proved to act as anti-scorbutics contain a large amount of *Potash*.

3d, That in scurvy the blood is deficient in *Potash*, and the amount of that substance thrown out by the kidneys less than that which occurs in health.

4th, That scorbutic patients will recover when *Potash* is added to their food, the other constituents remaining as before, both in quantity and quality, and without the use of succulent vegetables or milk.

5th, That the theory which ascribes the cause of scurvy to a

deficiency of *Potash* in the food, is also capable of rationally explaining many symptoms of that disease.

Before attempting to prove any of these propositions, I will give a table containing the amount of potash contained in several articles of diet. The potash was determined by forming the double salt with the bichloride of platinum ($KC + Pt Cl_2$), $\frac{19.33}{100}$ giving the per centage of potash (KO).

Analyses showing the amount of potash (KO) in 1 ounce avoirdupois of the following substances :—

	Grains
1 oz. of Baker's Best Bread (City),.....	0.259
1 oz. of Best Bread (West End),.....	0.257
1 oz. of Home-made Bread, probably containing potatoe flour,	0.262
1 oz. of Best White Flour.....	0.100
1 oz. of Bran,	0.609
1 oz. of Rice,	0.005
1 oz. of Rice,	0.011
1 oz. of Oatmeal.....	0.054
1 oz. of Split Peas,.....	0.529
1 oz. of Raw Beef,.....	0.599
1 oz. of Salt Beef, raw,	0.394
1 oz. of Salt Beef, boiled (slightly salted),	0.572
1 oz. of Boiled Mutton,	0.637
1 oz. of Dutch Cheese,	0.230
1 oz. of Boiled Potato of large size,	1.375
1 oz. of Raw Potato (small),	1.310
1 oz. of Boiled Potato, without peel and well done, water containing much potash,.....	0.529
1 oz. of Onion (small),.....	0.333
1 fluid oz. of London Milk,	0.309
1 oz. of Orange (not ripe) including septa,	0.675
1 fluid oz. of Lime Juice,.....	0.352
1 fluid oz. of Lemon Juice,	0.846

(1.) *Potash is deficient in scorbutic diets.*—This is easily proved by examining some of the dietaries of Union workhouses and prisons, when the inmates have become scorbutic. Thus in the Crediton Union, see *Provincial Medical and Surgical Journal*, June 1847, the usual weekly dietary consisted of—

	Men.	Women.
Bread,	102 oz.	85 oz.
Cooked Meat,	12 —	12 —
Soup,	3 pints,	3 pints.
Broth,	4½ —	4½ —
Cheese,	8 oz.	8 oz.
Rice, or Suet Pudding,	14 —	14 —
Potatoes,	4½ lbs.	4½ lbs.

If we estimate the amount of potash taken by the inmates of this workhouse, we shall find the men's food to contain about 186 grains, and the women's about 181 grains. This amount would be much influenced by the mode in which the potatoes were cooked; if not too much boiled, and with the skins on, they would contain a much

larger amount of potash than if boiled without their skins, and much done. Under this diet the inmates remained healthy; but, owing to the scarcity of potatoes, boiled rice in equal weights was substituted, and in a few months the inmates became scorbutic. When the substitution was made, the weekly amount of potash taken by the men was about 51 grains, and by the women 46 grains, or a reduction of more than two-thirds took place. Rice and potatoes do not differ much in their composition, except in the salt of *potash* contained by the latter. Both contain starch and vegetable albumen. In the weekly diet list for the military prisoners at the Milbank Penitentiary, when they were subject to scurvy, (see Dr Baly's paper in *London Medical Gazette*, Vol. I. 1841-2,) we find the amount of potash taken by each prisoner during the first three months of imprisonment to be about 44 grains; during the second three months, about 50 grains; after six months, about 68 grains. At present, when potatoes are added, the weekly amount of potash is from 210 to 230 grains, and no case of scurvy has arisen since the change.

Again, if we examine some of the diets used by the labourers suffering from scurvy, and detailed in the last July number of this Journal, we shall perceive that the weekly amount of potash varied from 20 to 80 or 90 grains, and that this was contained chiefly in the bread, the analysis of which gave a much greater amount than the corresponding quantity of flour, thus indicating some admixture, as of alum and potatoe flour.

In the cases which have occurred under my care, the diets have consisted chiefly of bread, butter, little or no milk, and no potatoes, occasionally a small piece of meat, bacon, or a salt herring, and a similar diet has been found to have been used by most patients who have become scorbutic within the last year. (See paper on scurvy by Dr Curran, in *Dublin Quarterly Journal* for August 1847, and by Dr Shapter, in the *Provincial Medical and Surgical Journal*).

In the diet of sailors we find abundance of meat (salted beef and pork); but no doubt the quality of these provisions is often much impaired by the prolonged action of the salt, causing the gradual exosmosis of the potash salts, and the substitution of those of soda. In an analysis of beef which has been exposed for only a few days to the action of brine, but where the thickness of muscle was not more than $1\frac{1}{2}$ inch, there was found to be a considerable diminution of the potash. One oz. of fresh beef gave 0.599 grains of potash, one oz. of salted beef 0.394 grains; and there can be little doubt but that a prolonged action of the brine would reduce very greatly the amount of potash salts in the largest joints. So that the sailors' weekly dietary, when no vegetables can be procured, consisting of $9\frac{3}{4}$ lbs. of salted meat, about 7 lbs. of flour in the form of biscuits, and $1\frac{1}{2}$ pints of peas, would contain about 90 grains of potash, supposing the meat such as stated above.

(2.) That all bodies proved to be anti-scorbutic contain a large

amount of Potash.—All fruits contain this substance in abundance, as oranges, lemon, limes, grapes, gooseberries, &c., and these are all highly anti-scorbutic. Potatoes also, which perhaps are the most valuable as an addition to a dietary for the purpose of preventing scurvy, and owing to the scarcity of which article this disease has been so prevalent within the last two years, contain, as the above analyses prove, a very large amount of potash, and when boiled (not too much, and unpeeled), still retain most of that ingredient; this also accords with the fact, that potatoes, when cooked in the ordinary way, are anti-scorbutic, and at the same time explains why the hard core of that tuber, which is so much liked by the Irishman, is most powerful in preventing the occurrence of scurvy, (see Dr Lonsdale in August number of this Journal). Milk, which is undoubtedly a good anti-scorbutic, and upon which the young of animals are for some time sustained, contains a very large proportion of potash salts compared with those of soda, being an exception to the relation between these two classes of salts which is found in the other animal fluids,—a pint of milk (London) and having a sp. gr. of 1·021, containing 6·180 grains. This was probably considerably diluted, as the usual sp. gr. is from 1·026 to 1·030. Berzelius's analysis gives about 9 grains; but the sp. gr. of the milk which he analyzed was much greater, about one-third. Fresh meat also contains potash in rather large proportion; and there is no doubt that animals, such as the carnivora, living entirely on this substance in its uncooked state, take an amount of potash quite sufficient for the wants of the system. When we examine other articles noted for preventing or curing the disease in question, we find that potash enters into the composition of all in considerable quantities; this is true with regard to cabbages, turnips, onions, garlies, leeks, and hence their efficacy, and also of pickles and sour-kROUT made from them; the same is the case with the young tops of plants, as of the *Pinus sylvestris*, &c., when a decoction is made. Potash is also found in spruce beer, wort, malt liquors, wines, especially the lighter description, which contain this substance in the form of a bitartrate, but which becomes deposited in the stronger varieties.

(3.) *In scurvy the blood is deficient in potash, and the amount of that substance thrown out by the kidneys is less than what takes place in health.*—100 grains of the dried serum of healthy blood, when incinerated and heated with the bichloride of platinum, gave 1·582 grains of the double chloride of potassium and platinum. 100 grains of dried serum of scorbutic blood, treated in the same way, gave only 0·627 grains of the same salt; so that the amount of potash in scorbutic blood was little more than one-third that contained in the blood in health, although the total amount of saline matters was nearly equal. In one case, a female, at 50, complaining of great prostration of strength, spongy gums, effusion in both ankles, &c., the amount of urine passed was $22\frac{1}{2}$ fluid ounces in the twenty-four hours—sp. gr. 1·015. Reaction *very acid*, and on standing deposited mucus

intermixed with uric acid crystals. The amount of potash excreted in twenty-four hours was much less than in health, being less than 7 grains; but a slight accident prevented a very accurate determination of the quantity.

In another case, the amount thrown out in twenty-four hours was 40 oz. Sp. gr. 1.010, and acid in its reaction.

(4.) *Scorbutic patients, when kept under a diet which gave rise to the disease, recover, when a few grains of Potash are added to their food.*—In several cases which came under my care, the treatment consisted in the daily administration of a few grains (from 12 to 20) of some salt of potash mixed with syrup and water. Sometimes the bitartrate, at other times the acetate, and also the carbonate and phosphate were used. All the salts appeared to act alike, and I have little doubt but the chloride of potassium would be found equally efficacious. When the cases were thus treated, all vegetables, milk, and malt liquors were strictly prohibited; and yet the patients rapidly recovered. Other cases were treated by fresh vegetables and milk; these also recovered, but certainly not more quickly than those from whom these substances were withheld, and potash salts substituted. On looking over the works of several writers on scurvy, I have frequently found that some *Potash* salt has been administered with *marked* benefit; thus, nitre has been recommended, nitre dissolved in vinegar, the bitartrate of potash, the oxalate of potassa; but the efficacy has always been ascribed to the acid contained in these substances, and no attention has been paid to the base.

(5.) *The theory which ascribes the cause of scurvy to a deficiency of Potash in the system, is capable of explaining some of its symptoms.*—Both soda and potash are constant constituents of the animal body, and it appears that they are not capable of replacing each other; for example, we always find the potash to exist in large quantities in the ash of muscle, soda in very small quantities (Berzelius, Liebig); in the ash of the blood we find the relation reversed. It appears also, that the muscular system requires the presence of potash, and we should therefore expect to find that where there is a deficient supply of this base, the effect would soon be manifested in the functions of that system. This we find to be the case in scurvy; without any amount of wasting of the body we find marked muscular debility, and this perhaps is one of the earliest symptoms of the disease.

Conclusion.—I have ventured to make public this theory of the cause and nature of scurvy sooner than I otherwise wished, both on account of the difficulty of procuring cases of this disease at the present time, and from the conviction that its being made known to the profession at large would be the most ready mode of having it confirmed or disproved. If true, it will be seen at once that its applications will be of the utmost importance, and the occurrence of scurvy, both at sea and on land, can be most readily prevented, by the introduction of a few grains of some potash salt, as the phosphate, chloride, tartrate, &c., into the food, or by these being taken in a separate

state. At sea, its applications would be invaluable, from the cheapness, stability, and the small space occupied by the remedy, when compared with lime juice; from its being able at all times to be procured from the ashes of wood or plants, especially tobacco, which contains it in abundance. If found to be a mere hypothesis, I have this apology to make, that in my mind it accounts better for the occurrence of this disease than any other yet offered; and it will still be an interesting fact, that *Potash* always accompanies the *real* antiscorbutic principle, was found deficient in scorbutic blood, and that several cases of scurvy rapidly recovered under the use of some of its salts, without the administration of any other remedy, dietetical or medicinal.

ARTICLE II.—*Reports of Cases treated at the Glasgow Eye Infirmary.* By ANDREW ANDERSON, M.D., Lecturer on the Practice of Medicine in Anderson's University.—No. II.

My last report contained an account of the post-febrile ophthalmitis observed in Glasgow some years ago. I now venture to record a few cases of more various character.

I.—*Successful Extirpation of an Enlarged Lachrymal Gland.*

David Gibson, aged nineteen, a healthy country lad, presented himself on the 29th of November 1843, with protrusion of the right eyeball. This had been first noticed four months previously; and the only cause to which he could refer its occurrence, was his having received a scratch with a nail on the lower sinus of the conjunctiva two months before.

On his admission I found the eyeball forced downwards and outwards, so that the upper edge of the right cornea had come to be on a level with the lower edge of the left. The lower lid was everted, and the globe of the eye projected somewhat over the malar bone.

This displacement was evidently produced by a tumour, the very hard nodulated edge of which could be felt protruding the outer part of the upper lid, just under the edge of the orbit, and could scarcely be moved, in consequence apparently of adhesion to the bone. I doubted at first whether it were not an exostosis, so hard and firm did it feel.

It seemed unconnected with the eyeball, which enjoyed its normal motions, and there was no protrusion under the conjunctiva. That membrane was slightly inflamed; but the skin covering the tumour was healthy. No uneasiness had ever been felt in the part, which could be handled without pain, and, notwithstanding the displacement of the eye, the patient could with it read letters an inch long.

On the 7th of December, with Dr Mackenzie's sanction and assistance, the tumour was removed.

An incision was made through the skin, orbicularis muscle, and fibrous layer of the upper eyelid, close under the eyebrow, and parallel to it, from its outer nearly to its inner angle; the levator muscle, tarsus, and conjunctiva being then depressed, the edge of the tumour was exposed.

The mass was with some trouble detached from the lachrymal fossa and roof of the orbit, to which it adhered with considerable tenacity; and having been then grasped with a pair of hooked forceps, was drawn forwards, so that by a little careful dissection I took it quite away.

Very little blood was lost, and the wound was closed with three stitches. The tumour was firm, oval, and slightly nodular, about as large as a chestnut, and closely invested by fibrous membrane. It had a uniform granular structure, without any bands or septa, and the central parts were the firmest. Under the microscope it appeared to consist of a fine fibrous tissue.

The eye retired into the orbit at once when the tumour was removed.

Considerable swelling of the lids ensued, and the lower sinus of the conjunctiva protruded much, having become infiltrated with lymph, so that I was subsequently obliged to cut it off.

The incision healed by the first intention, except at one point, from which pus was discharged in considerable quantity for about ten days; after that time the swelling had subsided. The eyeball gradually regained its natural place, and by the end of the month the patient was dismissed, seeing perfectly to read small type, and enjoying all the natural motions of his eye. The cornea, which had been for a fortnight after the operation rather dull, had now regained its natural moisture and polish.

Interesting cases of tumour of the lachrymal gland will be found in Dr Mackenzie's work, and in the late numbers of the Dublin Quarterly Journal of Medical Science; in Mr Lawrence's Treatise there are also several recorded, one in particular which much resembles mine.

In *this* the favourable circumstances were, that the tumour did not stretch among the muscles of the eye, and was not very large. Hence it was not necessary to disunite the lids at the outer canthus before removing it, nor to risk destruction of the levator palpebræ. To make, as some have recommended, a vertical incision on the brow, seems to me useless at the least; for the bony edge of the orbit must needs always bound the space we cut in, and may in all cases be laid bare by the incision which I practised.

It is obvious that the sooner such an operation is undertaken, the better for the patient's ultimate power of sight; and the result of the present case fulfilled my most sanguine hopes. The optic nerve will bear much stretching without irremediable injury; although, after displacement such as this, some time must elapse before its functions can be perfectly regained.

II.—*Hypertrophy of the lower portion of the Lachrymal Gland.*

John Whitelaw, a healthy boy of eight years old, appeared at the Infirmary on the 24th of May 1845, each eye presenting an appearance which I had never before observed.

An elongated granular mass, evidently formed by the hypertrophied lobules of the lower portion of the lachrymal gland, encroached on the outer half of the upper sinus of each conjunctiva, and caused, when the eyes were shut, a slight external fulness. The affection seemed to have existed for a considerable time, but no more minute history could be obtained.

By repeated leeching, and inunction of blue ointment into the skin, the swellings were in a month's time reduced to one-third of their original bulk. The boy then ceased to attend.

I have not been able to find recorded any case precisely resembling this; but have little doubt of its being of the nature I have assumed for it.

It was the appearance of the swellings, rather than any uncomfortable feeling, which seemed to create uneasiness in the mind of the mother of the boy: and, had the mercurial frictions proved useless, an operation for the extirpation of the tumours would, in my opinion, have been quite unwarrantable.

III.—*Intermittent Strabismus.*

Agnes M'Gilvray, a rather delicate-looking child of five years old, was brought to the consultation on the 21st of July 1845, squinting inwards most decidedly with both eyes. Her mother stated that she had done so for about two years, during which she had once or twice had symptoms supposed to indicate irritation within the head; but that the most curious part of the malady was its being regularly intermittent, for she squinted only on alternate days; or if by chance the convergence should last, as it sometimes did, for two days, there followed other two during which the squint was absent.

I scarcely believed the story at first; but soon had ample proof of the correctness of the mother's statement. Next day the strabismus was gone, and on the following day it had returned; and the affection seemed to undergo no change for the space of two years, during which the child continued to present herself at intervals.

I had recourse to various treatment, but in vain; quinine, iodine, and other remedies, had no effect; on the 2d of June 1847, she was reported as still squinting on alternate days.

This is a remarkable, and, as far as I know, a solitary case. The records of periodical diseases are however so curious, that nothing of this kind need much surprise us. I am not inclined to lay much weight on the presumed head symptoms as indicative of organic cerebral disease of any kind. I think it much more likely that the disorder is purely functional, and hope for its disappearance as the girl grows older.

IV.—*Subcutaneous Osseo-calcareous Tumour of the Eyelid.*

Charles Forbes, aged twenty-six, presented himself on the 5th of July 1844. Eighteen months before, he for the first time observed, under the skin of the left lower eyelid, towards the inner canthus, a small hard tumour about the size of a large pin's head. It was at that time perceived only when he stooped; but it grew rapidly, and in the course of a month had acquired its present size, about that of a field-bean, and had become permanently visible.

I found it lodged in the cellular tissue of the eyelid, being sunk between the eyeball and the edge of the orbit, from which place, when the eye was pressed upon, it started forwards under the skin, below the inferior lachrymal canalicule.

The concretion, as it seemed to be, caused no pain; but its pressure on the eye produced a haziness, of which the patient complained as being diffused over the upper part of his field of view.

I extracted the mass through a small incision in the skin; and found it to consist of concentric osseo-calcareous layers, enclosed in a cartilaginous shell.

As the wound healed, vision went on improving.

I am at a loss to account for the origin of this body. I at first suspected that it might be a lachrymal calculus; but its position and its structure, as discovered after it had been removed, forbade the idea.

V.—*Examples of the Operation for Artificial Pupil, with Maunoir's Scissors.*

1. Samuel Espie, a healthy young man of twenty-one, was admitted to the Eye Infirmary, April 30th, 1845. His left eye had been blown out by small shot sixteen weeks before, and the right cornea had at the same time been wounded. In consequence of this injury, the pupil appeared displaced upwards, so as to be close to the edge of the cornea, while it was much elongated transversely. The mere slit which it thus formed was filled up with lymph, and vision was reduced to mere perception of the presence of light.

The eye being quite free from irritation, I proceeded, three days after, to the operation, performing it with Maunoir's scissors in the usual way. The cut in the iris gaped widely, and there was but little bleeding. The eye did not inflame, the wound healed kindly, and the oval, pointed pupil remained clear.

On the 14th of May, Espie could distinguish a pen; on the 21st, he could read letters an inch long: and in a year after the operation he could, with two and a half inch cataract glasses, read a large printed bible without much difficulty. Subsequently, I regret to say, he took fever, since which his vision has not been so good.

2. Marion Martin, aged twenty-seven, a healthy young woman from the country, presented herself May 28th, 1845. The left eye had, in consequence of an injury seventeen years before, become atrophic, and with the right she could barely count the fingers held between her and the light, that eye having, in the following year, suffered from inflammation. Its pupil was, on her admission, found to have contracted round a patch of opaque capsule, to which its edge adhered so firmly that belladonna could not move it in the least.

As I had but little hope that I should be able to tear up this adhesion with a cataract needle, I attempted to destroy it with Maunoir's scissors, introduced as in Espie's case, but used not, as then, to incise the lower fibres of the iris, but rather to cut the opacity out of the pupil. In this I succeeded, leaving an aperture pretty clear, and nearly circular.

This was done on the 1st of June, and considerable inflammation followed, obliging me to take a little blood from the arm, and leech the temple. The lens, at the same time, became opaque, and filled up the pupil.

Early in July I introduced a common cataract needle through the sclerotic, and broke up this cataractous lens; but the eye again inflamed in consequence, and the pupil once more closed around some shreds of capsule.

On the 7th of August I operated once again with Maunoir's scissors, and then just as in Espie's case. A good quantity of opaque softish lenticular substance escaped through the incision, and the pupil, as in his case, continued clear and open. There once more ensued considerable inflammation, for which I judged it necessary to bleed again, and gently to mercurialize my patient. On the 20th of August the eye was quiet, and the pupil clear.

It was interesting to trace the gradual return of vision after so many years of blindness, although the presence of the inflammation complicated of necessity in some degree the result. For a few days after the final operation the patient could perceive nothing but much light of various colours; in a fortnight after the operation—that is, when the inflammation had subsided—she could see objects, but not distinguish them; in two days after this she could tell one finger from another; and she was sent home on the 10th of September, able to read printed letters an inch long.

Six months elapsed before I heard of her again: she could then, with the aid of her two and a half inch cataract glasses, read small type. She complained, however, of severe frontal pain, which seemed to be weakening her sight; and I recommended daily friction of the temple with a mixture of equal parts of extract of belladonna and mercurial ointment. My friend, Dr Brown of Langholm, kindly saw this prescription carried into effect; and in March last (1847) he informed me, that to it he attributed her immunity from pain, and the good vision she then enjoyed. When I last heard of her, she

could, without glasses, go about all country work ; and, when using them, read easily small type.

3. Mary Browning, aged thirty-two, was operated upon by me for cataract, July 11th, 1845. I then broke up the right lens, which I found, as I expected from its appearance, very soft. The operation was followed by inflammation, on the subsidence of which the pupil was seen to have contracted firmly around opaque shreds of capsule. My patient could with this eye see nothing, and the cataract in the other eye had been, from the time of her admission, so much advanced, that with it she scarcely saw at all.

In the middle of the next September I attempted to open up the pupil with a needle, passed through the sclerotic ; but failed, on account of the toughness of the ring of lymph which bound the iris to the remains of the capsule.

Seeing no prospect of my being able to drill the pupil open, as some have in such cases recommended, I made a new pupil with Maunoir's scissors, precisely as in Marion Martin's eye.

This was on the 1st of October. The eye did not inflame ; in ten days my patient could read the title-page of a book through two and a half inch cataract glasses, and in a fortnight after the operation, I dismissed her, able to read small type with ease.

4. Janet Thomson, aged fifty-eight, had her right eye operated on by me, for the extraction of a lenticular cataract, on the 21st of June 1847. The lens having sunk somewhat in the eye after the cornea had been cut, and the vitreous body having in small part escaped, I merely divided the cataract as much as possible, with a needle introduced through the incision, and so allowed the parts to heal. No inflammation supervened, but the pupil closed around some fragments of the capsule.

On the 29th of July, the eye being quiet, I made an artificial pupil, as in the last recounted case.

The result was equally pleasing. On the 19th of August, nothing unpleasant having taken place, I dismissed the woman, able to read small type with ease through her two and a half inch glasses.

I quote these cases from the Journals, not for any novelty which they present, but as illustrations of the ease and advantage with which, in this way, and in such cases, an artificial pupil may be formed. It is an operation, of course, unsuitable to many a case ; but where it is applicable, the facility and certainty with which it may be executed, and the comparatively small chance that inflammation shall follow, recommend it to approval, warmer than some have given it. I am surprised that it should not be even mentioned in Ruete's recent work.

VI.—*Cure of Partial Xerophthalmia.*

Thomas McLusky, aged nine, was entered on the books of the Infirmary on the 23d of June 1847. The outer part of each ocular conjunctiva presented a well-marked patch, of "xeromatous" character,—dry, glistening, and of parchment-like glossiness. It did not encroach upon the cornea, but covered a portion of the sclerotic about as large as the nail of the little finger. There was slight catarrhal conjunctivitis, but very little uneasy feeling. No history could be obtained of the time or manner of the first appearance of the malady.

The corrosive sublimate collyrium, the red precipitate ointment, and drop of diluted vinum opii, were daily used for a week or two, but without the slightest benefit. I then bethought me that perhaps an alkali might soften the dry epithelium, and alleviate this commonly reckoned incurable disease. A drop of

aqua potassæ was therefore introduced into each eye ; and in two days, when the inflammation it produced had disappeared, the application was repeated, and continued thus at intervals for some weeks.

The result was very gratifying : the dryness gradually lessened, the conjunctiva becoming more moist and soft. On the 27th of July the cure of the right eye was completed ; and when the boy gave up attending, some weeks thereafter, that of the left was all but finished.

Encouraged by this result, I tried the same remedy in a bad case of corneal xeroma, but, I regret to say, without effect.

ARTICLE III.—*Further particulars of a Case of Intestinal Concretions, formerly Reported ; with Remarks.* By ROBERT TURNER, M.D., Keith.

IN *The Monthly Journal* for September 1841, the case is recorded of Alexander Gordon, a small farmer on the estate of Cairnfield, in this county, who passed fourteen intestinal concretions, and had completely regained his former health and strength in May of that year, when the report was taken. The same individual has since passed other eighteen of these bodies ; and his case being thus, it is believed, without a parallel in the history of this affection, the following brief detail of the circumstances of his second attack may be interesting to the profession.

The patient's straitened circumstances not admitting of the sweeping alimentary reform recommended to him, or, indeed, of any material change in this respect, his dietary became, at an early stage of his convalescence, in every particular the same as before—*two-thirds*, at least, of the solid ingesta consisting of *oatmeal*.¹ A tolerable measure of health was notwithstanding attained, and he continued in the uninterrupted enjoyment of it till towards the end of 1843, when a train of symptoms occurred identical with those of his previous illness. Impaired appetite, disturbed sleep, with gradual loss of flesh and strength, were those first complained of. The bowels again became sluggish, and, although recourse was frequently had to castor oil and other laxatives, a stool could seldom be procured oftener than once or twice a-week. After a time, this state began to alternate with, and eventually it altogether gave place to, diarrhœa of a watery character, sparingly mixed with feculent matter, as in his first attack. Tormina and meteorism, arising from an hour and a half to two hours after meals, and partially relieved by eructations and vomiting, recurred with progressive severity ; and the abdominal tumour again became perceptible, occupying its former situation, inducing the same feeling of tension across the umbilical and hypogastric regions, and rapidly increasing in size. From November of 1845, till about the beginning of September 1846, he was constantly

¹ In the abstract of my former communication to the *Journal* regarding this case, which is given in Vogel's *Pathological Anatomy* (Dr Day's translation), p. 379, *pulse* is erroneously stated to have formed part of Gordon's diet.

confined to bed, with marked aggravation of all his symptoms, and emaciated to an extreme degree. Early in September of last year, he began to pass the concretions. The entire mass was felt, as formerly, to descend, all at once, from the situation it had occupied since it was first detected to the anus, occasioning an insupportable sensation of weight, with almost incessant straining; and all the foreign bodies (eighteen in number) were discharged in three days. Three of these, passed on the first day, are described as being about as large as hens' eggs, and the remaining fifteen, voided on the two following days, are said to have varied from the size of partridge's eggs to that of filberts. Immediate relief from suffering of no ordinary intensity followed their discharge.

The man's recovery has been complete. I saw him on the 17th of April last, in robust health, and fully equal to his laborious "hand-darg." On this occasion, he walked a distance of some miles at the conclusion of his day's work, to meet Mr Grant, surgeon, of this place, and myself.

From causes which it is unnecessary to mention, remedial measures had been adopted only at an advanced period of this attack. They consisted in the daily exhibition of an emollient enema, and the administration of opium; to which last appliance the patient awards the credit of bringing about his second recovery also, and, seemingly, not without reason, as the descent of the concretions was again observed to coincide with the establishment of the free physiological action of this drug. In what form or dose it was taken, I have not been able to ascertain, not having had an opportunity of seeing the patient during his recent illness. The foregoing account is, in substance, that given me by himself, confirmed in its objective details by his relatives, as well as by his clergyman, the Rev. Mr Wilson of Enzie Chapel; and, although thus necessarily imperfect, I believe its accuracy may be relied on.

Specimens of the concretions were preserved for my inspection. They closely resemble, in outward appearance and in internal structure, as seen under the microscope, those of 1841, so carefully analysed by Dr Douglas Maclagan, and described by him in his excellent paper, "On the Constitution of Intestinal Concretions."¹ In those of latest formation, the identity of the fibrous matter, of which they chiefly consist, with the hairs of the caryopsis, and fragments of the envelopes of the oat, is as obvious as in the others. Like them, too, they are seen, on section, to be studded with minute crystals, are smooth, and of greatest density at the surface, and their arrangement in concentric laminae around a calcareous nucleus is the same. In some instances, these layers have been so loosely coherent as to have separated on the body being broken into. A specimen in my possession, one of the largest, and weighing 520 grains, is marked externally by ten cup-like depressions, evidently formed by the apposition

¹ Monthly Journal for September 1841.

of as many other concretions. Some of the smaller ones also present inequalities on their surface, doubtless from the same cause.

To the production of this singular form of pathological epigenesis in man—the *fungus bezoard* of Fourcroy and Vaquelin, but more appropriately designated, by Dr Maclagan, the *fibrous concretion*—four conditions appear requisite:—1. Torpor of the large intestine. 2. The presence, in the diet, of oatmeal in considerable quantity: or perhaps occasionally—as in the example related by Braconnot and Champion, as well as in that recorded by Denis¹—of some other article of food yielding vegetable fibre. 3. The presence, in the intestine, of a nucleus of greater density than the body of the concretion, around which the latter is deposited in concentric layers. This nucleus may consist of a biliary calculus (Dr T. Thomson), or of a fruit-stone, or seed (Clark, White, Hay, Thomson, Children), a portion of bone (Coe, Hooke, Thomson, Laugier), a nail (Haller), or other extraneous substance; or, as in the case of Gordon, it may be composed of “a thin shell of phosphate of lime,² the probable source of which will be afterwards considered.—4. Constriction of the tube below the seat of atony.

The weakened propulsive power of the colon, or loss of the healthy consentaneous action of the muscular fibres in some part of it, was a prominent condition in Gordon's case, and had, in both attacks, constituted the first of the series of diseased actions, preceding the formation of the concretions, it may be presumed, many months. The same state has been noticed as occurring early, in various other examples of this affection; and it seems probable that, had clinical observation of *the disease* been prosecuted with that assiduity which has been brought to the task of determining the chemical composition of *its product*, impaired contractility of the large intestine would have been generally recognised as the first appreciable link in the chain of morbid sequences. This condition persisting, and the contact of the excrementitious portion of the alvine contents with the mucous surface, above the inactive part of the tube, being thereby unduly prolonged, irritation here would ensue, signified, as in the case under consideration, by tormina and muco-serous diarrhoea. A state of matters would then exist resembling that which has been sometimes observed as the result of ordinary constipation; “that singular condition of the bowels,” to employ the language of Dr Abercrombie, “in which fluid feces may be discharged regularly and freely, and apparently in abundant quantity, while there is going on, for a length of time, an immense accumulation of feculent

¹ Annales de Chimie et de Physique, tom. xx.; and Archives Générales de Médecine, tom. xvii., quoted by Dr Maclagan.

² Not the *carbonate*, as stated by Vogel (loc. cit.) Vide Dr Maclagan's essay in the Monthly Journal, already referred to.

matter, in a very hardened state, extending through the whole of the colon."¹

To this stage I would refer the production of the calcareous nucleus. "The liquid evacuations of persons suffering from diarrhoea," says Vogel,² "almost invariably contain precipitates, consisting of ammoniaco-magnesian phosphate, and phosphate of lime." "The formation of many intestinal calculi," he elsewhere remarks,³ "is due to an exudation of fibrin, or a coagulum of blood retained in the intestinal canal, and undergoing further changes, the constituents soluble in the intestinal fluid being gradually removed, and merely the insoluble portion—the calcareous salts—remaining. Such calculi consist, for the most part, of protein compounds, coagulated fibrin, mixed with salts of lime and fragments of food; they are formed after inflammatory exudation of the intestinal mucous membrane, and after hemorrhage into the canal. To this class belong the concretions which were analysed by Dublanc. They were discharged by a child after inflammation of the bowels, and consisted of fibrin, with a trace of fat, and phosphate of lime. A second kind of intestinal concretion consists, principally, of earthy salts (phosphate and carbonate of lime, ammoniaco-magnesian phosphate, and phosphate of magnesia)."

I infer, then, that the nucleus of phosphate of lime, in the case of Gordon, was derived from the exudation of irritated mucous membrane, and produced in the manner described in the above extract. It is true that the salt is contained in oatmeal; but the supposition that any portion of this "shell" was supplied by that article of the diet, is altogether irreconcilable with the circumstance of the deposit being unmixed with the fibrous part of the oat, and occupying the centre of the concretion; and the fact, that the "thin shell of phosphate of lime" enclosed "some animal matter resembling dried blood, also points unequivocally to exudation into the canal, consequent upon irritation of its lining membrane, as the source of this central portion of the concretion. I am further inclined to regard the proportion of the phosphate, found by Dr Maclagan in the body of the concretion—20 parts in 100—as too large to have been supplied wholly by the ingesta, although unquestionably derived from them in part; for a specimen of ordinary oatmeal examined by him, afforded in 100 grains only gr. 0·6 of phosphates. When it is considered, too, that the amount of fibrous matter in the concretion did not exceed 36 *per cent*, the disproportion between the actual quantity of phosphates it contained, and that which could with any degree of probability be supposed to have been supplied by the food, becomes still more evident. But it is easy to conceive that the fibrous matter, while being deposited, would, by reason of its porous structure, ab-

¹ On Diseases of the Stomach, and other Abdominal Viscera, Second Edition, p. 341.

² Op. Citat., p. 377.

Ibid. p. 376.

sorb a part of the diarrhœal discharge in which it was bathed, and retain in its interstices the crystals of the earthy salts which it had imparted a disposition to the fluid to precipitate. Its pores thus, in time, becoming filled, it would no longer imbibe the liquid coming in contact with it, but would still attract to its surface the crystalline deposit, until coated over with this, which the mucus from the discharge would render adhesive; and we have here, probably, the *rationale* of the greater density of the surface than that of the substance of the concretion; a condition uniformly existing in the case of Gordon, and due—so far as microscopic examination enables me to judge—to the larger proportion of calcareous matter in the former situation. In this way we may also account for the presence of the “external crust of phosphate of lime, mixed with animal matter,” observed in some of the concretions in Professor Monro’s collection. “On the surface of a few,” adds Dr T. Thomson, who analysed these specimens, “were noticed crystals of the phosphate of ammonia and magnesia;” the salts just enumerated being precisely those mentioned by Vogel as of “almost invariable” occurrence in liquid alvine discharges.

A nucleus once supplied, the deposition of the fibrous laminæ appears to proceed rapidly; and, in retaining these accumulations, the influence of the loss of propulsive power in the large intestine is reinforced by the supervention of another condition—constriction of the tube below the seat of impeded or suspended peristaltic action;—a state which also occurs as a distinct disease, depending on intestinal irritation, and has been described by authors under the name of spasmodic stricture of the rectum. Reasons for assuming its agency in Gordon’s first attack are offered in my former paper, and the same grounds exist for concluding that this spastic closure of the tube was present also in his recent illness. In both, the situation occupied by the mass of concretions, before its descent to the anus, seems to indicate the lower part of the sigmoid flexure of the colon, or upper extremity of the rectum, as the limit of the constriction superiorly; and the result, each time, shows that this condition of the intestine may exist long without inducing structural change, and still amenable to treatment. It may, in fact, be doubted whether a really permanent constriction of the gut is so constant an anatomical character of the affection, leading to the formation of the fibrous concretion, as is generally believed. It is possible that observers have here, as in analogous circumstances, mistaken a normal for a pathological condition,—a source of fallacy pointed out by Dr Abercrombie in his remarks on the post-mortem appearances of ileus:—“In a fatal case,” he observes, “we generally find one part of the intestine in a state of distension, and another part empty and collapsed—presenting nearly the form of a cord; and there has been supposed to be a difficulty in determining which of these is the primary seat of the disease but the collapsed state, in which it assumes the

*form of a cord, appears to be the natural state of healthy intestine when it is empty."*¹

The combined action on the growing mass of concretions, of the *vis a tergo*, exerted by the preternaturally excited intestine above, and the resistance *a fronte* at the constricted part, would gradually dilate the intermediate portion of the tube; but this dilatation, owing to the comparatively unyielding nature of the perpendicular muscular bands of the intestine, would not be uniform; and the result would be an exaggeration, simply, of the natural sacculated appearance of the colon,—sufficient in degree, however, to constitute the “pouches,” lodging the concretions, described by Monro—the “diverticula or coarctations” of Vogel. The compression sustained by the mass, before the individual concretions had attained their greatest hardness, would occasion the inequalities of their surface above described; and the whole would at length become firmly impacted in the dilated portion of the bowel, down to the seat of stricture, presenting to external examination the appearance of an even and scarcely moveable tumour. Meanwhile the liquid excrementitious matter straining through the interstices of the fibrous substance of the several concretions, while yet pervious, and the interspaces of the aggregated mass, would find an outlet at the constricted part, although this would retain all solid matters.

At this advanced stage of the disease, digestion and absorption of the chyle could take place but very imperfectly; indeed, before relief was obtained on each occasion, in the case on which these remarks are founded, assimilation appears to have been reduced as nearly as possible to the minimum amount compatible with the maintenance of the powers of life. The immense degree of distension which the intestine must have suffered, where it surrounded the concretions, was manifested by the bulk and rounded form of the tumour; and the severe and oft-recurring tormina, eructation, and vomiting, bore evidence of the great and prolonged efforts of the irritated tube to expel the foreign bodies, as well as the powerful nature of the obstacle opposed to their expulsion.

The imperfect state of our knowledge, as applied to the *diagnosis* of this affection, was adverted to in my former communication on the subject; a practical illustration in point is supplied in the following statement:—“Upon the very bold operation of cutting out these concretions, when lodged in the colon, proposed by Dr Monro, senior, we think it our duty to state, that the diagnosis is so difficult, that in one case, where the operation was strongly advised, it turned out, upon dissection, that the disease was a *scirrhus pylorus*.”²

It is not, perhaps, exceeding the truth to assert, that received opinions, as to the means of distinguishing this affection, are as much the fruit of mere conjecture as of observation at the bed-side, for

¹ Op. citat. p. 145.

² Edinburgh Medical and Surgical Journal, No. 33, p. 112.

which, indeed, but few opportunities appear to have been enjoyed. That little or no progress has been made in this direction within the present century, is rendered evident by the fact, that one of the latest systematic writers¹ on the disease, confines himself to laying down a series of propositions, embodying the substance of Professor Monro's observations, published in 1811, and refers, for fuller information, to the work itself.

The case of Gordon, although it cannot be received as a certain basis for any general conclusion, tends to throw doubt upon the correctness of the last-named authority's position, that "when two or more concretions are lodged within the intestines, they may be made to strike against each other." It might, besides, be urged against the diagnostic value of this sign, even if its occurrence were certain, that in some cases of ordinary colic, the abdominal parietes undergo a peculiar state of contraction, giving them—in the language of Cullen—"the appearance of a bag of round balls." It must have occurred to every practitioner, to meet with cases, particularly in emaciated subjects, in which common scybala presented to external examination the semblance of distinct globular masses, capable of being pressed against each other, and of great apparent hardness. In a recent example, occurring in my practice, a state of irregular contraction of the large intestine exactly simulated the presence within it of solid spherical bodies. The case was one of scirrhus disease of the rectum—which was so contracted about two inches above the anus as with difficulty to admit a small-sized urethral bougie,—and consequent fecal accumulation. For months before death, well-defined round swellings, usually three or four in number, and about an inch apart, were generally perceptible at a particular time of the day, between the umbilicus and left iliac region; and their resemblance to hard moveable balls was perfect. Post-mortem examination, however, disclosed nothing beyond distension of the entire colon, and upper part of the rectum, with ordinary feculent matter, of solid, but by no means hard consistence.

The appearance referred to, then, so far from countenancing, may rather be held to negative the supposition, that intestinal concretions are present, or is, at best, a deceptive indication of their existence.

But the difficulties surrounding the diagnosis do not appear to be absolutely inherent. They are probably, in a great measure, capable of being surmounted by strict inquiry into the patient's previous history as regards the habitual state of the alimentary canal, and nature of the diet, and by careful observation of the disease in all its stages. The peculiar cord-like feeling of tension across the abdomen, if not a pathognomonic symptom, I am disposed to consider as pretty certainly distinctive of the affection, when accompanying the other symptoms noted in the case under consideration, and when the abdominal tumour is at the same time perceptible. Important aid in

¹ Dr Symonds, in the *Library of Medicine*, vol. iv. p. 123.

the investigation might be obtained from exploration of the intestine, by means of Dr O'Beirne's defecation tube, or the esophagus tube of the stomach pump, which measures would also form part of the appropriate *treatment*. To secure the full action of the opium in producing muscular relaxation, it appears necessary to administer it (or its alkaloid, morphia) in large doses, repeated at intervals of half an hour, till a state verging on narcotism is induced. If the constriction were thus overcome, and the presence of the foreign bodies ascertained by means of the tube, the colon might next be distended with tepid water, or a stimulating injection, and the expulsion of the concretions be thereby promoted. The action of the narcotic might, if requisite, be aided by the exhibition of belladonna as an enema, ten grains of the extract dissolved in as many ounces of water,—in which form and dose I have twice employed it (once successfully) in ileus. The condition of the patient, in all cases of intestinal concretions, would probably render the use of tobacco, and other powerfully depressing sedatives, inadmissible.

This second deliverance of the patient Gordon without operative procedure, affords the same argument as the first, and doubly cogent by repetition, against the adoption of the proposal of Monro, Secundus; and the practice of Richerand and Maréchal,¹ of dividing the sphincter ani for the removal of the concretions, when they have descended to the lower part of the rectum, seems equally unnecessary; since these bodies, if of the fibrous variety—which is that most likely to occasion difficulty on account of size—could be broken down, in this situation, with ease and safety, and brought away piecemeal should their extraction entire be impracticable.

The efficacy of *preventive* measures in this affection, does not require to be dwelt upon. The subject of the foregoing report appears now to be fully impressed with their importance. His diet, since his last illness, has been chiefly animal; and he has hitherto avoided, and undertakes to eschew religiously, in all time coming, his ancient enemy the oatmeal. Although, as already mentioned, his general health was excellent when I last saw him, an inactive condition of the bowels remained, for which I prescribed an occasional dose of the *compound aloes pill*, and the regular use of the infusion of *Chiretta*, a recent addition to our *Materia Medica* which seems to deserve its Indian reputation as a tonic, specially serviceable in costive habits.

ARTICLE IV.—*Case of Extensive Scrofulous Ulceration, with Abscess bursting into the Trachea.* By JAMES FRAZER, Surgeon, Glasgow.

(Read before the Glasgow Medical Society, 19th Oct. 1847).

ABOUT eighteen months ago D. M., an engineer, æt. thirty-six, of temperate habits, married, and the father of six apparently healthy children, consulted

¹ Cooper's Surgical Dictionary, sixth edition, p. 21.

me for enlargement of the lymphatic glands of his neck. All the marks of a scrofulous habit were strikingly developed ; he stated, however, and the remark was corroborated by his friends, that up till that period he had enjoyed excellent health, and that in particular he had been free from any strumous affection, and was ignorant of hereditary taint. All at once, about two months previously, his attention was directed to the simultaneous enlargement of several of the glands. When he first called on me they had attained considerable size, and presented the usual appearance. His general health was little, if at all affected; he had no cough.

The ordinary remedies were applied, but the disease made rapid progress. Open ulceration followed ; the ulcers healed slowly, leaving unsightly cicatrices. These were succeeded by other tumours, which ran a similar course; till, at the end of a few months, in defiance alike of regimen and medicine, the greater part of the anterior and lateral aspects of the neck became one mass of disease. About this time I lost sight of him, but afterwards learned from his wife that, anxious to try the effect of change of air, he had adopted the only available means within his reach of doing so, by engaging himself to work as a jobbing mechanic along the line of the North British Railway ; that he was unavoidably much exposed to cold and wet, and, as a consequence, had become much worse. He continued nevertheless, impelled by pressing necessity, to work till about seven weeks ago, when, unable to hold out any longer, he returned to Glasgow, and called upon me. I then made the following report. Tongue clean ; appetite good ; bowels regular ; pulse 90, small, weak, and readily excited ; complains much of debility ; sleeps well ; has had a slight cough for two weeks ; no dyspnœa ; the sound on percussion under the left clavicle is somewhat dull ; the other portions of the lungs seem to me to be free from disease. The neck now presents the most hideous appearance, the skin of all the lateral and greater part of its anterior aspects is destroyed, and the right external ear, below the meatus, is gone ; many of the ulcers are of great depth ; the superficial glands in every other situation of the body are healthy.

On the 20th of last month he stated, that when undressing for bed on the preceding evening, he felt something give way in his neck ; this, he said, was followed by spasmodic coughing, which, however, soon went off ; a considerable quantity of matter then discharged itself from an opening, situated about half an inch above the right clavicle, and close upon the posterior border of the sterno-cleido mastoid muscle, immediately after which he was much alarmed at air escaping from the wound. On examination of this wound, I found that when he shut his mouth, pressed his nostrils together, and attempted a forced expiration, an ichorous discharge was forced from a small sinuous opening at the bottom, after which there was a smart rush of air. In ordinary respiration the discharge alluded to issued from the opening in the form of bells, one bursting, and another occupying its place in quick succession. On introducing a probe, it passed at once, without obstruction, horizontally inwards and forwards into the trachea.

Up till Saturday last, the progress of the disease in the neck seemed stayed, and I was in hopes that granulations were about to close up the opening ; but a considerable piece of cheesy-looking matter was then displaced from its external orifice, followed by a thin purulent discharge, since which the escape of air has been uninterrupted. When seeing him yesterday, I observed that a small portion of skin immediately to the right of the trachea assumed a dark livid appearance, and was becoming rapidly destroyed ; but as this is on a parallel line with the opening already formed, and could not therefore afford a more depending opening, but increase the constitutional irritation, my object will be, as long as possible, to preserve it entire.

I have purposely avoided extending this communication by detailing the treatment employed : to have done so would but have been

to present an epitome of the various remedial measures for this wide spread and important disease. It is by no means surprising, that from the influences of causes so favourable to its development—cold, wet, and the depressing passions—the symptoms in this case lately assumed so malignant a character; and the circumstance of an abscess opening into the trachea is by no means unique. The earlier history of the case, on the other hand, seems to claim for itself an interesting character, not only from the severity of symptoms first manifesting themselves so late in life, but from the entire absence (till within these few weeks) of any thing indicative of pulmonic disease.

The various medical authors I have consulted on this subject seem to agree in the opinion, that it is rare indeed for scrofula, without its exciting causes, to exhibit itself after puberty, and that, when it does so, the lungs are almost invariably the seat of the disease.

Among other observations which followed the reading of this paper, it was predicted that this case might yet do well; the prediction has fortunately been verified, the opening referred to was filled up within a fortnight with healthy granulations. The whole diseased surface now wears a healthy aspect; while the patient's general health has improved considerably. The only local application used of late has been a weak solution of nitrate of silver, while he has been taking cod liver oil to the extent of 3 oz. daily.

ARTICLE V.—*Contributions to Pathology and Rational Medicine.*

By JOHN HUGHES BENNETT, M.D., F.R.S.E., Lecturer on Pathology and the Practice of Physic, Director of the Poly-Clinic at the Royal Dispensary, Edinburgh, &c.

No. XII.—CLINICAL AND HISTOLOGICAL RESEARCHES ON CANCEROUS AND CANCROID GROWTHS.—(*Continued.*)

OBSERVATION XX.—*Tumour of the Parotid Gland—Hemiplegia of the Face—Death.*

IN the following case the tumour was considered by all who saw it to be essentially malignant. The attention of surgeons therefore is particularly directed to it, from the circumstance that its minute structure differed widely from that of true cancerous growths.

Mrs S., aged fifty-eight, of spare but otherwise healthy frame of body, consulted me about the commencement of 1845, for an unpleasant sensation she experienced in the left eye. On examination, I found that the eyelids could not be closed, that the under one was dragged down, that there was constant epiphora, and that the mouth was slightly dragged towards the right side. Paralysis, depending upon lesion of the portio dura, was diagnosed. The symptoms

gradually increased in intensity. Towards the end of 1845, her face and features presented all the symptoms of the lesion in a very marked manner. About this time, also, a small indurated swelling was observed immediately below the left ear. This slowly increased, and towards the end of 1846, had obtained the size of a hen's egg, the motor paralysis of the face being in no way diminished, and her general health continuing good. In the spring of 1847, the tumour had considerably extended downwards and backwards, and was irregular and nodulated on the external surface. She never suffered pain, and only experienced inconvenience from the tears occasionally rolling over the left eyelid upon the cheek, the difficulty of mastication, and the facial deformity. On the 18th of August, I found her labouring under the most violent pain in the right hip and knee joints, apparently rheumatic. She had gone into the country a few miles, thinly clad, and exposed to a cold evening breeze, in an open phaeton. The pains were very intense, increased on the slightest movement. The parotid tumour was now much increased in size, presenting a nodulated prominent mass the size of the fist, extending downwards and inwards towards the throat, and its most prominent part of a deep crimson, and even purplish hue. It was, however, every where hard, non-resistant, and free from pain. The intense rheumatic pains in the two joints continued intense, and defied all kinds of treatment, internal and external, for four weeks. The confinement to bed reduced her strength considerably, and it was with the greatest difficulty bed sores on the back were prevented.—*September 16th*, she complained of slight difficulty of swallowing, which in a few days became so great, that nothing but liquid food could be taken, and even that with great difficulty.—*September 22d*, she complained of an incessant discharge of thick, glairy mucus, apparently from the throat, which caused her much trouble to get up.—*September 24th*, the strength was now greatly diminished. She could take little nourishment, or none: the pulse was feeble, and accumulation of mucus in the throat threatened suffocation. Died on the 28th at 4 A.M., preserving her intelligence nearly to the last, and without any symptom having been manifested referrible to derangement of the thoracic or abdominal organs.

Secutio Cadaveris, September 30th, 1847.—The tumour and the cervical region only were allowed to be examined. The former was the size of a man's fist, nodulated on the surface, hard to the feel, and the skin covering it of a deep red colour, but not ulcerated. It occupied the whole region at the angle of the jaw, overlapping the mastoid process posteriorly, pushing up the ear, and descending inferiorly to the middle of the neck. The skin was thin over the tumour, and so firmly adherent that it could not be dissected off. The edges of the tumour could easily be raised from the parts it covered; but its roots sunk deep into the space at the angle of the jaw, and could be traced back as far as the vertebræ. It closely embraced the bloodvessels and nerves, causing more or less atrophy of them, and the sterno-mastoid muscle was completely atrophied anteriorly in front of the tumour, and converted into fibrous tissue.

The tumour on section was of a yellowish-white colour, and of tolerably firm consistence, resembling an hypertrophied parotid gland. A glutinous substance was deposited in it, which could be scraped from the cut surface by the knife; but no cancerous juice could be squeezed out. All the lymphatic glands of the neck were much enlarged, and were similarly affected.

The trachea, esophagus, and other structures of the neck, were healthy.

Microscopic Examination.—On examining the viscous juice scraped from the section of tumour, it was seen to contain numerous corpuscles, as represented Fig. 51. They were of oval form, and great delicacy, varying in size from the 1·100dth to the 1·75th of a millimetre in their longest diameter. They generally contained one, although sometimes two granules. Here and there one of these corpuscles might be seen somewhat elongated, and a few were distinctly caudate. They were embedded in a transparent blastema, sprinkled throughout which were numerous granules. On the addition of

acetic acid, the corpuscles were contracted to one half or one third their former volume, and the cell-wall, which previously was very delicate and transparent, became thicker and more distinct. A section of the tumour with Valentin's knife, exhibited the arrangement of its fibrous matrix, consisting in some places of an open network of filamentous tissue, containing groups of the corpuscles formerly described—Fig. 53; or dense bands of the same tissue, forming loop-like terminations—Fig. 54. The addition of acetic acid did not bring into view any nuclei amongst the fibrous tissue.

Fig. 51.



Fig. 53.

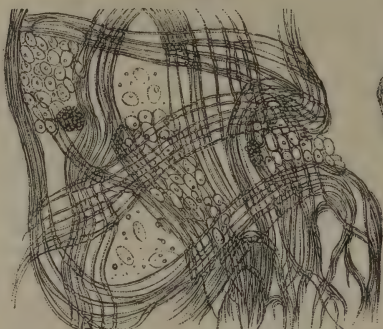


Fig. 54.



Fig. 52.

Fig. 51. Corpuscles in tumour described Obs. XX. Fig. 52. The same after the addition of acetic acid. Fig. 53. Fibrous matrix of the tumour, with loculi and groups of corpuscles. Fig. 54. Fibrous bands from another portion of the tumour.

A section through the sterno-mastoid muscle, where it passed over the tumour anteriorly, and the muscular fibres seemed lost in it, displayed inferiorly the fasciculi, terminating in a rounded abrupt manner. The transverse striæ were distinct nearly up to these terminations, when the fasciculus seemed to be composed wholly of minute molecules and granules, (not sufficiently well represented by the engraver—Fig. 55). From these rounded ends of the muscular fasciculi, fibrous tissue seemed to arise abruptly, which became identified with that of the tumour, except that for some distance they followed the course of the muscle. Between the fibres oval groups of the corpuscles might be seen here and there, together with other groups of granules, and compound granular corpuscles.

Fig. 55.



Fig. 55. Section of the sterno-mastoid muscle where it appeared atrophied, and converted into fibrous tissue. a. Muscular fasciculi. b. Fibrous tissue, containing groups of corpuscles peculiar to the tumour, with granules and compound granular corpuscles.

Remarks.—The only other case similar to the above which has ever been minutely examined, so far as I am aware, is one recorded by Sedillot. It was that of a man, aged fifty-one years, in whom a tumour of the parotid, the size of a nut, was of fifteen months' growth. It then became painful, the skin assumed a bluish livid appearance, and ulcerated. The ulceration extended rapidly, the pains became lancinating, suppuration was abundant, and he died after the disease had destroyed the parotid gland, half the ear, and the mastoid process. Portions of tissue removed from the ulcer were examined by M. Küss, who found evidently the same corpuscles as I have described, with this difference, that they were more granular. Unfortunately, he does not inform us what effect acetic acid had upon them. He further says, that the tissue in some points had a vaguely fibrous aspect, but that true fibres were rare. This probably arose from the circumstance, that nearly all the tumour had ulcerated away.

Now, it may be asked what was the nature of this tumour? Mr Syme, who was early consulted in the case, considered it to be malignant, and beyond the reach of art from the first, and I myself had no doubt of its cancerous nature. Yet on examination not one of the cancer cells, which we have seen to be always present in undoubted cancer, was to be discovered. It must be remembered also, that Mrs S. never felt pain in the tumour, and that it continued hard up to the period of her death; a result evidently produced from paralysis of the nerves of deglutition, rather than from any particular change in the tumour itself. Any false conclusions we might derive from these points, however, are at once corrected by the case of Sedillot, in which there were lancinating pains, ulceration, and sinking from an exhausting discharge. Notwithstanding these symptoms, so indicative of a malignant growth, Sedillot appears to arrive at the conclusion, that it was not cancerous. At least, although he expresses doubts on the subject, the case is denominated "*canceroid* of the parotid region."

What is the nature of the corpuscles described? May we consider them as nuclei of cancer cells? Certainly, their general size and structure would warrant such a supposition. But the occurrence of numerous nuclei in a tissue, which increases in size so gradually without any of them passing into cell formation, is opposed to our present notions of cell growth. Be this as it may, there can be no doubt that the tumour of the parotid gland now described, presents a very different structure, and sometimes runs a different course, from true cancerous tumours in general. Whether this fact is destined to modify our views of these tumours, and change our practice, further observations only can determine. In the mean time, I cannot too earnestly recommend this form of growth to the attentive consideration of surgeons, whose confirmed ideas with respect to its cancerous nature may possibly undergo very important changes.

ARTICLE VI.—*Report of the Surgical Operations performed at the Newcastle-upon-Tyne Infirmary, during a period of seventeen years and a half.* By SAMUEL FENWICK, M.D., Lecturer on Pathological Anatomy at the Newcastle-upon-Tyne School of Medicine and Surgery.—(Continued from page 415.)

6. *The Influence of Sex upon the Results of Amputations.*

INDIVIDUALS of different sexes differ so much in their employments, habits, and the other circumstances in which they are placed, that we might anticipate a considerable disproportion in their power of withstanding the dangers of an amputation. The following Table shows that the mortality in each hospital is usually much greater amongst the males than amongst the females :—

TABLE XXVI.

Shows the Mortality which has occurred after Amputations performed on Males and Females in different Hospitals.

	Males,			Females,		
	No.	Dead.	Av. Mortality.	No.	Dead.	Av. Mortality.
Newcastle Infirmary, ...	190	46	4·13	35	8	4·37
Glasgow Infr. (Lawrie),	216	88	2·45	55	11	5
Glasgow Infirmary,	85	26	4	15	4	3·75
Edinburgh Infirmary, ...	29	14	2·07	14	4	3·5
M. Malgaigne,	445	245	1·81	115	54	2·12
Total,	965	419	2·3	234	81	2·88

The greatest difference is observable in the Glasgow Infirmary report, as given by Dr Lawrie: according to it, the females having had but half the average number of deaths which had occurred amongst the males. It will be also remarked, that four times as many males as females suffer removal of the limbs,—a circumstance chiefly arising from the greater exposure of the former to accidental injuries. The next point in which we must compare the sexes, is in their comparative mortality after amputations performed at different sites. Not having the opportunity of examining the reports of the American hospitals, I am only able to present the results of the amputations of two Scotch infirmaries, which are given in Table XXVII.

TABLE XXVII.

A Comparison of the Mortality following different Amputations performed on Males and Females at the Edinburgh and Glasgow Infirmaries.

	Males,									Females,								
	Edinburgh Infirmary.			Glasgow Infirmary.			Total.			Edinburgh Infirmary.			Glasgow Infirmary.			Total.		
	No.	Dead.	Aver. Mort.	No.	Dead.	Aver. Mort.	No.	Dead.	Aver. Mort.	No.	Dead.	Aver. Mort.	No.	Dead.	Aver. Mort.	No.	Dead.	Aver. Mort.
Thigh,	17	8	2·12	27	7	3·85	44	15	2·93	8	3	2·66	9	3	3	17	6	2·83
Leg,	8	4	2	28	9	3·11	36	13	2·76	4	3	1	3	7	1	7
Shoulder, ...	2	1	2	7	2	3·5	9	3	3	1	1	1	1	1	1
Arm,	1	13	5	2·6	14	5	2·8	2	2
Fore-arm,	1	1	1	10	3	3·33	11	4	2·75	1	1	2

Only in the leg operations have the amputations on the females been much more successful than on the other sex. The average mortality in the thigh amputations is greater in the former than in the latter, and in the arm, shoulder, and fore-arm operations, the numbers are too small to enable us to compare their results. It might, however, be expected that we should find a considerable difference in mortality between the sexes, when the operations upon them are classified according to the diseases for which they were required; and, to elucidate this point, the following statement has been compiled from the records of the Newcastle Infirmary. In reading it, it must be remembered that the sites are those of the diseases or injuries, and do not refer to the parts at which the limbs were removed.

TABLE XXVIII.

Comparison of the Success of Amputations performed on Males and Females at the Newcastle Infirmary.

		Females.					Males.				
		No.	Dead	Aver. Mort.	Cure.	Dth.	No.	Dead	Aver. Mort.	Cure.	Death.
Diseased joints and bones.	{ Knee and thigh,	9	2	4·5	55·2	19	40	7	5·71	48·25	14·85
	{ Foot and ankle,	7	1	7	65·66	60	42	2	21	50·09	22
	{ Wrist and hand,	2	47	...	6	2	3	40·5	7
Other diseases.	{ Leg and foot, ...	6	2	3	63	23	19	10	1·9	54·48	15·2
	{ Arm and elbow,	3	36·5	...	3	1	3	48·5	16
Traumatic amputations,.....	{ Leg and foot, ...	5	3	1·66	52	19	33	11	3	67·7	12·55
	{ Arm and elbow,	3	54·33	...	21	4	5·25	53·8	25·75

In the amputations for diseased joints and bones, or in that class in which, of all others, the patients are most favourably circumstanced for recovery, we here find the advantage on the side of the males. Thus, of 88 cases of these operations performed on the males, 11 died, or 1 in 8; whilst, of 18 females who suffered amputations for the same diseases, 3 perished, or 1 in 6. Or, if we compare them when classified according to the situation of the disease, the difference appears still greater,—the mortality amongst the males in the thigh operations being 1 in 5·71; whilst amongst the females it amounted to 1 in every 4·5. In the leg amputations, of the males only 1 death took place of 21, whilst of the females 1 occurred in every 7.

That there is a greater mortality amongst the females suffering amputations for this class of diseases, appears to be confirmed by the statements of Dr Lawrie. Thus, of 96 males whose limbs were removed for diseased joints and bones, 16, or 1 in every 6 died; whilst, of 35 females, 7 perished, or 1 in 5. That this excess of mortality does not arise, in the case of the females, from the shorter duration of the disease in them, is easily demonstrable. Thus, only one female is recorded in the operation books of the Newcastle Infirmary whose diseased joint had existed for less than one year, and death followed the amputation. No case amongst the females terminated fatally where the disease had lasted from one to five years;

but of four whose joints had been affected above that time, two died after the removal of the limbs, or 1 in 2. Neither does it appear to have arisen from greater debility occasioning death by the shock of the operation; for none of the deaths amongst the females took place within ten days after the performance of the amputations.

It will be seen, in Table XXVIII., that, after amputations for other diseases, the females have had the advantage in the Newcastle Infirmary, partly, I conceive, from their being less subject to those diseases which it is most dangerous to remove by the operation, and partly, also, we may suppose, from a less plethoric condition of body, enabling them to pass through the inflammatory periods occurring after an amputation with less danger than the other sex. Thus, of 17 cases of amputation for ulcerations of the leg, 12 were men, of whom 7 died, or 1 in 1.71; whilst of 5 women only 1 perished after the operation. If we class together all the amputations performed for pathological causes, the females seem, in a great degree, to have the advantage; thus, Malgaigne gives 280 cases of this operation on males, of which number 138 deaths took place, or 1 in 2.03, and 98 cases of females, of whom 44 died, or 1 in every 2.22; and Dr Lawrie states, that of 110 pathological amputations upon males, 29 died, or 1 in 3.8; whilst, of 41 females upon whom the same description of operation was performed, only 7 died, or 1 in 5.85.

In Table XXVIII., we should observe that the traumatic amputations had proved most fatal in the Newcastle Infirmary amongst the women. This is apparently an anomaly; for, according to Malgaigne, whilst of 165 traumatic amputations performed on males, 107 perished, or 1 in 1.54; of 17 operations performed for accidents on females, only 10 died, or 1 in 1.7; and, according to Dr Lawrie, the average mortality amongst the males for traumatic amputations, was, at the Glasgow Infirmary, 1 in 1.79; but amongst the females, only 1 in 3.5 did not recover from the effects of a similar operation.

To what conclusion, then, can we arrive, with respect to the influence of the sex of the patients upon the results of amputations? From the discrepancies upon this point observable in the reports of different hospitals, from the variety in the results of amputations performed for different diseases on the same sex, is it not probable that sex has of itself but little effect on the issue of an amputation; and that this disproportion in the mortality between the males and females, observable in some hospitals, is chiefly to be attributed to the difference in their habits and employments, or to the difference of the diseases for which the operation is required?

7.—*The Influence of Age upon the results of Amputations.*

The age of a patient is always an important consideration in determining upon the performance of an amputation, and the propriety of the operation often depends upon this point alone. How necessary, then, to have accurate ideas with respect to the amount of influence which age usually exercises; how requisite is it, that our

opinions upon this point should be based rather on facts than upon theories; and that our practice should be guided rather by deductions from figures, than by hasty generalizations or vague impressions!

We see, by the following Table, the general results of amputations performed at different ages.

TABLE XXIX.

Shows the Mortality following Amputations at different Ages in the under-mentioned Hospitals.

	Under 10 Years.		10 to 20.		20 to 30		30 to 40.		40 to 50.		50 to 60.		60 to 70.		70 to 80		80 to 90.	
	No.	Av. Mor.	No.	Aver. Mort.	No.	Aver. Mort.	No.	Av. Mor.	No.	Av. Mor.	No.	Av. Mor.	No.	Av. Mor.	No.	Av. Mor.	No.	Av. Mor.
Liverpool North Hospital,	6	...	11	11	11	5.5	7	1.4	8	4	5	1.66
Glasgow Infirmary,	17	4.25	75	3.75	68	2.61	53	2.96	30	2	17	1.54	8	2	2	...	1	1
University College Hospital,	6	...	16	5.33	17	8.5	10	3.33	9	4.5	3	...	4	...	1
Pennsylvania Hospital,	Under 20 Years.		13	13	26	3.71	22	3.1	16	2.28	2
Massachusetts Hospital,	Under 20 Years.		13	13	31	3.5	9	3	10	5	3	3	1
Newcastle Infirmary,	15	15	53	5.88	65	5	30	3	31	3.44	21	2.62	5	2.5	2	...	2	2
Total,...	38	7.6	150	4.68	218	3.82	135	3.13	103	2.57	54	2.45	22	2.44	6	...	3	1.5

In addition to the above statements, we have the following from Malgaigne:—

TABLE XXX.

A Statement of the Mortality of Amputations performed at different Ages in the Hospitals of Paris, as given by Malgaigne.

2 to 5 years.			5 to 15 years.			15 to 20 years.			20 to 35 years.			35 to 50 years.			50 to 65 years.			65 to 80 years.		
No.	Dead	Av. Mor.	No.	Dead	Av. Mor.	No.	Dead	Av. Mor.	No.	Dead	Av. Mor.	No.	Dead	Av. Mor.	No.	Dead	Av. Mor.	No.	Dead	Av. Mor.
5	3	1.66	66	22	3	81	36	2.25	193	102	1.89	126	76	1.65	70	50	1.4	19	10	1.9

The age of the patient has evidently a considerable effect upon the result of an amputation. The greatest difference in mortality is observable in the Liverpool Northern Hospital, in which, of 17 cases of amputation under 30 years of age, only one died; whilst of 13 between 50 and 70 years of age, 5 perished.

The mortality, it will be observed, does not increase in any regular ratio; but the influence of age seems, as it were, to become less as we approach the extreme limits of life.

Thus, whilst 1 in every 7.6 has died of the persons suffering amputation who are below 10 years of age, and 1 in every 4.68 of those between 10 and 20, in the next decennial period only 1 in 3.82 sinks after the amputation. In Table XXX. the cases are differently classified from those in the table immediately preceding it; and from it we learn, that whereas from 5 to 15 years of age is the

most favourable, below 5 years of age is one of the most fatal periods of life in which a patient can submit to the removal of a limb. Another remarkable circumstance, and one to which we shall afterwards have to refer, is, that in persons between 70 and 80 years of age, the mortality is much less than in those who are classed in the periods immediately preceding or following that time of life—in Table XXIX., none out of 6 cases having perished of those between 70 and 80; whereas, excepting in those below 10 years of age, no amount of mortality, equally small, can be remarked in any other period. In like manner, Malgaigne gives 1 in 1·9 as the average mortality between 65 and 80 years of age; whereas all the preceding periods beyond 20 years of age had exhibited a greater amount of death.

We have before usually found the greatest danger to life coexist with the longest time of cure; but in the next Table this rule appears curiously reversed. We have there, under 10 years of age, an average mortality after amputations of 1 in 15, and the period of cure 72·9 days; whereas, between 10 and 20 years of age, the average mortality is 1 in 5·88, and the time of cure only amounts to 51·58 days.

TABLE XXXI.

Shows the Time of Cure of Amputations performed on Patients at different Ages, together with the Mortality observed at different periods after the Operations at the Newcastle Infirmary.

	Under 10 years.	10 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.
Average time of cure,	72·9	51·58	51·15	57·76	51·33	53·9	36·5	54	49
Average mortality } in 1st week, }	15	10·6	10·83	15	31	10·5	2
Do. in 2d & 3d week,	8·42	9·33	10	3·8	2·5
Do. after 3d week,	12	...	5	5·4	14

In order to contrast the influence of age upon the success of an amputation as observed when it is performed at different sites, I have constructed the next Table—the first part is formed from the records of the Newcastle Infirmary, the second part from Malgaigne's statements of the mortality in the hospitals of Paris, and consists solely of males.

TABLE XXXII.

Shows the Mortality following Amputations of different parts, performed on Patients of different Ages, as observed at the Newcastle Infirmary and in the Parisian Hospitals.

	Under 10 Years.	10 to 20 Years.	20 to 30 Years.	30 to 40 Years.	40 to 50 Years.	50 to 60 Years.	60 to 70 Years.	70 to 90 Years.	Newcastle Infirmary.
Lower extremity,.....	0 in 9	5 83	5·77	2·66	3·12	2·5	1·5	1	Malgaigne.
Upper extremity,.....	6	6	3·66	5	6	5	0 in 1	0 in 3	
		2 to 5 yrs.	5 to 15.	15 to 20.	20 to 35.	35 to 50.	50 to 65.	65 to 80.	
Thigh,.....	...	1	2·28	1·86	1·53	1·22	1	0 in 1	
Leg,.....	...	2	6	2·12	1·88	1·9	1·41	1	
Arm,.....	1·75	4	1·86	2·25	1·8	3	Malgaigne.
Fore-arm,.....	2	0 in 1	5	2·66	3	0 in 1	

The effects of age are here very different upon a number of subjects undergoing amputation of the upper and of the lower extremities. In the latter class of operations the mortality seems to have gradually increased, with the exception of the period between 30 and 40 years of age, in which the amount of death is greater than in the subsequent period. In the amputations of the upper extremity no similar rule can be laid down; for the number of deaths is greater between 20 and 30, than between 60 and 90 years of age. Again, in Malgaigne's statement, in amputations of the thigh and leg the risk of death seems to increase with the age of the patient, excepting below 5 years of age; whilst in those of the arm, the number of deaths occurring in persons above 65 years of age is less than at any other period, excepting between 15 and 20 years. In the amputations of the fore-arm the same circumstance is observable, the mortality between 20 and 35 years of age being 1 in 5; whilst in the period most favourable in the graver amputations, viz. from 5 to 15 years of age, it is 1 in 2.

We have before observed the difference of mortality between the cases of pathological and traumatic amputations, and have seen how different are the results they furnish, even when examined under similar conditions of sex. It, therefore, becomes necessary to ascertain the effects of age upon each of these classes of operations separately. In Table XXXIII. will be found cases of pathological amputations as given by Malgaigne and Lawrie, and as extracted from the books of the Newcastle Infirmary, the operations at each place being classified according to the ages of the patients upon whom they were performed.

TABLE XXXIII.

A Statement of the Mortality of Amputations performed for Diseases, on Patients of different Ages, and at various Hospitals.

Years of Age.	Glasgow Infirmary.			Newcastle Infirmary.			M. Malgaigne.				Pathological Amputations.
	No.	Dead.	Av. Mort.	No.	Dead.	Av. Mort.	Years of Age.	No.	Dead.	Av. Mort.	
1 to 10,....	15	4	3·75	6	...	0 in 6	2 to 5,....	4	2	2	
10 to 20,....	48	5	9·6	30	2	15	5 to 15,...	57	15	3·8	
20 to 30,....	33	10	3·3	44	5	8·8	15 to 20,...	66	28	2·35	
30 to 40,....	30	8	3·75	18	7	2·57	20 to 35,...	128	63	2·03	
40 to 50,....	15	5	3	18	4	4·5	35 to 50,...	72	40	1·8	
50 to 60,....	4	1	4	16	5	3·2	50 to 65,...	40	29	1·37	
60 to 70,....	6	3	2	4	2	2	65 to 80,...	11	5	2·2	
70 to 80,....	2	
80 to 90,....	1	

From the above statements it is pretty evident, and the opinion is supported by the study of other operations performed for diseases, that whilst amputations for disease are most successful when performed on persons between 5 and 20 years of age, the chance of recovery of those under 5 years of age is comparatively small. It should be also remarked, that the increase of mortality in this class of amputations is not gradual; but that, after a certain period, what

had been at a previous age an operation of but small hazard, gradually assumes a most severe character. This turning-point is in these British hospitals at 20 years of age; but in the Newcastle Infirmary the mortality does not become excessive until after 30, when the amount of death which had been, up to that age, so dissimilar in the Glasgow and Newcastle Infirmarys, becomes afterwards more closely approximated. After the age of 30, the mortality seems as a general rule gradually to increase in all the hospitals, until the commencement of old age, when the danger usually becomes less; a smaller amount of death having occurred, both in the French hospitals and at the Newcastle Infirmary, after 70 years of age, than had been observed in the period immediately preceding that advanced time of life.

TABLE XXXIV.

Shows the success attendant on Amputations, performed for Diseases upon Persons of different Ages, as observed at the Newcastle Infirmary.

LOWER EXTREMITY.

	Diseased Joints and Bones.						Other Pathological Amputations.					
	Under 10 years	10 to 20 years.	20 to 30 years.	30 to 40 years.	40 to 50 years.	50 to 60 years.	10 to 20 years	20 to 30 years.	30 to 40 years.	40 to 50 years.	50 to 60 years.	60 to 70 years.
Number,.....	6	23	37	13	13	6	4	3	3	2	6	3
Deaths,.....	...	1	2	5	13	1	1	2	2	0	3	2
Average Mort,...	...	23	18.5	2.6	4.33	6	4	1.5	1.5	...	2	1.5
Time of Cure,..	30	49.9	51.1	52.1	55.8	63.6	65	59	50	44	65.6	44
Mort. in 1 week,...	...	23	37	13	3	3	...	6	...
Do. after 1st week,.....	36	3	4.33	6	4	2	2	...	2.5	1.5

TABLE XXXV.

Shows the success attendant on Amputations of the Upper Extremity, performed for Diseases upon Persons of different Ages, as observed at the Newcastle Infirmary.

UPPER EXTREMITY.

	10 to 20 Years.	20 to 30 Years.	30 to 40 Years.	40 to 50 Years.	50 to 60 Years	60 to 70 Years.	70 to 80 Years.	80 to 90 Years.
Number of cases,.....	3	4	2	3	4	1	2	1
Deaths,.....	...	1	...	1	1
Average mortality,.....	...	4	...	3	4
Average cure,.....	61	48	34	26	38.6	29	54	49
Mortality in 1st week,.....	...	4	4
" after 1 week,.....	3

In these Tables it is attempted to gain a more accurate idea of the hazard of the operation performed at different ages for certain diseases, than can be obtained from a more general classification.

In cases of amputations for diseased joints in the lower extremity, the increased mortality, after 30 years of age, is exceedingly striking; for of 32 cases of amputation performed on persons above that age, 9 perished, or 1 in every 3.55, whilst of 66 of those under that time of life, only 3 died, or 1 in 22. After 50 years of age the mortality becomes less than in the 20 years before that time; so that in persons

between 30 and 50 years of age, the amputations for diseased joints in the lower extremity have been least successful. How carefully, then, should we in these operations consider the time of life at which our patient has arrived before pronouncing a favourable prognosis! The mortality in the second week after the operation shows us, that it is in the occurrence of a less amount of secondary inflammations that the superior success of early and extreme age consists.

If we compare the average mortality during the second week after amputations for diseased joints, in persons between 30 and 50 years of age, with that occurring in the same period of life in traumatic amputations, we shall discover but little advantage to arise from the circumstance of the disease having previously existed; the mortality after the first week being, in the former class of amputations, 1 in 3·55, whilst in the latter it is 1 in 2·83. That the time required for cure also increases with the age is worthy of remark; each period between 10 and 50 years of age being marked by a very gradual increase, whilst below and above these periods the difference is very considerable.

Amongst amputations of the lower extremity performed for other diseases, a different law of mortality may be observed; and this we might be led to expect, on account of the subjects of the operations of which it is composed, not having been so much reduced by disease as those cases from which the other side of the Table has been formed. Amongst the other pathological amputations, the usual comparatively small amount of death from secondary disease in persons below 20 years of age is observable; but in the succeeding period, in which, amongst amputations of diseased joints, we remarked the mortality to be still comparatively small, we here find it to have risen to an equality with that of the most fatal age. It is curious that, amongst these amputations, the time of cure is as great below 20 years of age as at any future time, therein differing from the results obtained by the analysis of cases of amputation performed for diseased joints.

In Table XXXV. I have classed together all the pathological amputations of the upper extremity, the numbers of the operations performed for each disease being too small to admit of their separation. No regular increase in mortality corresponding with the age can be remarked. The fact, that only 1 out of 8 of these cases of amputation who were above 50 years of age died, whilst 1 out of 4·5 perished who were between 20 and 50, is valuable with reference to the operation of resection of the elbow joint. It teaches us to perform the latter operation during the earlier periods of existence, as there is both a greater chance of its being successful, and a less probability of recovery if amputation be resorted to; on the other hand, after 50 years of age, the hazard from the amputation decreases, whilst the danger from the removal of the joint becomes greater. It is worthy of observation, how slow the wound is in healing, below 20 years of age, amongst pathological amputations of

the upper extremity ; and, however inexplicable the fact may be, it will be seen, by examining Tables XXXIV. and XXXVI., that at this early period of life, although the mortality is usually small, yet the time required for cure is generally higher than at any other period.

TABLE XXXVI.

Shows the success attendant on Amputations, performed for Accidents on Persons of different Ages, at the Newcastle Infirmary.

	Lower Extremity.							Upper Extremity.						
	Under 10 Yrs.	10 to 20 Years.	20 to 30 Years.	30 to 40 Years.	40 to 50 Years.	50 to 60 Years.	80 to 90 Years.	Under 10 Yrs.	10 to 20 Years.	20 to 30 Years.	30 to 40 Years.	40 to 50 Years.	50 to 60 Years.	
No. of cases,	3	8	12	8	10	3	1	6	15	7	3	3	1	
" deaths	...	4	5	2	5	2	1	1	3	2	1	
Aver. mort.,	...	2	2.4	4	2	1.5	1	6	5	3.5	3	
Av. in 1st week,...}	...	2	12	...	10	...	1	6	...	7	
Aver. after 1 week,..}	2.75	4	2.25	1.5	5	6	3	
Aver. cure,..	86.5	61.5	63.6	66	42.25	59	53.3	38.6	78	70.6	...	

TABLE XXXVII.

Shows the Mortality following Amputations, performed for Accidents on Patients of different Ages, at the Glasgow Infirmary and Hospitals of Paris.

Ages.	Glasgow Infirmary.			Ages.	Lower Extremity.			Upper Extremity.			
	No.	Dead.	Av Mort.		No.	Dead.	Av. Mort.	No.	Dead.	Av. Mort.	
Under 10,....	2	2 to 5 years,	1	1	1	M. Malgaigne.
10 to 20,....	30	13	2.3	5 to 15 "	6	5	1.2	2	1	2	
20 to 30,....	35	16	2.18	15 to 20 "	10	6	1.66	4	1	4	
30 to 40,....	23	10	2.3	20 to 35 "	49	29	1.68	11	8	1.37	
40 to 50,....	15	10	1.5	35 to 50 "	32	22	1.45	18	11	1.63	
50 to 60,....	13	10	1.3	50 to 65 "	20	17	1.17	7	3	2.3	
60 to 70,....	2	1	2	65 to 80 "	1	1	1	4	2	2	
70 to 80,....	2	
80 to 90,....	1	1	1	

The great mortality amongst cases of amputation performed on persons below 20 years of age, for accidents, at once strikes our attention, as we have previously found this the least fatal period of life amongst pathological amputations. It is, however, easily explained by looking at the time at which death occurred. It appears from Table XXXVI., that in all the fatal cases below 20 years of age, death took place within the first four days ; and that out of seven amputations where the patients survived beyond that time, none were afterwards lost. There can be, therefore, little doubt that the great mortality observed amongst those of the same age at the Glasgow and Parisian hospitals, might be similarly explained. We have thus, in Tables XXXIV. and XXXVI., examined forty-four cases of amputations performed at the Newcastle Infirmary on the lower extremity, the subjects of which were below 20 years of age. Of these, 5 died within the first four days, or in the period of shock ; whilst of 39 who survived the depression of the operation,

only 1 was subsequently lost, and this case was amongst the most unhealthy class of diseases. From this, the practical conclusion appears to be, that in cases of amputation, when the patients are below 20 years of age, we need scarcely fear the occurrence of phlebitis, or secondary inflammations; and that, although the patient is more apt, at this age, to sink from the double shock of an accident and subsequent operation, yet, if he survive this trial, his chance of recovery will be exceedingly good. The examination of the results of amputations of the upper extremity, seems to throw some doubt upon the truth of this rule; for, of 24 cases of the operation performed on this limb in persons below 20 years of age, only 1 died within the first four days, whilst of 23 who lived beyond the first four days, 3 afterwards perished. It should, however, be remembered, that persons at this period of life having usually the care of the machinery in factories, are especially liable to accidents of the upper extremity, complicated with severe injuries of other parts of the body; and it is, I believe, from this cause that so large a mortality is observable in Table XXXVI. As was before remarked, excepting where the patient has been prepared by previous disease, as in diseases of the joints, the wound of an amputation will usually be found longer in healing in those below 20 years of age, than in persons suffering the operation at a somewhat later period of life. Between 20 and 50 years of age, the mortality does not gradually increase in the traumatic amputations of the lower extremity; but it is less between 30 and 40 than in the period either immediately preceding or following that age. This fact is observable both in the Glasgow and in the Newcastle Infirmary reports. The danger is, however, less between 20 and 30 than between 40 and 50 years of age. But if, on the other hand, we divide, as in Malgaigne's report, this period into equal parts of fifteen years each, it will be found that but a small difference in mortality exists between the first and the second half of this thirty years. It will be useful for the practical surgeon to remember, in deliberating upon the removal of a leg for an accident, that from 30 to 40 is a more successful age than from 20 to 30, and that this latter period is, again, somewhat, but not very much, more favourable than from 40 to 50. The time of cure seems curiously to follow the rule of its being longest where the mortality is least: thus, between 30 and 40 years of age, it is 66 days; between 20 and 30, 63·6 days; and between 40 and 50 years of age, it is only 42·25 days. In the upper extremity, out of 13 cases of traumatic amputation between 20 and 50 years of age, 3 perished, or 1 in 4·33, a mortality considerably higher than what took place before that time of life. According to Malgaigne's statistics, this is the most dangerous period of life for removing the upper extremity for an accident; and this opinion is strengthened by Table XXXV., upon which we have before commented. After 50 years of age, the danger from the operation, when performed for injuries on the lower extremity, rapidly

increases ; thus differing from what we before remarked in the amputations for diseased joints, and in those performed for pathological causes on the upper extremity.

In traumatic amputations upon the upper extremity, the mortality, as seen by Malgaigne's statements in Table XXXVII., diminishes after 50 years of age, forming a striking contrast with the increased mortality in amputations of the lower extremity after that time of life. As we before found so great an influence exercised by the duration of a disease for which an amputation is performed, it is now necessary to ascertain at what age this influence is chiefly experienced. Table XXXVIII. has been constructed from the records of the Newcastle Infirmary, and is designed to illustrate this point with respect to amputations for diseased joints of the lower extremity.

TABLE XXXVIII.

Shows at what Age the duration of the Disease in a Joint has the greatest effect on the Mortality following the Amputation required for its removal, as observed at the Newcastle Infirmary.

	Under 20 Years.		20 to 30 Years.		30 to 40 Years.		40 to 50 Years.		50 to 60 Years.		
	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	No.	Av. Mort.	
Within 1 yr.'s duration,	{ 3	...	5	2·5	5	2·5	Thigh.
	{ 5	...	3	...	2	2	1	Leg.
1 to 2 years' duration	{ 1	...	5	2	Thigh.
	{ 1	...	6	...	1	...	1	Leg.
2 to 5 years' duration.,	{ 2	1	1	1	Thigh.
	{ 3	...	3	...	1	...	1	1	Leg.
Above 5 yr.'s' duration,	{	2	...	3	1·5	2	2	2	...	Thigh.
	{ 1	...	3	2	Leg.

As former Tables have shown how great is the difference in the mortality between 20 and 30 years of age, according as we investigate it in the class of amputations for diseased joints, and in those amputations which are performed for accidents ; so, in Table XXXVIII., we find that all the deaths at that period of life had occurred amongst those who for the shortest time had suffered from disease. It will be advisable, therefore, in persons between 20 and 30 years of age labouring under incurable diseases of the joints, to delay the performance of amputation as long as the strength and other circumstances of the patient will permit, instead of resorting at an early period to the operation, on account of there being but little prospect of a natural recovery. In that time of life which, in amputations for diseased joints of the lower extremity, is most fatal—viz. from 30 to 50 years of age—we find also, that an amputation succeeds better if the disease has existed from one to five years, than if the operation be performed at an earlier stage of the disease. Thus, of 8 cases in which the illness had existed only one year, 3 died, or 1 in every 2·6 ; whereas, of 8 who had suffered from the diseased joint from one to five years, only 2 perished, or 1 in 4. Only two cases were operated upon who were above 60 years of

age; in both, the disease was of long standing, and in both the operation was successful.

In analysing the causes of death in secondary amputations, it was found that, whilst the patients undergoing that operation were not so liable to inflammatory affections as those suffering amputation immediately after the receipt of an accident; yet, that they were more likely to sink from exhaustion than cases of pathological amputation, and that this, in fact, constituted the chief danger of the amputation when performed at a considerable distance of time from the accident. From this we might be led to conclude, that the period of life in which the reduction of the strength of the patient by disease has the greatest effect in preventing subsequent inflammation, and, at the same time, that period in which the greatest power of resisting shock existed, we should find the most favourable for the secondary amputations; and this supposition Dr Lawrie's statistics confirm. Between 20 and 30 years of age, no deaths occurred out of 7 secondary amputations; whilst before that period, in which we found, by a previous Table, shock had so deadly an influence, 7 died out of 13, or 1 in every 1·85. Between 30 and 50 years of age, during which death most generally occurs from secondary inflammation, whether the operation be performed for traumatic or pathological causes, 12 died out of 16 cases of secondary amputation, or 1 in 1·33; whilst, beyond 50 years of age, of 8 cases, 6 also died, or 1 in 1·33. From 20 to 30 years is the age, then, at which we gain most by deferring, for the longest time, an amputation for an accident, both because the reparative process is most active at that period, and therefore there is a greater chance of a natural recovery, and also because there is less danger than at other times of life of the patient sinking from exhaustion immediately after the operation; whilst, by the continuance of suppuration, the chance of the secondary inflammations to which that age is liable after amputations, is removed. In patients below 20 years of age, we should remove the limb at an earlier period if there seems but small probability of a natural recovery from the accident; because there is less power in persons of that time of life of surviving the shock of the amputation, if it be long deferred, and we gain less in other respects by delay, on account of the small chance of the occurrence of phlebitis, or other secondary inflammations, after the amputation, when performed on persons of that early age. But the question arises, at what age are persons suffering compound fractures of the limbs least likely to reach that period when amputation can be safely employed? From an analysis of a number of such accidents to the lower extremities, recorded in the operation books of the Newcastle Infirmary, I find, that of persons below 20 years of age, 3 died out of 24, or 1 in 8, within the fourth and twenty-first day after the infliction of the injury. In none of these, therefore, could any chance of a favourable result, after amputation, have existed, otherwise that operation would have been attempted. Of 22 cases of compound fracture between

20 and 40 years of age, 4 died, or 1 in 5.5; whilst of 20 cases who were between 40 and 60 years of age, 2 perished, or 1 in 10; and of 4 persons above that time of life, none died. It should be, therefore, borne in mind, in attempting to save a dangerous case of compound fracture of the leg without amputation, when the person is between 20 and 30 years of age, that there is less chance of a patient reaching the period of suppuration, when secondary amputation, if required, is most successful; although, if that operation can be performed, there is greater probability of recovery than if it were performed upon a person below 20 years of age.

In persons between 30 and 40 years of age, I should imagine the attempt to save a compound fracture of the lower extremity was exceedingly hazardous, both on account of their liability to the secondary inflammations which are apt to follow the accident, and also because, if the secondary operation be permitted, the chance of recovery after the amputation is so small. As the amount of fatal disease between the fourth and twenty-first days after compound fractures has been so much smaller in persons between 40 and 60 years of age than in those of the period immediately preceding that time of life, we must either suppose that the surgeons of the Newcastle Infirmary had amputated a large number of the worst cases who were at this time of life, which they would have attempted to have saved if their ages had been less; or, otherwise, that persons of that age suffering severe accidents, are really less liable to fatal diseases between the fourth and twenty-first day after a compound fracture, than those between 20 and 40 years of age. But, from whichever of these causes this small amount of mortality has arisen, it is evident that accidents of greater severity, occurring in persons between 40 and 60 years of age, may be more safely deferred to the suppurative stage than is usually imagined. And, although the mortality in the case of secondary amputations above 50 years of age, is 1 in 1.33, yet we have found, by Tables XXXVI. and XXXVII., that an equally excessive amount of death takes place amongst those at that time of life who undergo the primary operation, when performed on the lower extremity; whilst in the class of diseased joints, to which secondary amputations are most similar, the number of deaths is remarkably small at that age. In order, however, to settle points of this importance, we should have large numbers of carefully observed and minutely classified cases, both of amputations and of compound fractures; for, I believe, there are few investigations more likely to prove useful than those which can be made regarding the age at which we may most safely treat a compound fracture without amputation.

I before stated my belief, that the discrepancies between the results of the practice of different hospitals respecting the mortality of the sexes, must be explained by the varying habits and occupations of the males and females in the districts to which the reports relate. The next Tables, however, show that, when analysed according to

age, an appearance of law can be obtained with respect to their comparative chances of recovery after an amputation.

TABLE XXXIX.

A Statement of the Mortality of Cases of Amputation, classified according to Age, Sex, and the cause requiring the operation, observed in the Newcastle and Glasgow Infirmaries.

	Pathological.				Traumatic.			
	Males.		Females.		Males.		Females.	
	Glasgow.	Newcastle	Glasgow.	Newcastle	Glasgow.	Newcastle	Glasgow.	Newcastle
	Av. Mort.	Av. Mort.	Av. Mort.	Av. Mort.	Av. Mort.	Av. Mort.	Av. Mort.	Av. Mort.
Under 20 years.....	7.5	16	6	0 in 4	1.6	4.14	0 in 5	3
20 to 30 /	2.88	7.6	7	0 in 6	2.07	2.57	3	0 in 1
30 to 40 /	3.5	3.66	4.5	1.75	2.2	3.33	0 in 1	0 in 1
40 to 50 /	2.5	4.33	5	5	1.5	2.75	...	2
50 to 60 /	4	2.8	...	0 in 2	1.37	3	1	1
60 to 70 /	1.33	1.5	0 in 2	0 in 1	2
70 to 80 /	0 in 2	...	0 in 1	0 in 2
80 to 90 /	0 in 1	1	1

TABLE XL.

Shows the Mortality of Cases of Amputation, classified according to Age, Sex, and the cause requiring the operation, as observed in the Hospitals of Paris.

M. MALGAIGNE.

	1 to 5 Years.	5 to 15 Yrs.	15 to 20 Yrs.	20 to 35 Yrs.	35 to 50 Yrs.	50 to 65 Yrs.	65 to 80 Yrs.	
Males,.....	2	4	2.36	1.94	1.89	1.43	2	} Pathological. Traumatic.
Females,....	2	3.57	2.33	2.55	1.54	1.16	3	
Males,.....	1	1.33	2	1.62	1.51	1.35	1.66	
Females,....	...	1	1	2.5	1.33	3	1.5	

Now, if we first examine the pathological amputations, we find that, below 20 years of age, when shock is chiefly to be dreaded, the females suffer more than the other sex. From 20 to 50 years of age the advantage is on the side of the females, excepting that at the Newcastle Infirmary, from 30 to 40, and in Paris from 35 to 50, the females have suffered less than the other sex. Beyond this time of life we have not the means of comparing them in the British hospitals; but at Paris the females present the greatest mortality from 50 to 65, and the males from 65 to 80.

In the amputations performed for accidents, the Glasgow report shows a greater amount of death in those below 20 years of age amongst the males; whilst at Newcastle and at Paris the same rule is observed as in the former class of operations. From 20 to 35 the females keep the advantage; but after this period of life there is a greater amount of success amongst the males.

(To be continued.)

ARTICLE VII.—*Case of Aneurism of the Arteria Innominata spontaneously cured, with Obliteration of the Left Common Carotid Artery.* By JAMES A. WISHART, M.D., Assistant-Surgeon 15th Regiment.

WILLIAM MARTIN, æt. forty, a labourer, married, of fair complexion. On the 8th April 1844, applied to Mr Biddle, surgeon at Edmonton. He complained of having caught a severe cold while at work, by which he had lost his voice. When Mr Biddle heard him speak, it at once struck him that his tone of voice was occasioned by pressure on some part of the windpipe. On placing his fingers over the clavicles, a most distinct murmur was perceived, extending also up the neck. Over the superior part of the sternum, a strong impulse, accompanied with a bruit, was felt, synchronous with the stroke of the heart. The pulse in the right wrist was barely perceptible. These symptoms indicated the existence of an aneurism. Along with it, the man had acute bronchitis, for which he was more immediately treated. His loss of voice, cough, and dyspnœa still continuing, he was admitted into Middlesex Hospital on the 27th of June, and discharged the beginning of August. When I first saw him on the 17th of this month, his symptoms were great dyspnœa, suffocating cough, inability to raise his voice above a whisper, moist rales over both sides of the chest, a strong impulse felt over the top of the sternum, and the pulse in the right wrist not perceptible.

The treatment was directed towards the disease of the lungs, and he was kept on very low diet. He continued much in the above state for a month, after which he gradually improved. He was strictly confined to the house, and took small doses of acetate of lead daily, for several weeks. I visited him occasionally till the end of the year. There was then only a very slight impulse to be felt over the sternum. He had had for some time back repeated attacks of hæmoptysis, connected with the state of his lungs, which were then in an advanced stage of phthisis. He lingered on in much the same condition till the middle of February 1845, when he became much worse, and died on the 21st. A month before his death no pulse could be felt in either of the carotids, for how long previous to that time it was absent I could not say.

On opening the body, thirty-six hours after death, both lungs were found full of tuberculous cavities. The aneurism was carefully dissected out and removed. It involved the whole of the arteria innominata. On laying open the aorta, the aneurism was found spontaneously cured, the innominata being entirely obliterated. The aneurism was considerably larger than a duck's egg. The tunics of the vessel were dilated in all directions into an oval pouch, which was completely and accurately filled by compact fibrine, deposited layer after layer in such a manner as to fill the interior to the level of the aorta. The orifice of the innominata was dilated at this part to the diameter of a crown piece, and the arch of the aorta was also somewhat expanded, in the coats of which there was an extensive deposit of atheromatous matter. The fibrine occupied a small extent of the interior of the aorta, so as completely to cover up the orifice of the carotid artery. This vessel, with the right carotid and subclavian, were entirely blocked up, and the circulation to the brain could only have existed in any considerable stream through the left vertebral artery, both it and the subclavian being somewhat enlarged in calibre. The circulation to the right superior extremity must have been carried on principally by anastomosis between the branches of the thyroid axis and vertebral arteries of the left with those of the right side. The right common carotid artery was slightly contracted, and filled with fibrine for about four inches above the aneurism. The right subclavian, and the branches of the thyroid axis, vertebral artery, &c., were pervious, and of their usual calibre. The aneurismal tumour adhered firmly to the front and right side of the trachea, on which it pressed, and slightly diminished its size; the mucous membrane of this part of the tube was of a reddish-brown colour, and a few points about the

size of pins' heads were raised, as if at one time the aneurism had showed a disposition to burst into the trachea. The pneumogastric nerve adhered closely to the coats of the sac in front, and was considerably stretched.

The preparation is in the museum of the medical department of the army at Chatham.

It will be observed that this man was kept on very low diet for a period of nearly six months, during which time he never left his room, and was principally confined to bed. It is probable that these circumstances, constituting pretty nearly the treatment of Valsalva, had great effect in producing the obliteration.

Part Second.

REVIEWS.

Contributions to the Pathology and Practice of Surgery. By JAMES SYME, F.R.S.E., &c., Edinburgh. 8vo. 1848. Pp. 336.

ALTHOUGH there are many surgeons who enjoy, to a great extent, the confidence of the profession, there are few among them who establish a solid reputation by introducing sound principles of general application, or new operations for the relief or cure of diseases. Indeed, it is rare that very popular surgeons leave any thing of value to their descendants. Their experience and operative skill, for the most part, perishes with them; and surgery as an art, although temporarily illuminated by the brilliancy of their talents, receives no permanent lustre from their achievements. Mere operative skill, or even profound powers of diagnosis, although highly valuable to the public, and necessary for building up individual celebrity, do not in themselves confer benefit on the science or art of surgery. This is only to be done by introducing a doctrine which will guide our practice, or inventing an operation which will improve it. In either case we recognise a step in advance, and are ready to honour, and hand down to posterity the names of those who have established the one, or accomplished the other. We feel satisfied that there is no one now living who more justly merits this distinction than the Professor of Clinical Surgery in the University of Edinburgh, and that the book now before us is a convincing proof (were any required) of the correctness of the statement.

The present work of Mr Syme consists of thirty-one articles or essays, all of which, with the exception of the first (in which he claims the discovery of rendering caoutchouc soluble, and applying it to various useful purposes), bear upon practical points of the highest

interest to the surgeon. The great majority of them have appeared during the last twenty years in various periodical publications, in the volumes of which they are now nearly, if not quite, beyond the reach of most readers. It is not a bare reprint of them, however, which is now offered to the public. They nearly all contain additional cases, observations, and remarks, and many of them have acquired increased value, from the circumstance that, since their original publication, the author has been enabled to confirm his previous views by enlarged experience, and to furnish the sequel of several rare and valuable cases.

It is not our intention to enter into an analysis of the different articles contained in this work. Mr Syme's style of writing is so good and terse—indeed, in this respect, almost a model for practical writers—that any condensation would only have the effect of rendering obscure, what is now unusually clear and intelligible. Our readers also must be aware that many of these essays appeared originally in the pages of this Journal—a circumstance to which we may allude with some gratification, as a proof of their great value to the profession. We shall content ourselves, therefore, with making a few selections from subjects that have not been previously published, as evidence that our estimation of the book is correct, and as specimens of its character.

ON THE TREATMENT OF ULCERS OF THE LEG.

Callous Ulcers.—The depressed surface and thick elevated edges which characterise the callous ulcer, depend upon a swollen state of the limb, not soft and yielding like that of ordinary cedematous effusion, but of firm brawny consistence. It proceeds from the continued irritation of the sore, usually aggravated by occasional attacks of inflammation, and is therefore apt to attend, in a greater or less degree, the other sorts of ulceration which occur in the same part of the body, so as, in a corresponding measure, to confuse their distinctive features and diagnosis.

Twenty years ago, the treatment of callous ulcers was believed to have been rendered perfect by combining the modes of management advocated by Messrs Baynton and Whately; the former of whom encircled the limb, for the extent of an inch or two above and below the sore, with straps of adhesive plaster; while, by the latter, a roller was applied from the toes to the knee for the purpose of affording general support. This process, from the expense of materials and the attention requisite for their proper employment, is hardly within reach of that rank which chiefly suffer from the disease, except in hospital practice, where cases of this kind, from their monotonous features, slow progress, and proneness to relapse, are admitted with reluctance. So far, therefore, as poor people were concerned, there was in general no efficient remedy available for the relief of callous ulcers.

In 1829, I proposed a different plan of treatment, which has now stood the test of sixteen years' trial in most parts of the world, and may, I think, be regarded in every point of view as preferable to the other. This was to apply a large blister over the sore and neighbouring swelled part of the limb, which has the effect of speedily dispersing the subcutaneous induration and thickening, so as to relax the integuments, and thus remove the obstacle opposed to healing action. In the course of a short time, seldom exceeding a few days after the blister has been applied, the surface of the ulcer, however deep it may have been, is found to be on a level with that of the surrounding skin,—not, of course, through any process of reproduction, or filling up, but merely from the removal of interstitial effusion, allowing the integuments to descend from the position to which they had been elevated, as may be readily ascertained by measuring the circumference

of the limb, before and after it has undergone the effect of blistering. But, along with this change of form, the ulcer, in other respects, no less speedily acquires the characters of a healing sore, assuming a florid colour, affording a moderate discharge of purulent matter, and presenting a granulating surface with surrounding margin of citatrising pellicle. No subsequent treatment beyond the attention requisite for ensuring quiet and cleanliness is needed; and recovery is completed, not only more quickly, but with much less tendency to relapse than when accomplished by other means.

The facility, rapidity, economy, and lasting effect of this treatment, seem to give it a decided advantage over the other methods in use; and, so far as I am aware, no one who has tried the plan ever afterwards hesitated to employ it in preference to any other. In order to derive the full amount of benefit which the practice affords, it must be carried fairly into effect; and with this view, the principle upon which it is founded should be distinctly understood. I still entertain the opinion originally expressed, that the blisters act beneficially by inducing a process of absorption. The enlargement of the limb being of secondary formation, and resulting from the continued irritation of a sore allowed to remain unhealed through neglect or improper treatment, when once established, prevents the contraction of granulating action, by which alone solutions of continuity, not within reach of union by simple adhesion, admit of reparation. Pressure, the horizontal posture, and all other means that tend to remove the obstacle thus presented, will promote the patient's recovery. But of all the means that can be employed for this purpose, blisters appear to be the most efficient, and should, therefore, be employed for the remedy, not only of the purely indolent, or callous ulcer, but of other kinds, which, in addition to their own peculiar characters, show evidence of complication with indurated enlargement of the limb. From this condition, it is hardly necessary to mention that the œdematous swelling of the weakness and impeded circulation must be distinguished.

Varicose Ulcers.—With regard to the varicose ulcer, I have merely to state that my opinion is not in favour of aiming at what is called the "radical cure," by obstruction of the vein or veins concerned. The ligature of Sir Everard Home, the incision of Sir Benjamin Brodie, and the caustic potass of Mr Mayo, have been succeeded by the much more sure, safe, and effectual method of Velpeau, who accomplishes the object of obliteration by passing a pin through the skin under the vessel, and then tying a thread tightly round the included part. I have frequently practised this procedure, and never met with any bad consequences from doing so; but am nearly satisfied, from what has fallen within my observation, that the operation is barren of good effects in permanently remedying the tendency to ulceration. If this should prove to be the case, it will be matter of less regret, on account of the improvements which have been made in conducting the palliative treatment, especially by the contrivance of elastic bandages and supports, much more convenient than those formerly in use; and by means of which, after the sores are healed, patients may be rendered not only comfortable, but pretty secure against relapse. The *Black Wash* has long seemed to me the best application for promoting cicatrisation of the ulcer. If it comes under treatment in an inflamed or irritated state, poultices should be employed in the first instance, and if the depressed surface and thick edges denote a complication of the callous condition, blistering will be proper instead of such relaxing means.

Mercurial Ulcers.—The mercurial ulcer of the legs, or that which proceeds from the injurious influence of mercury upon peculiar constitutions of the system, occurs in two different forms, being either superficial and confined to the integuments, with their subjacent cellular texture, or deeply seated in the periosteum and surface of the bone. The former is preceded by flattened indurations of the skin, which, after slowly suppurating, discharge their contents by different apertures that communicate together, so as to produce an irregular ulcerated surface, with burrowing sinuses. The latter generally occupies the shin, and is recognised by its firm connexion with the bone, which appears enlarged and irregular, either from really being so, or from the deceptive thickening of its periosteum.

The superficial mercurial sore was formerly treated more by internal than external remedies; and, like the other effects of mercury co-operating with venereal poison upon unsound constitutions, being regarded as the legitimate offspring of syphilis, received, under the title of antidote, a fresh supply of the poison which had given rise to it. The changes immediately attending this most mistaken and mischievous practice, being usually beneficial so far as existing symptoms were concerned, tended to confirm the delusion, which too frequently led the patient, by progressive steps of weakness, emaciation, and disease, to his grave. The enlightened views of the late Dr Thomson gave Edinburgh a distinguished place in reforming this department of medicine; and though the comparatively slow progress of improvement in the capitals of England and Ireland, may still, perhaps, tolerate the administration of mercury for the ulcer in question, it was long since abandoned in this school, and succeeded by treatment of a local kind. This was destruction of the textures concerned by application of the caustic potass, which, at once depriving them of vitality, reduces the parts surrounding and uniting the ulcers to a state of slough, which separates in due time under the employment of poultices, and presents a healing surface of granulations.

This procedure was certainly efficient, but very painful, and often required repetition, before recovery could be completed. It therefore readily gave place to the more recent introduction of iodine, as a corrective of the constitutional derangement, which gives rise to ulcers of the kind under consideration, as well as to other so-called syphilitic symptoms. Small doses of the hydriodate of potass, such as two grains, three times a-day, administered in simple watery solution, and without any sarsaparilla,—of which I never prescribed a particle, either solid or fluid, in hospital practice,—quickly affords the relief desired, with no assistance, except the application of lint moistened with water, or a diluted solution of sulphate of copper, and occasional blistering, especially if there be much thickening and induration.

The deep-seated form of mercurial ulcer which affects the periosteum and surface of the bone, has been, and I fear still is, in some parts of the world, the subject of more unwarrantable and hurtful practice than almost any other surgical ailment, although it admits of treatment peculiarly safe, speedy, and effectual. This is merely the application of blisters over the whole extent of enlargement, followed by the use of simple lotions and gentle pressure, while the hydriodate of potass is administered internally.

At no very distant period, this form of ulcer being attributed to the direct effect of syphilis, was considered a warrant for repeated courses of mercury, and long-continued drenching with sarsaparilla, with benefit, it may have been, to the apothecary's pocket, but with what loss to the patient's health may be imagined, now that mercury is known to be the principal source of the evil it was employed to remedy. The local treatment, also, of those days was hurtful. In the first place, by opening, through means of incisions or caustic, abscesses of the periosteum, which readily admit of absorption, under the same management that proves useful after ulceration has taken place; and, secondly, by confounding the rough surface of bone so exposed with that most obstinate of diseases, caries. Under this erroneous impression, operations no less severe than unnecessary, were performed. I have seen the choffer bristling with cauteries carried into the operating theatre, while unfortunate shin-bones were rasped and chiselled, preparatory to the burning thought requisite for their remedy. I have also known cases which had resisted the prolonged horrors of these rough and frequently repeated proceedings, yield at once to the application of blisters.

But there is reason to fear that operations still more severe, and, if possible, still less warranted, have been performed on account of chronic swellings affecting the periosteum and bone, through the proposal of Sir Benjamin Brodie, to trepan the tibia for the discharge of matter pent up in its cancellated texture. The symptoms of this abscess, as described by that gentleman, are enlargement towards one extremity of the bone, pain more or less severe, usually remitting and recurring with increased intensity at variable intervals, induration and adhesion

of the integument to the periosteum, tenderness under pressure, especially at particular points; in short, as he remarks with regard to one of his cases, "all the symptoms of chronic periostitis." Now, if the two conditions are thus so similar, it is plain that the one which admits of remedy by the use of gentle means may be mistaken for the other, in which, it is alleged, nothing can afford relief except making an aperture by the trephine for the escape of matter. Several patients, accordingly, have come under my care, after being advised in London to allow their tibias to be trepanned, and recovered completely without undergoing any operation. Indeed, Sir Benjamin Brodie says, that, "even if you are mistaken in your diagnosis, no harm can arise from the operation. Nay, it is a question whether good may not arise, under certain circumstances, from taking away a piece of bone when it is affected with chronic inflammation, even though there be no abscess." And he illustrates this view by relating a case in which recovery followed the boring of a hole through the humerus in search of matter, without any being found. But, with all deference to an authority so justly respected, I must protest against the license thus afforded to practise an unnecessary, painful, and, as I believe experience would show, dangerous operation; especially as the affection, admitting of remedy by the use of gentle means, is so much more common than chronic abscess in the tibia. I have for many years been looking for this disease, but hitherto without success; and, though not at all disposed to question the reality of such an occurrence, I feel entitled to regard its absence from the field of observation submitted to me as a proof that it must be a rare event in the practice of surgery—Pp. 42-50.

DISARTICULATION OF THE CLAVICLE FROM THE STERNUM.

In the beginning of May last, I was asked by Dr Combe to see a gentleman in Leith, between thirty and forty years of age, who had, for about two years, suffered from a tumour of the clavicle. It was of an oval form, as if resulting from a general expansion of the bone, and extended from the sternal articulation to within a short distance of the acromion, possessed a very firm consistence, and was occasionally the seat of painful sensations. During the period of its existence, the patient had had occasion to travel round the world, and in his progress obtained many different opinions respecting the disease, with no less various advice as to its treatment. At the advanced stage when it came under my observation, the case seemed free from any obscurity either as to its nature or the course requisite for its remedy. There was obviously a morbid growth affecting the bone throughout its whole thickness, and admitting of removal only by excision of the clavicle.

On the 13th, I performed the operation in the presence of Dr Combe, with the assistance of Drs Duncan and Mackenzie. The patient being seated on a chair, an incision was made along the whole extent of the bone, and a second at its sternal extremity, extending upwards and downwards at right angles to it, in this form:—

Having reflected the flaps, I divided the muscular attachments, and ascertained the precise limit of the disease towards the acromion, which it so nearly reached, that the easiest way of proceeding would have been to separate the bones at their junction. But thinking that the object would be equally effectually attained by division of the clavicle, with less injury to the connexions of the shoulder, I sawed the bone through, so as to leave about an inch of its extremity. The clavicle was next forcibly pulled outwards by means of strong forceps, while its remaining attachments were carefully separated towards the sternum, until nothing remained but the ligaments of the joint, which I then divided so as to complete the disarticulation. Three small vessels were tied, and the wound was dressed as usual.

Union by the first intention took place through nearly the whole extent of incision, and the patient recovered with so little disturbance, either of a local or general kind, that he was able to leave his bed-room before the end of a week.

Complete cicatrization was delayed at the acromial extremity of the wound, by the separation of small particles from the bone, apparently in consequence of the injury it had received from the saw. But this did not prevent the patient, in the course of a few weeks, from proceeding to Wales, where he intended to reside, and whence he writes that the wound is quite healed, and the arm getting strong and useful. As I fully expected that the absence of the clavicle would have occasioned considerable deformity and want of power, it was an agreeable surprise to see, that hardly any observable alteration in appearance resulted from the operation.

I am not aware that disarticulation of the clavicle from the sternum has hitherto been practised in this country, and therefore think it right to place the case just related upon record. The only part of the operation in the slightest degree difficult or embarrassing, was in separating the large articular surface of the bone from its connexions, where the vicinity of the pleura and venous trunk rendered extreme caution requisite.—Pp. 293–295.

ON THE CONTRACTILE OR IRRITABLE STRICTURE OF THE URETHRA.

Although the distinction between spasmodic and organic strictures, or, in other words, between the semblance and reality of contraction, has been long established in Surgical Pathology, the latter of these conditions was not supposed to vary except in degree and situation. The treatment, therefore, did not seem to require any diversity of procedure, and in this country most practitioners, preferring the plan of dilatation by bougies, employed them upon all occasions. But, however efficiently strictures of the urethra might in general be thus treated, no surgeon could employ the practice to any considerable extent, without encountering embarrassing cases that presented more than usual difficulty, or even baffled every effort to accomplish recovery. I do not here allude so much to the mere tightness of contraction, and difficulty consequently experienced in passing a small instrument through the stricture, as to the unyielding disposition manifested by the constricted canal, and its tendency to contract, perhaps even more closely than before, after being partially or completely dilated. One other feature of such obstinate cases of great importance to notice is, the strong and general sympathy of the system with every change taking place in the local disease; whence rigors and febrile attacks, leading to various derangements in different parts of the body more or less intimately connected with the part originally affected, are apt to result from attempts even of the most gentle kind to pass instruments into the bladder. Some constitutional disturbance, as that which occasions arthritic symptoms, would sometimes appear to be the cause of this particular state of stricture; and a local irritation, such as that of urinary concretions, is certainly adequate to produce the same effect, since all the features of excessive obstinacy and irritability are occasionally presented by stricture, in patients suffering from stone, and disappear at once so soon as it is removed. But, independently of either the one or the other of these influences, the peculiar condition of stricture to which I wish to direct attention, may exist in its most perfect form, and is then found to constitute one of the most vexatious subjects of treatment so long as it is combated by the means in ordinary use. The patient, in vain expectation of relief, is apt to require in succession the assistance of many different practitioners, each of whom, supposing that the previous want of success has depended upon deficiency of skill or care, proceeds to a repetition of the dilating process, destined to afford only a similar disappointment, or the more serious consequences already mentioned as not unusual under such circumstances. The following case presents a good illustration of this obstinate stricture. It led me to adopt the mode of treatment which I am now desirous of recommending, and will probably prove more impressive if allowed to stand alone, than if associated with other instances of the operation. I have repeatedly performed it with perfect success, and never with any unpleasant consequences; so that instead of dreading, as formerly, to meet with the form of stricture in question, I now undertake its charge with the confidence of a satisfactory issue; and, while doing so, reflect with much regret

upon the suffering that it would have been in my power to relieve, if this plan of treatment had occurred to me at an earlier period.—Pp. 319–321.

Here follows a remarkably interesting case, where graduated bougies, and repeated divisions of the stricture within, caused no benefit, but which was at length cured by dividing it from without.

The last extract we shall give is a case, which, with the author, we believe to be unique in the annals of surgery.

CASE OF RECOVERY AFTER RUPTURE OF THE URINARY BLADDER BY EXTERNAL VIOLENCE.

On the evening of the 5th of July, I was requested, by Messrs Joseph and Benjamin Bell, to see with them a young gentleman who seemed to have had his bladder ruptured. Circumstances prevented me from meeting these gentlemen until midnight, when I learned from them that the patient, a stout youth, seventeen years of age, after dining with his family, had gone out to take a walk, in the course of which he had encountered a low paling, about two feet high, and attempted carelessly to leap over it, but, instead of doing so, had fallen forwards, so as to strike the lower part of his belly with great force on the points of two upright spars of wood. He immediately complained of intense pain, and of a feeling as if his bowels had protruded; his brother, who had accompanied him, at the same time remarking that his clothes were distended over the belly. With the aid of support on each side, he then accomplished a few steps, so as to reach a carriage that had been brought as near as possible to the place where he was lying. A catheter had been introduced, and, after drawing off four ounces of bloody urine, allowed to remain in the bladder. We found all the usual signs of ruptured bladder; there being great pain and distension of the belly, with a sunk, anxious look. Twenty leeches and hot fomentations were applied. The catheter was taken out, and an opiate prescribed.

On the following day the abdominal pains and swelling were increased, there being dulness on percussion below the umbilicus, and more than ordinary resonance above it. The catheter was introduced with the effect of withdrawing a few ounces of bloody urine. In the evening, leeches were again applied, and an opiate prescribed.

Next day, the 6th, he was found to have passed a restless night. There was some confusion of ideas, and considerable impatience for a change of posture, &c. The abdominal swelling had increased, and there was some cedema of the posterior parts from the chest down to the thighs. The catheter was introduced twice, and each time drew off a quantity of bloody urine, similar to that which had been obtained on the former occasion.

On the 8th he was in much the same state, and not sinking, as we had had fully expected to find him; but the whole trunk was fearfully swollen, and his respiration was performed as if only a small portion of the lungs had room to act. The cedematous effusion had greatly increased at the lateral and lower parts, while the tympanitic condition was still more manifest anteriorly and superiorly. Below the umbilicus there was not only complete dulness on percussion, but obscure fluctuation, which, after careful deliberation, induced us to think that an incision in the linea alba, a little way above the pubis, might be of use. After cutting through a thick mass of condensed texture at this part, I saw a stream of clear fluid begin to trickle out, and, wishing to see the effect of what had been done before proceeding further, we ordered a large soft sponge squeezed out of warm water to be applied so long as the fluid should continue to escape. In the evening it was ascertained that a very large quantity of urine had passed from the wound, and the abdominal swelling was considerably reduced, while in all other respects the patient appeared to have experienced relief. The catheter had been introduced twice during the day without obtaining a drop of water, although nearly a tumblerful was taken off by it in the morning before the incision.

On the 9th the pulse had fallen to 100. The abdomen was greatly collapsed,

and every thing seemed favourable, and he continued in this satisfactory state until the following evening, when it was observed that the urine ceased to come freely away, and he became restless, with a return in some degree of his former unpleasant symptoms.

On the 11th there was considerable swelling of the lower part of the belly, with quick pulse and foul tongue. The wound looked dry and white, as in a patient after lithotomy who is suffering from inflammation at the neck of the bladder. We entertained serious apprehensions of the sequel, but thought it right to enlarge the aperture lest there should be any obstruction to the flow of urine. I effected this by means of a bistoury, so freely, that my finger could be introduced down to the muscles, which were found separated from each other in the mesial plane to the extent of an inch and half, so as to form a narrow slit through which the water passed.

On the 12th the belly was collapsed. The pulse had fallen to 80, and the patient was quite easy.

No particular change occurred until the 19th, when a considerable quantity of sloughy cellular substance was extracted from the wound; and on the 21st, in consequence of there being a rather copious discharge of matter from the cavity lying between the integuments and muscles of the abdomen, a free counter opening was made on each side, as low in the flank as the point of a catheter could be pushed. A great improvement speedily followed the establishment of these drains so far as the superficial parts were concerned, although there was still a copious issue of thick matter from the aperture between the muscles.

On the 26th, seeing a slough at the orifice, I seized it with dressing forceps, and gradually pulled out a bag bearing no small resemblance to the bladder, which was found to consist of dead cellular substance, lined with a white deposit from the urine. I then passed my finger down into the bladder and felt a rent more than an inch long in the anterior part of the fundus without, or on the pubal side of the reflection of the peritoneum.

On the 5th of August the patient very unexpectedly passed seven ounces of water by the urethra, though the catheter had been frequently introduced before without obtaining so much as a tea-spoonful, ever since the opening above the pubis was made. In the course of a fortnight the patient was in every respect perfectly well, and quickly regained his strength without the slightest trace of inconvenience from the injury.

As there is not upon record, so far as I know, any well authenticated instance of recovery from rupture of the urinary bladder by violence, this case may be regarded as of some interest, and also, perhaps, prove of practical use on such occasions, by suggesting the possibility of affording relief through local treatment. Mr Benjamin Bell informs me, that while he was a resident pupil in Bartholomew's hospital, it was found, on examining the body of a child, that in addition to other injuries of a mortal kind caused by the wheel of a carriage, the bladder was ruptured precisely in the same situation as in the patient whose case has just been related. If the rupture takes place above or within the reflection of the peritoneum, there cannot be the slightest chance of escape. But if the rent is at the anterior part, so as to discharge the contents of the bladder by a sudden gush into the cellular substance, and condense it in such a way that merely the portion in contact with the urine is deprived of life, it appears that the patient may be saved by timely incisions.—Pp. 332–336.

From the specimens now given of four articles, our readers may judge of the value of the other twenty-seven, which include treatises on excision of the elbow and amputation of the ankle joints, two most important operations with which the author's name is inseparably connected.

We cannot conclude our notice of this book, without complimenting our worthy publishers on the simple and elegant style of its produc-

tion. The paper and print are admirable, and the illustrations will bear comparison with the best productions of the London press.

On the Causes and Treatment of Abortion and Sterility. By JAMES WHITEHEAD, F.R.C.S., Surgeon to the Manchester and Salford Lying-in Hospital. Pp. 426. London: 1847.

MR WHITEHEAD'S work embraces a great deal more than the title of it would lead us to expect. Of his ten chapters, the four last alone are taken up with "the Causes and Treatment of Sterility and Abortion." His first chapter treats of the physiology of menstruation, and the nature of the vaginal and uterine mucus. In reference to the periodicity of menstruation, Mr W. found, that "of 520 females 359 had always menstruated regularly, no particular difference being observable, either in the length of the interval between one period and another, or in the number of days during which the menses continued to flow. In about one-tenth of the number of cases last mentioned, which may be termed regular, the menstrual periods recurred at intervals, varying from twenty to twenty-seven days, calculating always from the day of commencement of one period to that of the next following; but in the majority, the interval was a lunar month." We have also, in the following quotation, some remarkable instances of irregular periodicity of menstruation:—

Of the remaining one hundred and sixty-one cases, which may be called *irregular*; in fifty-one, menstruation recurred every lunar month, but every third or fourth return a difference of three or more days was observed in the duration of the discharge, and often a difference, also, in the quantity thrown off in a given time; thirty-eight menstruated every lunar month generally, but every third or fourth time from four to seven days earlier; and these deviations were so marked and constant in most instances as to be anticipated at particular times: the amount of excreted fluid was also variable in these cases; fifteen had the menses every three weeks generally, but every third or fourth return from four to seven days later; fourteen every twenty-four days, but occasionally the interval was twenty-eight days; five every five or six weeks, but having occasionally an interval of only a month; two every eighteen days, of whom one had the discharge four days and a free interval of only fourteen—the interval now and then, however, being two or three days longer, and the duration of the discharge correspondingly abbreviated; the other having every third or fourth time an interval of a month. One menstruated every lunar month, but at the middle of every third interval she had an additional discharge every way similar to the catamenial, which continued thirty or forty hours, making no perceptible difference in the regular periods. Two menstruated every fourteen days, in one of whom the discharge generally continued seven days, leaving a free interval of seven,—but occasionally the interval was ten days, and the active period only three or four; in the other case the discharge generally continued two days, but sometimes only a few hours. One individual had the menses monthly, but every third time she missed the period, having a free interval of two months, her health never suffering in consequence. And thirty-two menstruated so irregularly as to afford no means of calculating the periods of recurrence or their duration with any degree of exactness.—Pp. 8-9.

Our author further states, that "persons of the bilious or lymphatic-bilious temperament have the menses in greatest abundance, and continued for the longest period; those of the sanguine temperament, the contrary; this may be, in some measure, owing to the free cutaneous transpiration generally observed in the last-named class of persons. In lymphatic subjects the discharge is usually abundant, but varies considerably, and is doubtless influenced materially by the amount of leucorrhœal discharge voided in the interval, to which affection this temperament predisposes more, perhaps, than any other."

"The mucus of the vagina," says Mr W., "in its normal state, always exhibits acid properties, that of the uterus is as constantly alkaline." He has never found the vaginal mucus alkaline unless where there was purulent secretion, nor has he ever found the uterine discharges other than alkaline, except where they were "of an ichorous nature;" ichorous discharges are subsequently explained as those resulting from some putrefactive process, as, for instance, from the retention in utero of a clot of blood, or a portion of placenta or ovum. This vaginal acid slightly coagulates the uterine mucus, unless when it (the mucus) is unusually abundant; and "it is the mixture of this acid product with the true catamenial fluid in its transit outwards, which gives to the latter its peculiar properties by which it is generally said to be recognised."

By "true catamenial fluid" the author designates the catamenial discharge collected directly from the os uteri by means of a speculum, without being permitted to come in contact with the vaginal mucous membrane. Mr W. collected in this way, for examination, about a dozen different specimens; and from this series of observations he concludes, 1st, that the "true catamenial fluid" is not so dark in colour as the ordinary menses; 2dly, that it is more viscid than systemic blood; 3dly, that it invariably coagulates; 4thly, that it most frequently escapes from the os uteri, partly in form of a thin coloured serum, and partly in flattened clots of the size of small orange seeds, which, soon after their escape, become broken down and dissolved as it were in the vaginal mucus, escaping at the *ostium externum* in the usual uncoagulable fluid form; 5thly, that it is invariably alkaline. Our author therefore considers that the true menstrual blood is extremely like that in the capillaries, and that the quantity of fibrine which it contains is not different from that which obtains in the circulating mass in its healthy condition; in short, the menses are not a secretion but an exudation of pure blood, the coagula of which are dissolved (unless in very great abundance) in their passage through the vagina, by a solvent power existing in the acid of the vaginal mucus.

Mr Whitehead has related several experiments which he performed in corroboration of the above. He seems to have paid much attention to the subject, and to have taken great pains to inform himself of the exact nature of the menstrual, uterine, and vaginal

discharges. We have no doubt that he believes himself to be quite original in his experiments; but he has been anticipated several years ago by MM. Brierre de Boismont, Denis, and Bouchardat.¹ Indeed there is such a very close resemblance, in many points, between Mr Whitehead and M. B. de Boismont, that one might almost imagine Mr W. to have, in some parts, translated his French *collaborateur's* work; this, however, we are assured our author would not have done without acknowledgment. M. de Boismont adopted the same method of obtaining the true menstrual fluid as Mr Whitehead—viz. by means of the speculum. Our author speaks of the irksomeness of this procedure; but M. de Boismont actually kept the speculum inserted in the vagina of a patient for ten hours, and thus collected for analysis about 330 grains of true menstrual fluid. We have not mentioned this to detract from Mr Whitehead, far from it; he merits the same acknowledgment of his labour as if his experiments had been original.

Our author corroborates by his observations the fact, "that the menstrual blood, under normal circumstances, proceeds entirely from the inner surface of the uterus, and that every part of the organ, including the upper portion of the cervix, is perhaps equally engaged in the performance of the function." In proof of this, Mr W. details a case wherein a female had a fatal attack of menorrhagia, and in whom, on opening the body, the uterus was found to contain a clot the exact mould of its cavity; "its inner surface presented numerous openings scattered over every part of it, obvious to the naked eye, some being sufficiently large to admit a good-sized bristle, or the end of a lachrymal probe, the largest and most numerous were at each side of the fundus near the horns of the uterus, and at the contracted part of its body near the commencement of the cervix. The openings had a valvular arrangement, a great number passing downwards towards the cervix, while those at the upper part of the organ appeared to pass towards the Fallopian tubes." The menses therefore, are, according to Mr W., "separated from the circulating current by simple exudation from the arterial capillaries in communication with the valvular orifices naturally existing upon the inner surface of the uterus."

Our author next treats of what he designates "spurious menstruation," *i. e.* a state of plethora of the uterus and surrounding organs, accompanied by many constitutional symptoms resembling those of utero-gestation, so similar that, in spite of the presence of a periodical bloody discharge *per vaginam*, it was frequently impossible to convince patients that they were not pregnant. In the three cases detailed in the work before us, there was also ulceration on the labia uteri; in fact, Mr W. maintains that this affection, viz., "spurious menstruation," is invariably associated with a morbid state of the parts situated external to the uterine cavity, generally of its cervix

¹ Brierre de Boismont, de la Menstruation, p. 171; Jacquemier, Manuel des Accouch., tom. i. p. 133.

and labia, sometimes of a portion of the vaginal mucous membrane ; and further, he seems nearly convinced that the menstrual discharge in these cases proceeds from the diseased surfaces alone, and not from the cavity of the uterus.

Mr Whitehead's second chapter is intended to treat of puberty. Where he confines himself to that subject his remarks are interesting ; but into this chapter he contrives to thrust some twenty pages of unnecessary matter about scrofula, syphilis, and the syphilitic origin of scrofula ; and as it is full of loose and fallacious reasoning, and not likely to conduce to edification, we shall dismiss it without further notice. We may however remark, *en passant*, that his investigations show that factory labour is by no means so injurious to the health of females as is generally imagined ; and that, in spite of what is said as to the effects of the high temperature of the factory rooms, menstruation does not commence so early in the girls employed in them as in females engaged in other kinds of occupation, and in those of the better classes ; the latter, in fact, suffer more from menstrual retardation. In 2127 females employed in factories, the average age at which the menses appeared was about fifteen years and eight months, and of these 24 per cent. suffered under disease consequent upon amenorrhœa ; in 1873 domestic and farm servants, sempstresses, shopkeepers, and educated females, the average age at which menstruation began was fifteen years and five months, and 20 per cent. suffered from retardation.

Our author's chapter on the "Diseases of Menstruation" is exceedingly meagre and uninformative, and his information with regard to membranous dysmenorrhœa is very antique and far behind. In Chapter IV., on the "Last Menstrual Crisis," we have a table of sixty-nine cases, showing the age at which menstruation ceased in each ; the average age of the last menstrual crisis, as deduced from this table, is a little above forty-seven years. The diseases which are most commonly met with at this period, Mr W. classes under three heads. "The *first*, and most common of these affections, is characterised by a muco-purulent discharge from the vagina, generally denominated leucorrhœa, but differing from this in several important particulars ; the *second*, is often accompanied by vaginal hemorrhage ; and the *third*, is characterised by a watery, sanious, serous, or ichorous discharge, sometimes mixed with blood, pus, mucus, or albumen-like shreds, and occasionally with small portions of fleshy matter." "This arrangement," we are told, "is not intended to include all the diseases of which the uterine system is susceptible ; but such only as are of daily occurrence, and which, implicating the lower extremity of the uterus or vagina, are capable of being brought within the sphere of ocular investigation." Our author continues :—

An individual, who has been the subject of leucorrhœal discharges during the period of child-bearing, and in whom the affection remains uncured, is pre-eminently susceptible of disease at the crisis of cessation ; the change in her is, gene-

rally speaking, earlier attempted, but occupies a much longer period, and is surmounted with greater difficulty than is encountered by others more favourably circumstanced in this respect. The last pregnancy in such a person is usually accomplished with great suffering, and not unfrequently has an abortive termination.—P. 156.

In such cases, there exists great liability to post partum hemorrhage; the secretion of milk is scanty and innutritious, and recovery is protracted for a long time, and incomplete. In females thus affected, the uterus is, in all instances, in a state of disease. The lesion consists in hypertrophy of the cervix, with granular ulceration or induration of one or both labia. Occasionally the labia are but little, or not at all diseased; but then the margin of the os presents a ring of intense redness. This is a sure indication of inflammation of the lining membrane of the uterus, or *endo-uteritis*. The treatment to be adopted in these cases consists of leeching, and the local application of nitrate of silver. In cases of *endo-uteritis*, our author recommends the injection, into the uterus, of a solution of nitrate of silver, with extract *conii* added. We have seen much benefit derived from opiate injections, coupled with mercurials, exhibited so as to get the patient as quickly as possible under their influence.

The *second* kind of disease mentioned by our author, we regard as nothing else than chronic metritis, with or without ulceration of the cervix. Mr W. details a fatal case of this variety, where he thinks it "highly probable that, had scarification of the cervix, or bleeding with leeches, from the same part, been practised in the first instance, permanent good might have been effected. The procedure was not adopted in this instance, from a groundless dread of interfering injuriously with the process of utero-gestation," the patient being at the time pregnant. We regard this as a practice fraught with danger to the fœtus. We have known more than one instance where the application of leeches to the os uteri of a pregnant female was speedily followed by abortion.

The *third* variety of disease is that accompanied by fœtid, or, as our author terms them, "ichorous" discharges. His third variety, in short, embraces the malignant diseases of the uterus. He very properly remarks, that "fœtid discharges from the vagina, when not occasioned by the process of decay which retained portions of the placenta, or of the membranes of the ovum, are wont to undergo, are justly regarded as indicative of serious organic mischief. When they occur for the first time, at a late period of life, especially if the menstrual functions have ceased completely to be performed, the existence of malignant degeneration may, for the most part, be confidently inferred."

We have next a very long chapter on the "Signs of Pregnancy." In the course of it, Mr W. takes occasion to recommend the propriety of sexual intercourse during menstruation, in order to insure fecundation. He states, that however repulsive this may be in a moral point of view, it is by no means unreasonable, and that the danger from copulation during this period is, in a great measure, assumed

and fanciful. "This statement," he says, "is founded on no small number of inquiries." His advice he bases, first, on the analogy alleged to exist between an animal in heat and a menstruating female; and, secondly, because it is asserted that the organs are in the most favourable condition for conception a *few days before*, or *immediately after* menstruation. Therefore our author recommends copulation *during* the catamenial flow! Our limits will not permit us to expose the absurdity of the former¹ of these assumptions, nor the incorrectness of the latter; that fecundation generally takes place before, or during menstruation, is denied by every practical accoucheur, and supported only by one or two speculative physiologists. We dare to affirm, as far as our reading extends (and that is not very limited), that Mr Whitehead is the only author who ventures to prescribe such a practice. We find no fault with it in a moral point of view, for we cannot see what morality has to do with the matter; but we consider it dangerous and disgusting with reference to health, cleanliness, and decorum.

Our author very properly attaches considerable importance to the changes which take place in the os uteri as indications of pregnancy. He says, "at six or eight weeks it becomes decidedly oval, or irregularly circular, with a puckered or indented boundary, having a relaxed and lobulated character." He makes no exceptions, and appears to forget that this description does not at all apply to the os uteri in primiparæ. In them, as well described by Chailly,² the transverse orifice of the womb becomes circular. "It is regular in its contour, and closed; the os tincæ is smooth and polished." There is no puckering or indentation in them. In speaking of the auscultatory evidence of pregnancy, our author encourages the use of the vaginal stethoscope. We are not one of those who exclaim against instrumental interference, *per vaginam*, where necessary; indeed, we would gladly see the speculum even more employed than it at present is; but we certainly look upon the use of the stethoscope, *per vaginam*, as unnecessary, and therefore grossly indelicate. His remarks upon the placental souffle are full of error, simply because there is no such thing as a placental sound in existence.³

We have next a series of cases to prove that a discharge, so like the menses as to be undistinguishable from them, frequently takes place periodically during pregnancy, forming what our author designates cases of spurious menstruation. Our limits will not permit us to do more than extract the following:—

On examination with the speculum, inflammation or ulceration of one or both labia, or of the cervix uteri, complicated, in some instances, with warty excrescences growing from the cervix, or from some part of the vaginal membrane, vaginitis, &c., was met with in every case, without an exception. Fifteen cases were

¹ *Vide*, for further information on this curious subject, Burdach's *Physiologie*, Bd. 1, S. 221; and Busch's *Geschlechtsleben des Weibes*, Bd. 1, S. 170.

² Chailly's *Midwifery*, translated by Bedford, p. 31.

³ *Vide* Naegele's *Geburtshulffiche Auscultation*, S. 21; and Depaul, *Traité d'Auscultation Obstetrique*.

submitted to this kind of examination at the time the blood was flowing. In not one of these did any fluid whatever escape from the interior of the uterus; the orifice being completely occupied at the time by a plug of transparent mucus. On removing the accumulated secretion by means of a piece of lint, the parts were immediately afterwards covered by a coating of blood, which was distinctly seen issuing from innumerable pores on every part of the diseased surfaces, and soon being in sufficient quantity to trickle down into the speculum. This blood was widely different, in its sensible properties, from that collected in the tube during its introduction, or at the os externum; being more florid, more strongly alkaline, and soon subsiding into a dryish clot, which could be separated from the interior of the instrument in form of a small cake of crassamentum. This was never the case with the former, which remained fluid or soft for a considerable time.

The evidence now produced appears sufficient to establish as a general rule, to which I am not as yet acquainted with an exception, that the blood discharged in cases of alleged menstruation during pregnancy, is furnished, not by the lining membrane of the uterus, nor by any healthy secreting surface—except sometimes perhaps the inferior part of the inner cervix; but by the lower extremity of the uterus external to its cavity, or by the contiguous vaginal reflection, being in a state of suppurative inflammation.—Pp. 222-223.

We have next a series of cases of absence of menses before pregnancy, thus showing that the presence or absence of a catamenial discharge is no unequivocal evidence for or against the existence of pregnancy. Under the head of causes of abortion, Mr Whitehead treats of nearly all the diseases of the uterus. In reference to the frequency of leucorrhœa, he observes, that out of 2000 females, 1116 were affected with this disease. He further remarks, that the virulence of the discharge is such, that it may produce blenorragia in the male, and its presence in the mother he regards as almost the sole cause of the purulent ophthalmia of infants. Gonorrhœa is stated to be much more frequently an affection of the uterus than of the vagina, the vaginal membrane being, in some measure, protected by its own mucus. Gonorrhœal inoculation induces superficial inflammation of one or both labia uteri, which is said to be very liable to extend into the cavity of the organ, and there produce endo-uteritis. From much that Mr W. says regarding this gonorrhœal inflammation of the uterus, we must, as yet, withhold our assent. It seems, in a great measure, drawn from fancy, and devoid of proof. His treatment of these diseases contains nothing new. He employs, every third or fourth day, solutions of the metallic astringents—viz. nitrate of silver, and sulphates of copper or zinc—and, in the intermediate days, tincture of matico where there is hemorrhage, or solutions of opium or tannin. These must be applied directly to the part, by means of a dossil of lint; injections being properly regarded as almost useless. One cause of abortion, and certainly not the least important—viz. disease of the placenta—is not at all mentioned by our author. Considerable advance has been recently made in the treatment of placental disease; and its importance surely demanded that some notice should be taken of it in a work professing to treat of “the Causes and Treatment of Abortion.”

The chapter on “Sterility” is little more than a reiteration of what

has gone before ; for the same diseases that cause abortion will also prevent conception.

Upon the whole, the work before us contains much that is valuable, although greatly obscured by a quantity of unnecessary, extraneous matter. Still, any one who will take the trouble, as we have done, to wade through the sand, will not fail to find a pebble or two of sufficient value to reward his labour.

A Treatise on Diet and Regimen. By WILLIAM HENRY ROBERTSON, M.D.; Physician to the Buxton Bath Charity. Fourth Edition, Re-written and much Enlarged ; in two volumes, 12mo,—Vol. I. London : 1847.

THE portion of Dr Robertson's work before us, consisting of three numbers, is the first volume of the fourth edition. As it has been wholly re-written, and the view given of the subject brought up with the current of science, it may be regarded as a new work. We have read this first volume, all that is as yet published, with much satisfaction. It deserves to be characterised as a very sensible book ; plainly the production of a man of much intelligence, of extensive reading, and of large experience in his profession.

The following passage from the author's preface sums up briefly the subjects of his work :—

The present edition of this work is prefaced by an introductory chapter on the origin, &c., of sporadic, endemic, and epidemic disease,—the influence of civilisation on human health, and the expectation of life,—the importance of information on the physical necessities of health, and the principal sources of disease,—and the evils which arise from the general ignorance of the public on sanatory questions, and from quackery, which is the child of such ignorance. The body of the work contains chapters on diet,—ventilation, climate, and change of air,—the hygienic effects of clothing, &c., especially with reference to practical inferences,—the physiological effect of water on the system, when applied to the skin, and when taken into the stomach,—mineral waters,—sleep,—influence on health and life-expectancy of the combined physical causes, with reference to employment, town or country residence, &c.,—and on the effects of mental culture on health.—P. viii.

The portion of the work before us, however, does not extend beyond the subject of Diet. The introductory chapter is a very sensible and well-timed discourse, bearing particularly on the necessity of active sanatory measures for the preservation of the public health. From this chapter, we quote the following passage as a specimen of our author's mode of treating his subject :—

On all these forms of disease, civilisation,—the drainage and cultivation of the soil,—adequate sewerage,—commodiously constructed and well ventilated streets, houses, and apartments,—have exercised, and may exercise, so large an influence as to mitigate, or possibly altogether do away with, endemic disease ; and to modify and greatly lessen the fatality of epidemic disease. These are the social gains accruing to every individual of a well-organised community, the degree of which can scarcely be estimated ; and which, however imperfectly and crudely conceived, might well reconcile us to the times we live in, and atone to

us for not having lived in "those good old days," when agues, malignant fever, plague, putrid sore-throat, and confluent small-pox, thinned, at short intervals, the population of the towns, villages, and hamlets of Old England. Civilisation, however, with all its great improvements, and the large boons it has conferred on the mass of the people, has dealt less mercifully with individuals. Shutting up so many in the close and less pure air of our large cities, and such numbers in the closer air of workshops, cellars, and small rooms,—confining so many to a single occupation, involving, perhaps, almost a single position of the body, day by day, and year by year,—making life a succession of privations, confinement, and mechanical and unvarying employment,—civilisation deals out to these her victims, a large amount of individual disease and suffering, which never reach the social importance of endemic or epidemic disease, but which sicken the life and shorten it, and affect injuriously the health and the probability of life of the posterity,—probably to destroy the individual and his descendants in the course of a few suffering generations, by forms of disease characterised in all their phases by diminished power, and called cachectic, having the generic name of scrofula common to the whole of them. And yet there is no doubt that much of this individual penalty, which is paid by social man for his civilized position, is no necessary part of the civilized state,—that, although large numbers of people cannot be brought together into one building, where they must spend a large part of every twenty-four hours, without some degree of physical injury to them all,—that, although individual health must be affected even by the crowding of large masses of people into the limited district of a closely built town,—yet, by sewerage, by cleanliness, by ventilation, a great amount of the evil may be done away with, and the expectation of life, although not equal to the highest standard attainable in a well-drained rural district, be greatly increased from that which obtains under less favourable circumstances.—Pp. 17-19.

In the part of the volume devoted to Diet, he has given a sketch of the function of Digestion as complete, perhaps, as the limits of the work would admit of. On the comparative digestibility of different kinds of foods, he dwells at considerable length, and treats this difficult part of the subject with much discrimination. After giving Dr Beaumont's table, drawn up from his observations on Alexis St Martin, and the result of the experiments of other authorities, he proceeds to arrange the several kinds of food in tables according to their digestibility, as determined by his own experience. These tables he prefaces with the following sensible remarks:—

In the question of the comparative digestibility of the different articles of food, we are then reduced to the necessity of finding general rules for our guidance in individual cases, from wide and careful observations of the digestibility of the different kinds of food, in health and disease; by no means conceiving such observations, however carefully made, to be infallible; but bearing ever in mind the probability, or at least the possibility, of frequent exceptions occurring, to any one of the generalisations at which we may have arrived. In fact, all attempts to frame such general rules are liable to strictures, on the ground of so many and signal exceptions to the rule being frequently met with. But, notwithstanding such strictures, dietetic observations are of the greatest use as guides; although they may not be found worthy of implicit trust, in every detail of every case that may occur. To prove that such observations must have much value, it is enough to show, from general experience, and universal admission, that some articles of food are almost invariably found to be less likely to agree with the dyspeptic or debilitated stomach than others are: for instance, that in nearly all such cases, mutton agrees better than lamb, or veal, or pork; that fat, as has been said, agrees better than lean,—starch than sugar,—eggs than cheese. In regard to the attempt to arrange the different kinds of food in tabular order, with reference to their digestibility, it may now claim, after a trial of eleven years, to

have undergone the test of time, and to have been proved to be useful; although it is admitted to be by no means an unvarying and certain guide.—Pp. 79, 80.

This table of meats indicates the following as the order of their digestibility:—1. Mutton; 2. Beef; 3. Lamb; 4. Veal; 5. Pork. That of Poultry runs thus:—1. Fowl; 2. Turkey; 3. Duck; 4. Goose. That of Game, as follows:—1. Hare, hunted; 2. Partridge; 3. Pheasant; 4. Venison; 5. Grouse; 6. Ptarmigan, Blackcock; 7. Hare; 8. Pigeon, Lark, &c.; 9. Rabbit; 10. Woodcock, Snipe, &c. The animal principles stand in the following order:—1. Gravy; 2. Gelatin; 3. Fibrin; 4. Fatty matters. He explains that gravy here includes beef-tea, made very strong from lean beef, with the fat removed. Of Fish, he puts the digestibility as follows:—1. Whiting; 2. Haddock; 3. Cod; 4. Sole and Flounder; 5. Lobster and Crab; 6. Oyster, raw; 7. Fresh-water Fish in general, as Trout, &c.; 8. Turbot; 9. Salmon; 10. Mackerel; 11. Oyster, cooked; 12. Herring and Sprat. The several forms of milk are placed in this order:—1. Whey; 2. Milk, skimmed; 3. Milk, unskimmed; 4. Cream; 5. Curd; 6. Butter; 7. Cheese; 8. Cream-cheese.

The different grains, and the like, he arranges thus:—1. Wheat-flour; 2. Rice; 3. Rye-flour; 4. Maize-flour; 5. Oatmeal; 6. Barleymeal; 7. Peasmeal.

We can afford space only for another of these tables, namely:—1. Asparagus, Sea-kale, Celery, Vegetable Marrow, Artichoke; 2. Cauliflower, the heart; 3. French Bean; 4. Potato; 5. Spinach; 6. Turnip; 7. Cabbage, Greens; 8. Carrot; 9. Parsnip; 10. Pea; 11. Windsor Bean; 12. Mushroom.

Of each of the articles contained in these several tables, our author has given a brief account,—and in regard to drinks he has followed a nearly similar plan.

The volume concludes with a copious exhibition of Dietaries, drawn from many different sources, as bearing on the question as to the quantity of food most favourable to health. Altogether, the work is worth the attention of the public, as well as of the profession.

Part Third.

PERISCOPE.

PHYSIOLOGY.

ON CHLOROFORM AND ITS ANALOGOUS COMPOUNDS. By Dr GLOVER of Newcastle.

[SINCE chloroform has become so important a substance, every thing concerning its history is rendered interesting. We therefore give a short abstract of a series of laborious experiments made by Dr Glover of Newcastle, and published in 1842, in an essay which obtained the Harveian prize of that year. Very little was then known of chloroform, so that its physiological history may be said to commence with these remarks.]

Dr Glover commences his chapter on "the Physiological Properties of the Chloride and Bromide of Olefiant Gas, Chloroform, Bromoform, and Iodoform," by stating that his attention was first drawn to this class of bodies by the curious results of an experiment performed by Dr Cogswell with the iodoform, then termed sesqui-oxide of carbon (in Cogswell on Iodine, p. 122). This experiment led him to investigate the corresponding compound of bromine-bromoform; and as, in his paper on bromine, the analogies between bromine and its compounds, and the corresponding compounds of chlorine and iodine, are especially kept in view, he was induced to extend his researches to chloroform. The entire class appeared to him "to form a new class of poisons, and to be possessed of properties not unlikely to be beneficial in the treatment of disease." He then states the composition of these bodies, taking the compounds of chlorine as examples, $C_4 U_3 \text{ Chl}$ and $U \text{ Chl}$ for the chloride of olefiant gas, and $C_2 U \text{ Chl}_2$ for chloroform.

After the striking resemblance in leading physiological properties between the members of the group has been shown, chloroform is selected to elucidate these properties especially.

In experiment fifty-three, 30 minims of the chloride of olifiant gas were injected into the jugular of a small but active dog. The result was a series of curious phenomena connected with the respiration, owing probably to the congestion of the lung. After death, on inspecting the lung, it was found enormously congested. An ass was killed almost immediately, by injecting three measured drachms of the bromide of olefiant gas into the jugular. Here the irritability of the heart appeared to be destroyed, and the cavities of the organ filled with masses of coagulated blood. The lungs were enormously congested, the bronchi filled with frothy serum, and the smell of the compound every where perceptible throughout the chest. (In about thirty seconds after the substance entered the jugular, its smell was perceptible in the expired air). The chloride of olefiant gas (Dutch liquid) introduced into the stomach, and the bromide similarly tried, were found to produce remarkable symptoms of affection of the chest, which inspection proved to be due to congestion of the lung—a state of stupor and great loss of power over the voluntary muscles, with some convulsive movements of the limbs.

In experiments fifty-seven and fifty-eight, chloroform injected into the jugular killed a poodle dog, and a powerful animal between a greyhound and Newfoundland breed, within a minute. In the former case, a measured drachm was used, in the latter, two drachms. The symptoms were short dry cough, becoming husky before death, and a few convulsive movements of the limbs. On inspection, the heart was gorged, and its irritability destroyed. The chloroform introduced into the stomach of rabbits, was found to occasion similar symptoms to those produced by the chloride and bromide of olefiant gas.

In several more experiments with chloroform, it was found to congest the lungs greatly when thrown into the jugular; this was ascertained by the hæmadynamometer. It was found also to act on the spinal chord, and to cause great prostration and loss of power. The following experiment is worthy of being quoted (experiment sixty-fourth):—Sixty minims of chloroform were injected into the peritoneal cavity of a large male rabbit. Death ensued in seventeen minutes.

Symptoms.—Slight transitory excitement; loss of power over the limbs; profound coma; excessively dilated pupil; heart's action feeble; great excitement of the respiration, and dilatation of the chest; a few slight movements of the limbs, synchronous with the respiration. No motion could be excited by pricking or pinching the limbs; nor did the eyelids move on approaching an object to the eye.

Similar symptoms were observed in another experiment, when the chloroform was introduced into the stomach of a rabbit.

In the general remarks at the end of the chapter, Dr Glover describes the class of bodies designated in the heading of the chapter, and especially chloroform (which throughout has been taken by him as an example of the rest), to possess a "remarkable power of obstructing the pulmonic circulation, whether injected into the veins or introduced into the stomach," a peculiar action on the

spinal chord and on the brain, and a corrosive and irritant action on the stomach when introduced into the organ. Reaction on the spinal chord is afterwards stated to be, that this organ "loses its sensibility under the action of this class of poisons," which, according to the recent experiments of Flourens on the mode in which the inhalation of chloroform produces the anæsthetic effects, appears to be true, and perhaps to explain the anæsthetic phenomena.

Finally, Dr Glover ascribes the discoloration, and even blackening of the mucous membrane of the stomach, which some of the class produce when introduced into this organ, to the compound action of the corroding and irritant action of the substances, their "effects on the colouring matter of the blood, and that congestion of dark blood found in the stomach in certain cases of coma."

Some observations on the medicinal properties of chloroform follow.—*Edin. Med. and Surg. Journal.* October 1842.

ON THE CONTRACTILITY OR IRRITABILITY OF THE MUSCLES OF PARALYSED LIMBS. By ROBERT BENTLEY TODD, M.D., F.R.S.

DR MARSHALL HALL, in various memoirs, has promulgated the doctrine, that paralysis from cerebral lesion may be distinguished from that which results from disease of the spinal chord, or the muscular nerves arising from it, by the irritability of the paralysed muscles, which he considers to be increased in the former, and generally diminished in the latter case. This distinction Dr Hall has connected with the theoretical view, that the spinal chord is the source of muscular irritability, and that the ordinary actions of the brain serve to exhaust this irritability, which is therefore increased by accumulation, when the cerebral influence is abolished from disease.

Dr Todd believes the facts on which this theory is based to be fallacious, and has instituted various experiments on the muscles of hemiplegic patients, by the application of the galvanic stimulus, which lead to results opposed to those of Dr Marshall Hall, by showing that, in the greater number of cases, where the paralysis appears to be due to cerebral lesion, the irritability of the paralysed muscles is diminished or abolished. In the greater number of these cases, the lesion is of some standing, and there is considerable atrophy of the paralysed limbs; in some, however, this is not the case.

In a smaller number of instances, Dr Todd has found the galvanic current to excite, in the paralysed limbs, contractions more vigorous and decided than those of the sound side. These are mostly recent cases, in which the muscles retain their firmness, and even present some degree of rigidity.

In a third class of cases, there is no difference between the irritability of the sound and paralysed muscles.

To explain these different conditions, Dr Todd supposes that where the muscles are distinctly atrophied, their loss of irritability follows from this as a necessary result; but that, where this is not the case, the irritability of the muscles stands in a direct relation with the degree of development of the nervous force in the affected part. Where the cerebral lesion is such as to keep the nerves in a state of irritation, the muscles of the limb are more easily affected than natural by the galvanic stimulus. When the nervous force, on the contrary, is diminished or lost, the contractility of the muscles is similarly affected.

Dr Todd sums up the results of his observations in the following propositions:—

"1. The contractility, or irritability of the muscles of paralysed limbs, bears a direct relation to their state of nutrition.

"2. The excitability of the paralysed muscles to galvanism, varies with the condition of their nerves more than with that of the muscles themselves.

"3. In the majority of cases of cerebral palsy, the contractility or irritability of the paralysed muscles is less than those of the sound side, simply because their nutrition is impaired by want of exercise.

"4. No diagnostic mark, to distinguish between cerebral and spinal palsy, can be based on any difference in the irritability of the paralysed muscles; for

the muscles in spinal paralysis exhibit the same states as those in cerebral paralysis.

"5. The tendency of strychnine to affect the paralytic limbs before the healthy ones, is attributable to its being attracted in greater quantity to the seat of lesion in the brain than to the corresponding part on the other side.

"6. The manner in which the muscles of a paralysed limb respond to the galvanic stimulus, assists us in forming an opinion as to the state of its nerves; if they respond feebly, or not at all, the nerves are in a depressed state; if they respond vigorously, more so than the healthy muscle, the nerves are in a state of irritation."—*Transactions of Medico-Chirurgical Society of London*, vol. xxx.

ON THE SARCINA VENTRICULI. By Dr SCHLOSSBERGER of Tubingen.

THE effects of reagents on the sarcina are interesting, as showing that it possesses chemical characters distinct from most of the organic forms which present themselves to the microscopic observer. Dr Schlossberger has ascertained, by his own experiments, that cold and hot water, alcohol, ether, volatile and fixed oils, have little or no effect on this parasite, except occasionally causing slight shrinking in volume. Organic acids, and dilute mineral acids, have likewise no effect, even at the boiling temperature. Iodine tinges it with its own yellowish colour, but gives no indication of the presence of starch. Concentrated sulphuric acid causes it, after a time, to divide into sections, precisely as Goodsir and others have described its natural division. Nitric acid develops, in some instances, a yellowish colour; in others, produces no change. Caustic potash in solution decolorizes it immediately, and, after several minutes, causes it to split up into its ultimate divisions. Many of them, however, generally resist this reaction for a considerable time. Fermentation and putrefaction cause no change.

The only organic forms liable to occur in the stomach which present similar chemical reactions to the above, are the torulæ of fermentation, from which the sarcinæ are easily distinguished. Disintegrated muscular fibre, which Schlossberger himself once thought might be the cause of the appearance presented by this parasite, disappears immediately under caustic potash.

The result of numerous experiments, in which various substances were submitted to artificial digestion, and to the vinous, acetous, and putrefactive fermentations, is, that the sarcina has not been artificially produced by any of these methods.

The sarcina has hitherto been found only in adults, in the stomach and alimentary canal, after death; in vomited matters; and in the fæces during life (by Hasse and Schlossberger). An examination of cases tends to prove, that the symptoms attending its appearance are by no means uniform. It is even doubtful whether its appearance be necessarily connected with any appreciable symptoms. It has been found in the fæces where vomiting was not present; and Virchow asserts that he has found it in an abscess of the lungs, and in the stomach after death, where no symptom during life of dyspepsia had existed.—*Archiv für Physiolog. Heilkunde*, vi. 1847.

PATHOLOGY AND PRACTICE OF PHYSIC.

MERCURIAL OINTMENT IN VARIOLA.

A YOUNG man, aged from twenty-five to thirty years, was received into La Charité hospital, under M. Briquet, on the first day of the eruption. On the following morning its confluent nature was apparent, and the application of mercurial ointment, thickened with a little starch, to the face was commenced. A few days afterwards, the whole body of the patient was covered with a confluent eruption, yielding a foetid purulent discharge. The face, however, preserved its primitive character, the pustules having been rendered abortive, and the skin not inflamed wherever the ointment had been spread. The point of the nose, however, where it did not rest on account of its prominent form, was

the only point swollen and discoloured. Beyond the limit traced by the ointment near the hair, behind the ears, and at the neck, large suppurating pustules had formed, as elsewhere. This young man is now convalescent, and does not present any cicatrix or swelling of the face. The eyes and eyelids have completely escaped, whilst the rest of his body is covered with deep cicatrices, and he has still some small cutaneous secondary abscesses.—*Annales de Thérapeutique*, Novembre 1847.

The above case is an excellent example of the good effects of mercurial ointment in rendering variolous pustules abortive. We have lately treated a severe case in this manner with the best results, and can recommend it to our readers, as a valuable application for diminishing the pains and swelling experienced in the face of small-pox patients, and preventing the cicatrices which some, more especially women, so much dread.

SPONTANEOUS CURE OF A CANCEROUS AFFECTION. By JOH. HIELSCHER, M.D.,
Vienna.

A MAN, aged sixty years, a watch-maker by trade, had suffered for several years under an affection, which he called cramps in the stomach. He was greatly emaciated. The appetite was increased, although the food taken was immediately vomited. At irregular periods he experienced pains of a boring character in the epigastric region, which were relieved by vomiting, the rejected matters being sometimes green, more frequently resembling coffee, and often darker coloured. Cancer of the pylorus was diagnosed, and the treatment restricted to oleaginous remedies and regulation of the diet. On the 4th May 1846, he vomited in the course of a few minutes about six pounds of blood, which was frothy on the surface, appeared of a dark brown colour, and contained lumps of black coagula, and remains of the food. The patient was almost a skeleton in appearance, greatly exhausted, the brow covered with a clammy sweat, the extremities cold, the pulse small, and in the author's presence he fainted. It was ascertained that, in order to relieve constipation, he had been in the habit of taking drastic pills, which, however, had not produced the desired effect, but seemed to increase the vomiting. Vomiting of blood had first appeared two days previously, but had only become violent during the last few hours. Ice pills, lemonade, and applications of ice to the epigastric region, were ordered. He could not eat, and, supposing that death was approaching, he received the sacrament.

The remedies ordered checked the hematemeses, and the patient fell into a deep sopor, from which he was every now and then aroused by spasmodic pains in the stomach. In three hours the application of ice was discontinued, and a mixture of mucilage with gr. opii. ordered. Cool soup was allowed for diet, and the vomiting was found to have ceased. During the next four days the pains in the stomach diminished in violence, and occurred at longer intervals.

On the 11th of May he was allowed a little wine and solid food. The constipation continued, which could not be relieved by clysters, and a purgative was ordered. At this time he insisted on having a stewed pigeon prepared for him, some of which he swallowed very hastily. There followed most violent pain in the abdomen, which concentrated itself around the umbilicus. The patient cried out, and threw himself about the bed in agony. No vomiting occurred, and he stated that his stomach was free from pain. It was thought too dangerous to give an emetic, but in two hours and a half the pain disappeared. The next morning there was feverish pulse, increased temperature of the skin, and flushing of the cheeks. The pains were felt in the region of the bladder; the urine only passed in drops, and micturition was painful. The urine contained a sediment, and was strongly ammoniacal. The lower part of the abdomen was swollen, and tender on pressure. By means of clysters, and the introduction of the finger, a few hard fecal masses were removed from the rectum, and cold applications to the abdomen, and decoction of linseed internally ordered.

On the 10th of May, after suffering several hours' severe pain, and the exhibition of three enemata, a copious stool was procured. It consisted of between five and six pounds of a milky-white, purulent-looking matter, mixed with a considerable quantity of blood, but little fæces. There was found in it a somewhat solid mass, of roundish form, like a mushroom without a stalk, about two inches in diameter, and one inch in thickness. It consisted of a solid nucleus, about the size of a sixpence (kupfergroschen), on one side covered with a delicate crust, and on the other embedded in a greyish-yellow, loose, and somewhat glutinous mass. In a few hours another stool followed, in which a similar mass, of smaller size, was contained. Both masses were examined by Dr Lautner, and were declared to be cancerous.

For a few days there passed involuntarily, per anum, a purulent bloody fluid. From this time recovery commenced, all pain disappeared; the urine passed without difficulty,—instead of constipation, there was diarrhœa; all vomiting had ceased, and the appetite returned. Appropriate remedies checked the diarrhœa, food was gradually increased; in some weeks he rose from bed, and when last seen in September he was quite recovered.—*Oesterreich. Med. Wochenschrift*, 8 Mai 1847.

Whether in this very singular case the symptoms were attributable to a cancerous mass in the stomach, which separated, and was passed by stool (as it would seem the author wishes us to infer), is a circumstance that unfortunately was not proved. The masses ought to have been subjected to a microscopic examination, without which the determining them to be cancerous was altogether impossible. We are aware that Dr Lautner, as Professor Rokitanski's assistant, has great experience in the examination of morbid products; but we cannot allow his *dictum*, in a case like the above, to have any weight, unless we were assured that the minute structure of the tissue had been most carefully investigated.

CLINICAL NOTES TAKEN IN THE FRENCH HOSPITALS.

GENERAL PROPHYLAXIS.—In all severe fevers, and in all important acute diseases which present themselves during the premonitory stage, or are developed in the hospital itself, such as typhoid fever, variola, pneumonia, meningitis, &c., M. Guérard practises the same treatment. Its object is to diminish the excessive action and congestions of the arterial system, and thereby prevent the complications. Experience has proved to him, that on fulfilling this preliminary indication, he meets afterwards with only a simple affection, easy to treat and cure. The prophylaxis referred to, consists in administering in such cases one or two grammes (15 to 30 grs.) of sulphate of quinine in solution every twenty-four hours. This practice hitherto has been attended with the best results, and it does not prevent the subsequent ordinary treatment of the disease, according to its intensity and nature, when these become evident.

BRIGHT'S DISEASE.—Most persons affected with Bright's disease perish from some later inflammatory disease of the thorax (pleurisy, pericarditis), or of the abdomen (febrile dysentery). A case of albuminous nephritis, now in La Charité, which, so far as the kidneys are concerned, seems to be going on well, since the albumen in the urine has greatly diminished, and the anasarca completely disappeared under the influence of purgatives, has just been suddenly seized with violent dysentery, without appreciable cause. This is probably the ultimate affection.—*Rayer*.

SYPHILITIC URETHRITIS.—M. Cruvelhier professes, contrary to the well-known doctrine of M. Ricord, that the simple urethral contagious blenorrhagia, without chancres, can produce constitutional syphilis, and that the ioduret of potassium is not necessary to combat the symptoms called tertiary, the liquid of Van Swieten being generally sufficient, as with the secondary phenomena. A man at this moment in the wards of M. Cruvelhier, at La Charité, with several

exostoses and periostoses, and nocturnal pains, had been affected only, according to his own statement, with a simple discharge for several years. The use of Van Swieten's liquid produced a prompt amelioration in every respect. This amelioration is progressive, and there is every reason to hope that it will produce a complete cure.

TUBERCLE AND CANCER.—In the hospitals of Paris, the greatest mortality is owing to pulmonary tubercle and cancerous affections. Cancer is found with fearful frequency, in all its forms, in every region and in every tissue. It is an incurable disease, whether operated upon or not, and we are ignorant whether the most successful operation has ever retarded for a moment its natural termination. There is not even a general indication with respect to its treatment, and prescriptions are limited to opiates in every form, in the vain presumption that such remedies calm the pains. This disease is incomparably less frequent in the centre of Italy, but instead, there are continually found severe affections of the arterial trunk, especially spontaneous aneurisms, with general disease of the arteries, which often render operations inefficacious or insufficient.

METHOD OF DISCOVERING IODINE IN THE URINE. By M. RAYER.—Boil thirty-two ounces of urine down to one ounce. Soak in this residue a strip of starched paper, then rub over it a glass rod, previously dipped in nitric acid, which will produce a blue colour if there be iodine present. We have seen M. Rayer determine in this manner iodine in the urine of an individual, on whom injection of the tincture of iodine had been practised for an hydropic knee. The same process will serve to prove the presence of iodine in the *Eaux de Challes*. It is remarkable for its extreme simplicity and readiness of execution. When the iodine is in large proportion, it may be found even without previously evaporating the urine. The process is insufficient when iodine is in feeble quantity, as in the *Eau de Vichy*, for example.—*Annales de Thérapeutique*, Novembre 1847.

SURGERY.

ON THE UTILITY OF MICROSCOPIC OBSERVATIONS IN THE DIAGNOSIS OF CANCEROUS TUMOURS. By M. MARMY.

EVERY day furnishes new proofs of the importance of microscopic facts in diagnosis, and especially in the treatment of tumours, which, when mistaken, become mortal, or at least in-operable, from their true characters not being distinguishable by the naked eye. The following case is one of the most striking examples of this kind that can be recommended to the consideration of those who still contest the utility of the microscope in the practice of medicine.

An officer of artillery, aged thirty-eight years, of a robust constitution, consulted M. Marmy in January 1846, for an orchitis, caused, as he believed, by friction against the pommel of the saddle. The left testicle was of regular form, but a third larger than the other; the cord healthy; no syphilitic affection for a long time. An antiphlogistic treatment soon restored the organ to its normal size; but, towards the end of February, the patient having again entered the service, the disease re-appeared, and, although it gave way a second time to the same treatment, it again returned. This time, the treatment, assisted by the administration of the ioduret of mercury internally and externally, then the ioduret of potassium, of the oxide of gold, continued a long time, and lastly, of purgatives and diuretics, supported by a rigid diet, were all ineffectual.

On the 1st of December, the tumour, which now surpassed in volume that of two fists united, presented a slight fluctuation at its inferior part. He experienced temporary pains, sometimes in the scrotum, at others in the cord, which was healthy. His general health was good. Nothing indicated then the existence of a malignant degeneration. M. Sedillot having made an exploratory

puncture, which gave issue only to a drop of blood, removed with the canula some portions of the matter, which, when examined with the microscope, appeared to be clearly cancerous. Cancer cells, with all their characters well marked, were seen swimming in a granular blastema, mingled with numerous drops of oil. In consequence of this examination, the operation of castration was proposed and practised.

Three days after the operation, the base of the wound exhibited a tolerably large clot of blood, which in three days more appeared to be organized, and white lines circumscribing areolar spaces were seen on its surface. Whilst preparing to destroy the clot with the actual cautery the moment a cancerous reproduction should appear, M. Sedillot determined, in the first place, to assure himself of its nature. He removed a small portion of it, which he examined with the microscope, and determined that the appearance was caused by the presence of altered globules of blood, and pus corpuscles. In fifty days the patient was perfectly cured.

The form of the removed sarcocele resembled that of an hypertrophied kidney. On section, its tissue presented all the characters of encephaloma, consisting of yellowish-white masses, soft, pulpy, and sprinkled over with small bloody extravasations. On a microscopic examination, there were recognised the same elements as were observed after the exploratory puncture.

Here then was a tumour, which at first offered none of the signs indicative of cancer. Absence of lancinating pains, regularity of form, general constitution of the patient perfect, anterior existence of syphilitic affections, habitual equitation, and disappearance of the swelling by antiphlogistics. Is it surprising that, with such diagnostic symptoms, the true nature of the affection should be mistaken? A microscopic examination, however, determined the disease. But its utility was not limited to this point. A bloody clot, appearing under the form of a fungus, caused fears that the cancer was about to be reproduced; instruments were already prepared to destroy it, when the microscope saved the patient from this painful and useless operation.

It should be added that the mother of the patient died a short time after of a cancerous ulcer of the breast.—*Gazette Médicale*, December 4, 1847.

SUBCLAVIAN ANEURISM CURED BY GALVANO-PUNCTURE. BY DR ABEILLE.

THE patient, a female of sixty-five years of age, had been affected with aneurism of the left subclavian artery during eighteen months previously to her requesting the professional services of Dr Abeille, principal medical officer in the Military Hospital of Givet (Ardennes). The tumour was situated between the *scaleni* muscles, and was of the size of a hen's egg. It was the source of much suffering, sleeplessness, and ringing in the ears of the patient. Added to these, she had a continual fear of sudden death, which induced her to submit to the performance of any operation which might be thought necessary for her relief. M. Abeille, considering the difficulty and doubtful practicability of applying a ligature on the vessel between the aneurismal dilatation and the heart, determined to give a trial to the method of cure, by passing a galvanic current through the contents of the sac.

After performing a series of experiments on dogs, which assured him of the possibility of obtaining a favourable result, the patient was submitted to the following operation on the 20th of February 1847.

As soon as the patient was rendered insensible to pain by the inhalation of ether, two pair of needles were inserted into the tumour to the depth of an inch, and a strong galvanic current was established in connexion with them. At first the effect on the patient was slight, but at the end of five minutes it required four assistants to hold her. The operation was continued for twenty-eight minutes. During this time the tumour was felt to be becoming gradually solidified, and before the withdrawal of the needles it had become perfectly

solid; and pulsation was no longer felt in it or in the brachial or radial artery below. The limb became engorged, and the patient complained of its being benumbed and prickling. During the operation, the artery above the tumour was partially compressed by an assistant. After the operation, this compression was continued by means of an apparatus for five or six hours. In withdrawing the needles, two of them were removed with ease, but the others required a good deal of rotary motion, and some drops of blood escaped from their punctures. The patient maintained the same position in which she had been placed during the operation, for eight or ten hours afterwards.

For forty-eight hours after the operation no pulsation could be detected in the arteries of the limb. No oedema, however, ensued, and sensibility remained unimpaired. At the end of this time the radial artery commenced to pulsate, and the limb gradually recovered its natural temperature. About the eighth day the tumour appeared to be diminished in size, and this diminution progressed gradually; so that, at the end of thirty-eight days, nothing but a small, oval, firm tumour could be felt on pressing strongly with the fingers in the situation of the swelling. During the first few days which followed the operation, there was some threatening of cerebral congestion, which was relieved by blood-letting.

No symptom of inflammatory action manifested itself either on the surface or in the tumour. From the punctures of the two needles, which were withdrawn with difficulty, there was slight discharge of blood and matter for three days; but they were cicatrized a few days subsequently.

The report is given seven months after the performance of the operation, and it is stated that there existed, at the end of that time, no trace of the aneurism; and in its place a hard flattened cord, to which the skin adhered. Immediately above the situation of the aneurism, two enlarged collateral branches were felt pulsating strongly. The patient was in the enjoyment of perfect health.—*Annales de Thérapeutique*, Novembre 1847.

COMPLICATED CASE OF HERNIA. By MR COCK.

AMONG some interesting observations on some obscure and difficult forms of hernia, by Mr Cock of Guy's Hospital, of which want of space precludes us from making fuller extracts, we find the following rare and instructive case:—

A labourer, æt. thirty, was admitted into Guy's Hospital on the 17th of July last, under the care of Mr Cock, labouring under a hernia of the right groin, which had been for several days in a state of strangulation. The tumour had existed for twelve years; and for the last few years a truss, which had been formerly efficient, did not prevent the occasional escape of the bowel. Four days before admission, the swelling had attained a larger size than he had ever previously observed, and he called in a surgeon who effected reduction; but the bowel soon resumed its former position. Reduction was again repeatedly effected; but the tumour as often returned into the scrotum. Partial reduction only could be effected after his admission into the hospital; and as soon as he had somewhat rallied, under the administration of stimuli, the operation was performed. The external ring was divided freely, without opening the sac. The contents of the sac were then easily passed up through the opening, and the hernia was apparently reduced. This apparent reduction was, however, accompanied with an increased fulness of the inguinal canal, and there was a tendency to the reproduction of the tumour as soon as the pressure was removed. The sac was then freely laid open, and about four inches of highly congested intestine were brought into view. On tracing this upwards with the finger, it was found firmly grasped by the margin of the internal ring, which was very deeply seated, as if it had been pushed up by the frequent operation of taxis beyond its normal position. This stricture was now divided, and the intestine reduced. Immediate relief was obtained; but, soon after, the patient began to complain of pain in the iliac region; became restless and excited; and died seven hours after the operation.

Post-mortem Inspection, thirty-seven hours after death.—On opening the peri-

toneal cavity, the portion of bowel which had formed the hernial tumour, and which had been returned into the abdomen at the time of the operation, was readily detected; but the internal ring was still occupied by a portion of small intestine, which passed through it from the abdomen, and disappeared, as it were mysteriously, without coming down into the hernial sac. On tracing this portion of intestine, it was found to pass through a ruptured opening at the back and upper part of the sac, immediately below and behind the internal ring, and had become lodged between the peritoneum and the fascia iliaca. The mass of bowel which had thus escaped altogether from the serous cavity, measured three feet in length, and occupied a considerable portion of the fossa iliaca, passing over the psoas, and overhanging the true pelvis. It was congested, but did not appear to have undergone actual strangulation. There were no positive signs of peritonitis; but the general decomposition of the body had obscured the precise state of the serous membrane.

This rupture of the sac had probably been caused by the patient's own forcible attempts to reduce the hernia; and it is probable that, as fresh portions of intestine descended into the scrotum, they were forced back, partly into the abdomen, and partly through this adventitious opening, till the accumulation of bowel above mentioned had become lodged in the iliac fossa, between the fascia iliaca and the peritoneum.—*Guy's Hospital Reports*, Second Series, vol. v.

SPONTANEOUS ANEURISM AT THE HEAD OF THE ARM. By M. VELPEAU.

A YOUNG man was admitted during last month into M. Velpeau's wards of La Charité on account of a tumour containing fluid in the above situation. It had no pulsatory movement, and pulsation had never been felt in it since its first appearance five or six years before. He had no pain in it, and suffered inconvenience only from its limiting the motions of the arm. M. Roux and several other surgeons had seen it, but none had come to the conclusion at which M. Velpeau arrived, that the tumour was aneurismal. He thought, however, that diminution in the size of the tumour could be produced by pressure, and, on introducing an exploratory needle, some arterial blood escaped. A ligature was accordingly placed on the brachial artery immediately above the tumour, but without the effect of changing in the least the size or condition of the latter. M. Velpeau is now attempting to obliterate the sac by producing inflammatory action in its interior, and for this purpose has introduced several needles through it.—*Annales de Thérapeutique*, Octobre 1847.

We have named this an aneurism, as it is so called in the journal from which we have extracted the case. The usual decisive symptoms of aneurism, however, are all absent, and deceptive symptoms only described. Nothing, we are convinced, is more deceptive than the diminution in size (*affaissement*) which appears to take place on pressing over such tumours. It must be remembered, moreover, that an exploratory needle may give issue to arterial blood without the tumour being an aneurism; and in the above case, we doubt not that the circumstance would have been mentioned had the blood issued in a full stream. The editor of the journal has promised to record the future history of the case, which we shall be anxious to learn.

P.S.—Since writing the above notice, we have received the next Number of the *Annales de Thérapeutique*, in which the termination of this case is given. The needles were allowed to remain in the tumour for ten days, and when removed at the end of that time, the contents of the tumour were still found to be fluid. Compression still diminished the size of the swelling, but pulsation had ceased. M. Velpeau then determined to lay open the sac, and apply a ligature above and below the opening in the vessel, if such were found to exist. An incision was made, five inches in length, through the tumour, when a cavity was found in the muscular substance, from which, on slackening the tourniquet, dark blood flowed

in a continuous stream. Several small bleeding points were tied, but the hemorrhage continued from above. On searching for the source of this bleeding, a small sac was found at the upper part of the wound, around the base of which ligatures were applied, and the whole cavity was filled with charpie. The hemorrhage was now entirely arrested, the patient was put to bed, and the limb placed in an elevated position. Two days afterwards there had been no return of the hemorrhage, but the patient had suffered from a severe attack of epistaxis, and seemed to be in an unsatisfactory condition.

In a note, it is added that, shortly after the report, the patient died from tetanus, and that, before death, bleeding had twice taken place from the wound.

M. Velpeau considers this tumour a rare variety of aneurism, consisting of the presence of a very small sac, like a thimble, around an opening in the coats of the artery; and that from this sac the blood exuded or escaped by a direct communication between the large cavity and the interior of the aneurismal dilatation. The idea of "*transpiration*" of blood through the aneurismal walls is opposed to established facts in the pathology of aneurism. The direct communication, however, between the cavity and the aneurismal sac, merely renders the case one of not uncommon occurrence; viz. the supervention of a *diffuse* aneurism on the rupture of the walls of a *true* aneurismal dilatation. The rarity consists in the existence of a spontaneous aneurism at the head of the arm, and, whether such was the nature of the case, we hope to be enabled to inform our readers, by an account of the autopsy, on a future occasion.

MIDWIFERY, AND DISEASES PECULIAR TO WOMEN.

VOLUMINOUS ENTEROCELE THROUGH THE FUNDUS OF THE UTERUS.

By M. LE CHAPTOIS.

A WOMAN, sixty years of age, mother of seven children, had been affected with what the author calls a vaginal hernia for a long time, for which she had never sought relief. The disease progressed, and she experienced a sensation of weight and dragging in the hypogastrium. Tenesmus and dysuria appeared, and gradually became so considerable that her body became bent forward, the head approaching the knees. Called in to relieve an unusual aggravation of this condition, M. Chaptois found that an enormous mass of the small intestines and the colon had passed out through the uterus; which was dragged with it and torn. The uterus was completely retroverted, turned inside out, like the finger of a glove, and hung between the thighs. The tumour was pyriform, of a reddish-brown colour, somewhat larger than the fist. The walls of the fundus were thin, and were apparently destitute of all circulation. Towards the right Fallopian angle there existed a slit, or tear, six centimetres in length, which had given passage to the mass of intestines, the last coil of which that had been forced out, was of a rose colour. The vitality, almost extinguished in the older part of the hernia, was manifested, in that most recently displaced, by distinct pulsations. The pulse was small and miserable; there was hiccough, cold sweats, and frequent syncope.

M. le Chaptois, after having washed the hernial organs, succeeded in causing them to re-enter the pelvic cavity, where they were retained by introducing into the vagina a fine sponge, sustained by a convenient apparatus. Frequent vomitings, and horrible convulsions, became manifested; but, after some abundant stools, she became tranquil. The appetite soon returned, and the convalescence proceeded regularly until the third month, when a peripneumonia caused death in fifteen days. There was no examination of the body.—*Gazette Médicale*, Decembre 18, 1848.

A REMARKABLE CASE OF FISSURA ANI FOLLOWING CHILDBIRTH, AND CURED WITHOUT INCISION. By DR J. VAN DEEN.

THE woman, of sanguineous nervous constitution, was safely delivered of her first child in her twenty-eighth year. Previous to marriage, she had been subject to dysmenorrhœa and habitual constipation, with a tendency to consumption and congestion of various internal organs, particularly the left lung; on account of these congestions she was, during her first pregnancy, five times bled.

After delivery she suffered from a severe milk fever. The lochia flowed for nine weeks, the milk was sparing in quantity, and the nipples severely chapped; these chaps disappeared with the lochia, and the patient then began to complain of a constrictive pain in the anus, particularly during alvine evacuations, which, from the feeling, seemed never to be perfectly accomplished; this pain gradually increased, and extended along the ischiatic nerve. On examination, a hemorrhoidal swelling, the size of a hazel-nut, was found on the perineal side of the anus, which was violently constricted; no fissure was discovered. In spite of the application of leeches the pain increased, particularly during evacuation of the rectum, which occurred several times a-day, and was constantly followed by prolapsus ani. The patient suffered also from a burning feeling in the anus, with paroxysmal increases, which she sought to diminish by the application of cold articles. The shooting pains in the thigh left after the application of a belladonna plaster. Seven days after the first, a second examination was made, when the hemorrhoidal mass was found extending from the anus to the vulva; on the right side of this tumour was a fissure commencing tolerably broad, and ending by an acute angle in the perineum; it also extended a few lines up the rectum, and ended there also very acutely; there was also combined, with this fissure, a smaller transverse one which ended in it. The surface of the fissure, and of a few sores consequent on the leech bites, was covered with a white purulent fatty matter. Belladonna ointment and an oil emulsion internally, produced no alleviation; a liniment of oil hyoscyamus and lime water proved, however, very useful. Eleven weeks after delivery the menses returned more copiously than formerly, and accompanied by violent pains in the fissure, and a burning heat in the vagina; two small sores, the size of peas, with bright red edges and a white surface, were now discovered, one on each side of the clitoris. On account of the violent pain, no examination of the rectum could be made. As soon as the menses ceased, the burning pain in the vagina ceased also, and the two sores healed. During the period following, the condition of the patient was very variable, but the violent pain was somewhat less, and the paroxysms did not follow one another so speedily. She drank liquorice water (Succ. liq. ʒij, Aq. com. ʒxxiv) constantly; the liniment was continued, Laud. liq. Syd. being added to it. On the reappearance of the menses, the same occurrences were repeated, but now there were three sores instead of two in the vagina; on their cessation the sores again healed, and this time much more rapidly than formerly. After this, the phenomena ceased, first, the pain, then the prolapsus—the stools becoming more consistent, then the sores and fissures healed; so that, five months after delivery, the patient was quite restored. It is remarkable that the chaps around the nipples, which healed previous to the occurrence of the anal fissure, after its disappearance re-occurred and remained; so much so, that the patient, whose milk was also since the return of the menses rather scanty, was obliged to give up nursing. Since then, the menses have twice recurred, each time with itching of the vulva, redness of the vagina, and a less degree of ulceration around the clitoris. There is now no trace of fissure, the functions are all regular, and the stools as formerly costive.—*Nieuw. Archief*. II. 2.

BRANDY IN CHILDBIRTH. By RETZIUS.

In a puerperal patient, the shoulder of the child had been finally wedged for three days, and, in spite of copious venesection, all attempts to turn proved

fruitless. Brandy was now given to the mother till complete intoxication was produced; in two hours after, turning was performed without difficulty. The woman recovered perfectly, but lost one of the labia externa from gangrene.—*Med. and Pharm. Journ.* Stockholm, 1845.

THE BLOOD OF MENSTRUATION. By E. H. WEBER.

EVERY now and then we read that the blood flowing during menstruation possesses no fibrine. The author gives a new proof to the contrary, in as much as in the bodies of such women as have died suddenly while menstruating, he has seen coagulated blood upon the uterine mucous membrane. If, therefore, menstrual blood does not coagulate, it is simply because it has already coagulated within the uterine cavity, and cannot, therefore, do so again; it is more fluid than ordinary blood, because, during its trickling descent, it becomes mixed with watery uterine, and vaginal mucus.—*Schmidt's Jahrb.*, 1847, No. VII. p. 139.

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVII.

MEETING II.—*Wednesday, December 1st, 1847.*—DR D. MACLAGAN in the Chair.

1. FURTHER PARTICULARS OF A CASE OF INTESTINAL CONCRETIONS, FORMERLY COMMUNICATED TO THE SOCIETY; WITH REMARKS. By ROBERT TURNER, M.D., Keith.—This communication is inserted entire in our present Number.

Dr Mackenzie stated, that he had opened the body of a patient who had been an inmate of the City Lunatic Asylum for upwards of thirty years. During this time, the greater part of his diet had been composed of oatmeal. The concretions were forty-two in number, and varied in size from a hazel-nut to a large chestnut.

Dr Christison remarked, that notwithstanding the population was nearly doubled since the time of Monro, these concretions are now scarcely to be met with, although they were then very frequent. He had never had but one submitted to him for examination, and that was by Mr Liston. Is it possible that it depends upon a different method of making the meal? He believed that oatmeal itself was not used so commonly now as formerly. He had also lately examined many specimens of meal, and had been unable to detect in them the spicula or bristles of the oat.

Dr Douglas Maclagan observed, that this was explained by the introduction of winnowing machines.

2. ON THE SUPERINDUCTION OF ANÆSTHESIA IN NATURAL AND MORBID PARTURITION. By Professor SIMPSON.—After making several quotations from the most celebrated obstetrical writers, in order to show the great agony experienced in human parturition, he pointed out, from statistical data, that long-continued pain was itself an evil, and tended to increase the mortality of all great operations. He stated that, since the latter part of January, he had employed etherization, with few and rare exceptions, in every case of labour which has been under his care. And the results had been most happy and gratifying. He never had the pleasure of watching over a series of more perfect or more rapid recoveries; nor had he once witnessed any disagreeable result to either mother or child. He had kept up the anæsthetic state during periods varying from a few minutes to three, four, five, and six hours, and did not remember a single patient to have taken it who has not afterwards declared her sincere

gratitude for its employment, and her indubitable determination to have recourse again to similar means under similar circumstances.

Two agents have the power of producing anæsthesia during labour, viz. the inhalation of sulphuric ether, and the inhalation of chloroform. He believed that the latter agent possesses various important advantages over the former, particularly in obstetric practice; and that, in particular, it is far more portable; more manageable and powerful; more agreeable to inhale; is less exciting than ether; and gives us far greater control and command over the superinduction of the anæsthetic state. He then gave the following cases as illustrative of its beneficial action:—

CASE I.—The patient to whom it was first exhibited had been previously delivered in the country by craniotomy after a very long labour. Her second confinement took place a fortnight before the full time. Chloroform was begun to be inhaled when the os uteri was becoming well expanded, and the pains very severe. In twenty-five minutes the child was born. The crying of the infant did not rouse the mother, nor did she awake till after the placenta was removed. She was then perfectly unaware that the child was born. She stated her sensations to be those of awaking from “a very comfortable sleep.” It was, for a time, a matter of no small difficulty to persuade her that the labour was over, and that the living child presented to her was her own.

CASE II.—The patient had, at her preceding confinement, been in severe labour for twenty hours—followed by flooding. She began the inhalation when the dilatation of the os uteri was half completed. The child was born in fifty minutes afterwards. She was kept under its influence for a quarter of an hour longer, till the placenta was removed, and the binder, body, and bed-clothes, all adjusted. On awaking, she declared she had been sleeping refreshingly; and was quite unconscious that the child was born, till she suddenly heard it squalling at its first toilet in the next room. No flooding. An hour afterwards, she declared she felt perfectly unfatigued, and not as if she had borne a child at all.

CASE III.—Patient unmarried. A first labour. Twins. The first child presented by the pelvis, the second with the hand and head. The chloroform was exhibited when the os uteri was nearly fully dilated. The passages speedily became greatly relaxed (as has happened in other cases placed under its full influence); and in a few pains the first child was born, assisted by some traction. Dr S. broke the membranes of the second, pushed up the hand, and secured the more complete presentation of the head. Three pains expelled the child. The mother was then bound up; her clothes were changed; and she was lifted into another bed. She slept a full hour afterwards. The patient recollected nothing from the time of the first inhalations; and was in no small degree distressed when not one—but two—living children were brought by the nurse to her.

CASE IV.—Primipara of full habit. When the first examination was made, the passages were rigid, and the os uteri difficult to reach. Between six and seven hours after labour began, the patient, who was complaining much, was apathized with the chloroform. In about two hours afterwards, the os uteri was fully dilated, and in four hours and a half after the inhalation was begun, a large child was expelled. The placenta was removed, and the patient bound up and dressed before she was allowed to awake. This patient required an unusual quantity of chloroform; in all, four ounces were used. Like the others, she was quite unconscious of what had gone on during her anæsthetic state; and awoke altogether unaware that her child was born.

CASE V.—Second labour. This patient, after being several hours in labour, was brought to the Maternity Hospital. I saw her some time afterwards, and found the first stage protracted by the right side of the cervix uteri being thick, œdematous, and undilatable. The inhalation of chloroform was begun, and the first stage was terminated in about a couple of hours. Two or three pains drove the child through the pelvic canal, and completed the second stage. Fifteen minutes in all elapsed from the termination of the first to the termination of the third stage, or the expulsion of the placenta. The patient was

dressed and removed into a dry bed, where she slept on for a short time before awaking, and being conscious of her delivery.

CASE VI.—Second labour. The patient, a person of small form and delicate constitution, bore her first child prematurely at the seventh month. After being six hours in labour, the os uteri was fully expanded, and the head well down in the pelvic cavity. For two hours subsequently, it remained fixed in nearly the same position, and scarcely if at all advanced, although the pains were very distressing, and the patient becoming faint and exhausted. She entertained some mistaken religious feelings against ether or chloroform, which had made her object to the earlier use of the latter; but she was now placed under its influence. She lay as usual like a person soundly asleep under it, and Dr S. was now able, without any suffering on her part, to increase the intensity and force of each recurring pain, by exciting the uterus and abdominal muscles through pressure on the lower part of the vagina and perineum. The child was expelled in about fifteen minutes after the inhalation was commenced. In a few minutes she awoke to ask if it was really possible that her child had been born; and was overjoyed to be told that it was so. In this case the forceps would in all probability have been ultimately required, perhaps hours subsequently, provided there had been no interference in the way mentioned.

CASE VII.—A third labour. The patient had been twice before confined of dead premature children; once of twins; the second time of a single child. The liquor amnii began to escape about one o'clock A.M., but no pains followed for some time. Dr S. saw her between three and four, with the pains commencing, and the os uteri beginning to dilate. In two hours afterwards the first stage was well advanced, and, the pains becoming severe, she had the chloroform exhibited to her, and slept soundly under its influence. In twenty minutes the child was born, and cried very loudly without rousing the mother. In about twelve or fifteen minutes more she awoke, as the application of the binder was going on, and immediately demanded if her child was really born and alive, as she thought she had some recollection of hearing the nurse say so. She was rejoiced beyond measure on her son being brought in and presented to her.

CASE VIII.—Fourth labour. The patient had born three dead children prematurely, about the sixth and seventh months of utero-gestation. During her present pregnancy she was placed under strict rules and discipline; and she used, from an early period, small doses of chlorate of potass several times a-day. She carried her child to the full time. Labour came on about one o'clock A.M. The membranes broke at eight A.M., when the os uteri was still very slightly open. It had made very little progress till ten o'clock, when Dr Keith exhibited the chloroform to her. The pains continued very strong and regular, the passages relaxed, and at half-past eleven she was delivered of a large living child. The placenta came away immediately; and she was bound up, and made dry, before she awoke. She remembered nothing whatever that had occurred after she began to inhale the chloroform till the period of her awaking.

The effects of the superinduction of anæsthesia in parturition are, if possible, still more marked and beneficial in cases of morbid labour and operative delivery. In proof of its influence in this respect, some examples of its employment in cases of turning, of the application of the forceps, and of embryulsió, were given.

CASE IX.—Fourth labour. The mother deformed, and the conjugate diameter of the brim of the pelvis contracted from the projection inwards and forwards of the promontory of the sacrum. Her first child was delivered by embryulsió; the second by the long forceps; the third was small, and passed without artificial assistance. On the present occasion, after suffering slight pains during the whole night, labour set in with greater severity towards morning. After being in strong labour for some hours, she was seen first by Mr Figg, and afterwards by Dr Peddie, her ordinary medical attendant. Dr S. was called to her about four o'clock P.M. The pains were then enormously powerful and straining, imparting to the mind the dread of the uterus ruptur-

ing under their influence; but the head of the child was still altogether above the brim, and only an œdematous ridge of the scalp pressed through the superior and contracted pelvic opening. The passages had become heated, the mother's pulse raised, &c., and Dr Peddie had tried three different pairs of long forceps. The urgency and power of the uterine contractions, the immobility of the head upon the brim of a deformed pelvis, and the state of the patient and of the parts, all showed the necessity of relief being obtained by artificial delivery. In her first labour she had been delivered under similar circumstances by perforation of the head. But here the child's heart was heard distinctly with the stethoscope, and he at once agreed to the proposition of delivering her by turning the infant,—compressing and indenting the flexible skull of the fœtus, instead of perforating it, and thus affording some chance of life to the child, and more chance of safety to the mother. The patient was placed under the influence of chloroform still more deeply than when the forceps were used, in order, if possible, entirely to arrest the uterine contractions. The hand was passed into the uterus, a knee seized, and the infant easily turned; but very great exertion and pulling was required to extract the child's head through the distorted brim. At last it passed, much compressed and elongated. The child was still-born, but, by applying the usual restorative means, it speedily began to breathe and cry. The child continues well, and the mother has made a rapid recovery.

CASE X.—In the Maternity Hospital; first child. Labour began at ten P.M. (21st Nov). Dr S. was desired to see her at six A.M. (22d). The os uteri was well dilated, but it was evident that the pelvic canal was contracted throughout, and the head was passing with unusual difficulty through the brim. The patient was complaining much of her sufferings. It was clear that it would be a very tedious and probably at last an instrumental case, and one therefore calculated to test the length of time during which chloroform might be used. She began to inhale it at a quarter past six A.M., and was kept under its influence till a quarter past seven P.M., the date of her delivery; *thirteen hours in all*. From the time it was begun to the time delivery was completed, her cries and complaints ceased, and she slept soundly on throughout the day. The bladder required to be emptied several times with the catheter. The head passed the os uteri at ten A.M.; and, during the day, gradually descended through the pelvis. At seven A.M. it was deemed proper to deliver her by the forceps; the head, which was now elongated and œdematous, having by that time rested for some hours against the contracted pelvic outlet with little or no evidence of advancement, the bones of the fœtal cranium overlapping each other, and the fœtal heart becoming less strong and distinct in its pulsations. A warm bath, irritation of the chest, &c., were necessary to excite full and perfect respiration in the infant. On afterwards measuring the quantity of blood lost, it was calculated to amount to 15 or 18 ounces. The mother's clothes were changed; she was bound up and removed to a dry bed before she awoke. She had at first no idea that the child was born, and was in no respect conscious of being delivered. In fact she had been "sleeping," according to her own account, from the time she had begun the inhalation, and only thought she once or twice remembered or dreamed that she heard the house surgeon speak near her. The mother and child have continued perfectly well.

CASE XI.—Patient with a deformed spine and contracted pelvic outlet. At her first confinement two different medical gentlemen had failed in effecting a delivery by the forceps. After being very long in labour, and the symptoms of the case becoming urgent, Dr S. saw her with Dr Paterson of Leith. The head was low down in the pelvis; but it was placed in the right occipito-posterior position (the third of Naegele), and the forehead instead of the vertex was presenting, one orbit being easily felt behind the symphysis pubis. It had been lodged in nearly the same position for many hours. The fœtal heart was still distinct, but weak. The forceps were applied—the head turned round

with them a quarter of a circle, into an occipito-anterior position (the second of Naegele); and, after being so adjusted, it still required considerable force to extract it. Before applying the forceps the patient was sent into a state of deep anæsthesia by the inhalation of chloroform; and subsequently, when she wakened out of it, she was in no small degree surprised to find that she had really been delivered while she was sleeping and resting so soundly. The placenta separated, and the uterus contracted firmly. The child, which was large, lived for eight hours after delivery; but, despite of all the measures tried, full and perfect respiration was never established in it, apparently in consequence of some effusion or injury about the base of the brain. Unfortunately a post-mortem examination was not obtained. The mother has made an excellent recovery.

Dr Christison said, that having had a strong conviction that the inhalation of this agent for annulling pain would prove one of the greatest discoveries in materia medica in his day, he had requested *Dr Simpson* to give him an opportunity of observing for himself its effects during labour. He had seen that the uterine contractions were in no degree diminished by it, and that, although the case was a complicated one, nothing could be more satisfactory than its progress and termination, the woman remaining all the time perfectly insensible. One great advantage of this new agent over sulphuric ether, was the simplicity in giving it. No apparatus was necessary. A handkerchief was better than any apparatus. He thought that the present discovery was a powerful illustration of the importance of watching the progress of chemical discovery. One of the last remarks made to him by Professor *Dumas*, before he left Edinburgh, was, "How admirable a proof of the absurdity of the prevailing tendency to sneer at the minutiae of chemical discovery! Here is a substance known for twelve years and more only as a subject of curious research, and seen even by few chemists, except those who had specially investigated its chemical relations, nevertheless at once advanced to be an important article of the materia medica." Some have complained of failures in using it, or of unpleasant effects afterwards. But at present we were in the infancy of our knowledge regarding the preparation of chloroform. The purification is probably a matter of great importance, and many spurious articles are met with at present in the shops.

Dr Douglas MacLagan asked if any dose of chloroform could interfere with uterine action.

Dr Macaulay doubted the propriety of giving it universally, and in all cases of labour. He believed that pain in parturition was as natural to the human female as the menstrual discharge. In obstinate cases of labour, and in cases of amenorrhœa, it might be proper to interfere with nature; but in ordinary or natural labour, as with natural menstruation, he thought this unnecessary. In some cases chloroform inhalation might be hazardous, as in cases of internal hemorrhage, where the feelings of the patient, and the syncope, often gave warning of danger. The effects on the brain also might induce eclampsia. He believed moderate pain to be frequently useful, and thought it improper under such circumstances to deprive an individual of her senses. He himself, if ill, would endeavour to preserve his mental faculties under all circumstances. Further, he thought this remedy capable of being greatly abused. What was the origin of dram-drinking? It frequently commenced in the endeavour to remove some inconvenience, by producing insensibility. He thought it better to learn to bear pain, than be always seeking to avoid it by such means.

Dr Roberts had observed nausea and vomiting produced by it in six cases. In two instances the urine had been discharged involuntarily, and in one lady three doses of one drachm had produced no effect.

Dr Duncan stated, that the nurses of the Royal Infirmary had observed nausea and vomiting to be common symptoms after the inhalation of ether, but not of chloroform.

Dr Simpson, in answer to *Dr MacLagan's* question, observed that chloroform was generally used in different doses in midwifery, from what it was in surgery. In surgery it was given in much larger and more powerful doses, so as completely to destroy all chance of suffering. In midwifery, *Dr S.*, used it in smaller doses, giving merely a large one at first, stopping whenever sopor or sonorous breathing began; and then subsequently keeping up its anæsthetic action by a few inhalations every two or three minutes, or before each returning pain. In this way the patient can be kept utterly free from pain without at all interfering with the uterine contractions. If a large dose be given and continued, its *deepest* effects, uterine action, will certainly be moderated, and at last stopped; and *Dr S.* had done so with, he thought, much advantage in turning. It was the more useful because it could be thus employed so as to produce two different results at will. As regards the objections of *Dr Macaulay*, he also *theoretically* had experienced some fear, that ether or chloroform might occasion hemorrhage; but experience had convinced him that, with common care, it had no tendency to produce this result. With respect to internal hemorrhage, we had better means of detecting it than by the sensations of the patient. He had never seen it occasion the slightest tendency to eclampsia. He had frequently observed it produce tonic spasms in experiments. During the period he had given ether so extensively, he had had one case, and one case only, of death among his patients. She had died of convulsions a fortnight after delivery. But that person had *not* taken ether; although, if she had done so, her death, doubtless, would have been ascribed by some to the inhalation. He could assure *Dr Macaulay*, that whatever might be his philosophic opinions regarding the propriety of enduring pain, pain itself was a mighty logician, and overthrew the strongest arguments and resolutions. Had *Dr Macaulay* lived in the days of monkish superstition, he would probably, like an anchorite, have fancied it his duty constantly to use a lash and hair shirt, or to make his patients use them. What amount of pain would please *Dr Macaulay*—sixty, twenty, ten pangs, or less? Perhaps he and his patients might differ in opinion on this point. *Dr S.* had very, very rarely, seen sickness or vomiting follow chloroform. Was it not owing to the quality of the chloroform, or its mode of administration? *Dr Simpson* concluded by observing, that he had tried to administer other substances in the form of inhalation. He had found that the inhalation of forty or fifty drops of oil of juniper had a marked diuretic effect. He considered that its administration, in this way, would be very useful in certain cases of dropsy.

The following office-bearers were then elected for the ensuing session:—*President*, Robert Hamilton, M.D.; *Vice-Presidents*, W. Beilby, M.D.; Sir W. Newbigging, M.D.; John Goodsir, Esq.; *Council*, Douglas MacLagan, M.D.; Robert Nasmyth, Esq.; James Begbie, M.D.; James Spence, Esq.; John Coldstream, M.D.; James Dunsmure, M.D.; Archibald Mackellar, M.D.; Richard Mackenzie, M.D.; *Treasurer*, Robert Omond, M.D.; *Secretaries*, John Hughes Bennett, M.D.; John Taylor, M.D.

MEETING III.—*Wednesday, 15th December 1847.*—*Dr BEILBY*, V.P., in the Chair.

1. CASE OF RECOVERY FROM A POISONOUS DOSE OF STRYCHNINE, WITH OBSERVATIONS ON THE TESTS OF THE ORGANIC ALKALIES. By *Dr THOMAS ANDERSON*.—This paper will appear entire in our next Number.

2. REPORT OF THE SOCIETY'S COMMITTEE ON THE PROPERTIES OF CHLOROFORM.—This report is given entire.

Committee.—*Dr Hughes Bennett*, *Convener*. Drs Alison, Christison, Douglas MacLagan, Taylor, Duncan, Roberts, Allen Thomson, and Gardener.

Reporter—*DR HUGHES BENNETT*.

Since a committee was appointed at the meeting of this Society, on the 10th of last November, many of the points on which it was presumed it would have

to deliberate, have already been decided by the profession at large. Perhaps there is no instance on record, where the discovery of a remedy has been so rapidly made known and so universally adopted as that of the inhalation of chloroform. Thus it is exactly only five weeks ago, that this substance was first publicly announced by its discoverer to this Society, and in that short period of time it has been universally adopted as preferable to ether for annulling pain; it has been given to thousands of individuals, and already numerous ingenious instruments have sprung into existence to render its administration more certain and convenient. Only one week after the publication of Dr Simpson's pamphlet on the subject, it was universally employed in the Parisian hospitals, and recognised as being superior to ether. The same appreciation of its advantages has every where attended its introduction. By means of the press, and by railways, a knowledge of its properties has already reached every part of Europe; and at this moment probably our American brethren are testing the merits of the new agent they have received from us, with the same interest and avidity with which we last January employed the old one we obtained from them.

But, although the great advantages and properties of chloroform are already fully estimated, there are many points connected with its action on the animal economy that require to be elucidated. Moreover, as the observations and experiments made by the committee are not only in themselves interesting and unusually exact, but may tend to throw some light upon its physiological action, and many circumstances connected with it, a succinct report of their labours may still be not altogether unprofitable. It should be understood, however, that the committee as a body only guarantee the accuracy of the facts,—all the conclusions and theoretical statements rest solely on the authority of the reporter.

First Series of Experiments.—November 13, 1847.

The first experiments before the committee were limited to observing the effects of chloroform inhalation upon the pulse and sensations, with various doses. They were mostly performed on young men, students or practitioners of medicine.

Experiment 1.—30 minims of chloroform were poured into a small bag of triangular form, made of oil skin and lined with linen. The subject of the experiment was Dr B—, æt. 25, who had previously more than once taken ether. Before the commencement of inhalation he stated that he was somewhat excited, and the pulse was counted to be 120. After the first few inspirations the pulse rose to 140. He inhaled a minute and a half without its producing any other effect.

The oil-skin bag was now laid aside, and 3j of chloroform was poured on a handkerchief. In 1 min. and 5 secs. unconsciousness was complete. The hand was pinched strongly, and several hairs of the head plucked out by the roots, without producing signs of sensibility. He continued insensible $\frac{5}{4}$ of a minute.

The pulse, which was 92 before the second inhalation, rose rapidly during its continuance to 120. It remained at 120, small, until the insensibility began to disappear, and then fell to 96.

Experiment 2.—The subject of experiment was Mr P—, æt. 21, labouring under slight bronchitis. He had never inhaled either ether or chloroform previously, and his countenance was pale and somewhat anxious.

30 minims of chloroform were poured on a handkerchief, and applied to the mouth and nose. He inhaled 1 min. and 15 secs.; no insensibility was produced, he felt himself pinched. The pulse which, at the commencement of the experiment, was 94, rose rapidly to 120, and afterwards sunk to 84. Copious diaphoresis was produced.

3j of chloroform was now poured on the handkerchief, and applied to the face. In 58 secs. he was completely unconscious. The pulse at the beginning

of the second inhalation was 84, and remained so during the whole time of inhalation.

Experiment 3.—Dr G——, æt. 28, inspired from a folded handkerchief on which 3j of chloroform had been poured. In 45 secs. he fell backwards, perfectly insensible to pinching. On recovering, he stated that he seemed at first to be looking through a mist, and saw indistinctly. The *pulse* was raised from 84 to 98. Had frequently taken ether. The inspirations in this experiment were long and well performed, hence probably the rapidity of the action.

Experiment 4.—Mr G——, æt. 20, had never taken ether. 30 drops of chloroform were used. The first few inhalations produced slight cough. He inhaled 58 secs., and said afterwards that he was unconscious; but he gave signs of sensibility when pinched, and when his hair was pulled.

30 drops more of chloroform were now placed on the handkerchief, and after inhaling 1 min. and 32 secs. he became completely unconscious. The *pulse*, which at the commencement of the second inhalation was 80, was not altered. He stated afterwards, that at first he thought he should have died of apoplexy, and he felt each stroke of his heart. There was considerable diaphoresis, but not so much as in the second experiment.

Experiment 5.—Mr L——, æt. 19, had previously inhaled ether. 80 drops of chloroform (a large dose) employed. Became unconscious after a few inspirations, and says he remembers nothing after the first one. The *pulse*, 84 before inhalation, rose immediately to 120.

Experiment 6.—Dr D——, æt. 22, had frequently inhaled ether. 20 drops of chloroform used. Was perfectly insensible to stimuli in 30 secs.; felt nothing. The *pulse* was 80, and was not affected.

Experiment 7.—Mr Y——, æt. 38. 3j of chloroform used. Became unconscious in 50 secs. There was slight cough at the commencement of inhalation. The *pulse* was 120 before inhalation, and afterwards became so rapid that it could not be counted.

Experiment 8.—Dr W——, æt. 26, wished to have two teeth extracted. 3j of chloroform was used. Inhaled $1\frac{1}{2}$ min., when Dr Roberts extracted a tooth. The dens sapientia of upper jaw. The *pulse*, at first 108, rose to 120. On recovering he said he was not altogether insensible, and that he had experienced a dragging sensation, but no pain. He thought the tooth had not been extracted, and consented to have another attempt made.

He again inhaled one drachm of chloroform for two minutes, when, appearing to be unconscious, Dr Roberts extracted the first molar of the left side in the under jaw. On recovering, he stated that he experienced the same sensation; but was surprised on hearing that both teeth were extracted.

Experiment 9.—Dr M——, æt. 36, inhaled from a handkerchief on which twenty drops of chloroform were put, holding it in his own hand. In forty seconds it produced the most extravagant gaiety, bursts of laughter, snapping of the fingers, and gesticulations expressive of great delight. On recovery, he stated that he had experienced exactly the same sensations as when he had taken nitrous oxide gas some time previously.

From this first series of experiments, it resulted that the effects of chloroform inhalation differed in different individuals, and according to the quantity employed. One drachm seemed to be the medium dose for producing unconsciousness. In the second series of experiments, therefore, this quantity was uniformly employed, and exact observations were made by different members of the committee as to the time the various effects were produced, and the changes in the pulse, respirations, skin, and pupils. The sensations of the individual also were accurately recorded. Stronger chloroform was used, and galvanic shocks, of considerable power, tested the sensations of the individual. Inhalation was also practised in different positions, with the results now to be detailed.

Second Series of Experiments.—November 17, 1847.

Experiment 10.—Mr A——, æt. 22, had never inhaled previously. *Posture.*—Sitting. *Time.*—After inhaling 35 secs. he became red in the face, and very agitated. In 38 secs. he commenced talking an unintelligible jargon, which he continued until 1½ min., when, after struggling somewhat, and foaming at the mouth, he fell back on a sofa near him, perfectly quiet and insensible. The application of wires connected with a galvanic battery, which drew considerable sparks from the face and head, produced no signs of sensibility.

The *pulse*, which was 76 before the commencement of the experiment, rose shortly after to 84. During the period of unconsciousness, it fell to 48. On his recovery it rose to 60, and, when counted twenty minutes afterwards, was found to be 66.

The *pupil*, at the commencement of inhalation, was of medium size. In 30 secs. it became slightly dilated, and continued so until he recovered intelligence, when it returned to its original size.

Skin.—There was evident diaphoresis.

Respirations.—Owing to the agitation and struggling, the respirations could not be counted; but, during the unconsciousness, they were accompanied by loud stertor.

Sensations.—At first, experienced a choking sensation. He then saw every thing green, and afterwards yellow. He remembered nothing more, and did not feel the application of galvanism.

In this experiment the effects produced seemed to be very serious. There was intense congestion of the scalp and face, foaming at the mouth, stertorous breathing, and unusually slow pulse. He presented all the symptoms of apoplexy. He also recovered slowly.

Experiment 11.—Dr W——, æt. 23, has inhaled both ether and chloroform, and is generally much excited.

Posture.—Sitting.

Time.—In 37 secs. laughed, struck his thighs, and exhibited signs of great gaiety. In 2 mins. he appeared to be perfectly unconscious; he breathed stertorously; all the muscles of the body became rigid; and he was drawn backwards, as in slight opisthotonos. He continued in this state 2½ mins., during which time strong galvanic shocks were given, without producing any signs of sensibility. In 5 mins. he rose up.

The *pulse*, at the commencement, was 76, but soon rose to 84. When felt at two intervals during the tetanic rigidity, it was 92, and, after galvanism was applied, it was 120; 11 mins. after the commencement of the experiment, it was 104.

The *skin* was not affected, and the number of respirations, and the state of the *pupil*, could not be observed.

Sensations.—He felt, at first, the most delightful feelings, and saw prismatic colours surrounding objects. He heard a ringing in his ears, and then became unconscious. He felt no galvanic shocks, but experienced fatigue for some time afterwards.

In this experiment some of the symptoms were also apparently serious, especially the stertorous breathing and opisthotonos. They were, however, accompanied with acceleration of the pulse, instead of retardation, as in the last case; and it is worthy of remark, that in the former experiment the symptoms were decidedly cerebral, whilst in the latter they were as distinctly spinal.

Experiment 12.—A young man, æt. 18, a printer by trade, being anxious to have a tooth extracted, was the subject of this experiment.

Posture.—Sitting.

Time.—In 1 min. and 18 secs. the handkerchief was withdrawn, and galvanism applied, which appeared to be felt. Inhalation was then continued 30 secs. more, when he became perfectly unconscious. The tooth was now extracted by Dr Imlach. In 2 mins. and 18 secs. he again appeared to feel the galvanism. In 3 mins. and 18 secs. he had recovered.

Pulse at the commencement was 120, and when he was insensible fell to 108, at which it remained until the tooth was extracted, when it fell to 84.

The *skin* was rather moist during the experiment, but the *pupil* underwent no change.

Sensations.—Stated that he saw, felt, or heard nothing. He was unconscious of the application of galvanism, although from the contortions produced they evidently affected the muscles strongly.

In this experiment, no unpleasant symptom was produced, and evidence was afforded that the cerebral functions were first affected, and subsequently the spinal ones. On his leaving the room, it was discovered that the bladder had been evacuated, a circumstance which was only observed in this individual.

Experiment 13.—Mr N—, æt. 26, has previously taken ether, and always been very violent, with stertorous breathing.

Posture.—Lying down.

Time.—In 35 secs. he struggled a little. In $1\frac{1}{2}$ min. was perfectly unconscious, and breathing stertorously. The inhalation was then discontinued. In 2 mins. galvanism was applied without producing any effect. In 3 mins. it produced contortions of the face. In 5 mins. 45 secs. he became conscious, and in $6\frac{1}{2}$ mins. sat up.

Pulse.—At the commencement 92; rapidly rose to 108. When he was breathing stertorously, it fell to 60, and continued so until consciousness returned, when it rose to 72.

There was no change produced on the *skin*, and the *pupil* during the stertor was dilated, and continued so.

The *respirations* at the commencement of the experiment were 35, and rose during the stertor to 42.

Sensations.—He experienced a strong pulsation in his head, and heard a loud buzzing noise. He saw and felt nothing, with the exception of slight suffocation at the commencement.

In this case the stertor, and the apparent oppression on the brain, were well marked. He was in no way violent, as he had formerly been; a circumstance which the committee attributed to the reclining position, but which he himself explained, by saying that his conduct was greatly under his control.

Experiment 14.—Mr M—, æt. 20, had inhaled chloroform previously.

Posture.—Lying.

Time.—At the commencement of the inhalation there was frequent cough. In 1 min. 15 secs. was perfectly unconscious to galvanism, and snored deeply. In 4 mins. he opened his eyes, and appeared conscious. In 15 secs. more, spoke; and in 5 mins. 15 secs. sat up.

Pulse.—At the commencement 84—immediately rose to 110. During the stertor it was 120 and small, but shortly after fell to 108.

The *skin* and *pupils* underwent no change.

The *respirations*, immediately after the inhalation, were counted to be 25. During the stertor they were 23.

Sensations.—Saw the gas light of a blue colour, and felt a roaring in the ears, like a railway train passing through a tunnel. He had felt nothing; but his sensations, so far as he could recollect, were most delightful.

With the exception of the stertor, this gentleman appeared to be in a healthy sleep. To ascertain whether these effects were attributable to position, Dr W—, the subject of Experiment 11, again inhaled in the recumbent position, and with a determination to control the excitement as much as possible.

Experiment 15.—Dr W— again.

Posture.—Lying.

Time.—In 30 secs. laughed, slapped his thighs, and was gay as formerly, but not so extravagantly. In 1 min. fell back, and stretched himself out. The

muscles, though not flaccid, presented none of the rigidity as formerly, and there was no opisthotonos. In $1\frac{1}{2}$ min. was quite insensible to the effect of galvanism, and breathed stertorously. In 3 mins. sat up. In 4 mins. stood up and staggered about the room, as one intoxicated. In $4\frac{1}{2}$ mins. he recognised the hour by his watch, but the excitement continued some time afterwards.

Pulse—84 after the first inspiration.

On this occasion, although the symptoms of excitement in the brain were well marked, they were evidently not so intense as formerly. The spinal symptoms also were not present.

Experiment 16.—Mr A—, æt. 19, had often taken ether and chloroform. Dose, 20 drops.

Posture.—Lying.

Time.—In 30 secs. he rolled off the sofa, and lay upon the ground insensible. In 1 min. and 15 secs. he sat up.

Neither the *pulse*, *skin*, *pupil*, nor *respirations* could be observed.

Sensations.—He experienced dizziness in the head, and ringing in the ears, followed by a pricking sensation like that of needles, which went down to legs and feet. He then became unconscious.

Experiment 17.—Mr B—, æt. 20, also inhaled only 20 drops.

Time.—He inhaled $1\frac{1}{2}$ min. without any apparent effect. The handkerchief was removed for 6 secs., and then reapplied. In 36 secs. more he was insensible to the action of galvanism. This continued 1 min., when he gradually recovered.

Pulse—At commencement 96; rose immediately to 132. During the insensibility was 120, and at the end of the experiment sunk to 108.

Skin—Was covered with slight perspiration

Pupil—Underwent no change.

Sensations.—He felt dizzy, and experienced an oppression at the chest; he then saw, heard, or felt nothing.

At the termination of this series of experiments, several members of the committee inhaled from the handkerchief, on which 20 drops of chloroform had been poured. They all sat upon the sofa, and held the handkerchief in their own hands. The effect in nearly all was great excitement of various kinds. In most, laughter and extravagant gaiety; one gentleman imagined himself to be at a public dinner, listening to a speech which he applauded and cheered with the most extraordinary enthusiasm. In another gentleman, however, Dr R., who was of full habit of body, æt. 38, the inhalation produced such pain and giddiness in the head, that he could not continue it. Another gentleman exhibited no excitement, but seemed as if in a tranquil sleep.

The next series of experiments were performed on animals.

Third Series of Experiments.—November 20, 1847.

Experiment 18.—A lively leech was put under a small bell glass, in which 20 drops of chloroform had been diffused. In 2 min. and a half it became perfectly still and rigid, as if dead. Some minutes after, on the application of galvanism, slight movements were made, but it never recovered.

Experiment 19.—A large and vigorous frog was placed under a large bell glass, in which 30 drops of chloroform had been diffused. At first, no change seemed to be produced, but gradually his movements became less lively. In 40 seconds they were slow, and in $1\frac{1}{2}$ min. he became immoveable, as if dead; 3 minutes afterwards, on being removed from the glass, the application of galvanism produced a strong tetanic spasm. In half an hour he had so far recovered as to be able to crawl somewhat, but next morning he was found dead.

Experiment 20.—Another active and larger frog was placed in the same jar, in which only 10 drops of chloroform were diffused. In 30 seconds the move-

ments were less lively. In 4 min. 10 seconds they became very feeble. The glass was now removed. Galvanism produced a powerful tetanic spasm. In 20 min. the animal had sufficiently recovered to crawl about, and on the following morning was found lively and active.

Experiment 21.—Another frog was put under the same bell glass, in which 3j of ether was diffused. In 5 min. he was reduced to nearly the same state of weakness as the animal in the last experiment. On the removal of the bell glass, he rapidly recovered, and in 15 min. leaped vigorously.

Experiment 22.—A pigeon was put into a large bell glass, filled with atmospheric air, and confined there 2 mins. without its producing any effect. A fresh pigeon was then put into the same glass, in which 10 minims of chloroform had been diffused. In 1 min. he fell down, the eyes closed, the feathers on the head and back stood up. In 2 min. the glass was removed. Galvanism caused the eyes to open, and the animal to shake its head. In 6 min. it began to revive, got up, and in 30 seconds more walked away.

Experiment 23.—Another pigeon was placed in the same jar, in which 3j of chloroform was diffused. In 30 seconds the eyes closed. In 40 seconds he fell down and could not be roused. The respirations were 108 in the minute. In 2 min. and a half they were 144. In 3 min. the respirations were invisible; and on removing the glass, in 3½ min., the animal appeared to be dead, and never recovered.

Experiment 24.—3j of chloroform was poured on a handkerchief and held to the face of a rabbit. In 80 seconds he fell down perfectly insensible. In 2 min. the respirations were 96. The handkerchief was then removed. In 3½ min. it endeavoured to get up, but was evidently too feeble. In 4½ min. walked without difficulty.

Experiment 25.—A handkerchief, on which 25 drops of chloroform had been poured, was held to the head of a hen. In 30 secs. it was perfectly insensible. In 2 min. the respirations were counted to be 30. They then became invisible, and the animal appeared dead for 3 min. longer. It then opened its eyes, and almost immediately got up and walked.

Experiment 26.—90 drops of chloroform were placed upon a handkerchief, which was held to the face of a rabbit. It struggled violently at first, but became insensible in 65 seconds. The respirations then were 36 in the min. 3j more of chloroform was then put on the handkerchief. The respirations immediately became irregular and more rapid. In 3½ min. slight convulsions of the hinder legs occurred. In 4 min. no movement or respiration could be observed. Galvanism produced slight spasms, but the animal never recovered.

Experiment 27.—3ij of chloroform, on a handkerchief, were applied to the face of a small cur dog. In 1 min. and 40 secs. it was perfectly insensible. The legs were rigid, and he uttered short suppressed cries. In 2½ mins. he ceased crying. The pupils became dilated; the respirations were 36 in the minute, and the limbs became placid. 3j more of chloroform was then poured on the handkerchief. In 4 mins. from the commencement of the experiment, slight spasms of the hind legs occurred, and the heart's action became very rapid. In 7 mins. 3j more of chloroform was poured upon the handkerchief, and in 9½ mins. no respiration or heart's action could be discovered with the stethoscope—the animal was dead.

On examining the bodies of the pigeon, rabbit, and dog, thirty hours afterwards, it was found that the right cavities of the heart were distended with blood, which was firmly coagulated. The lungs and the other organs were perfectly healthy.

Another series of observations consisted in examining the bodies of six rabbits, to whom poisonous doses of chloroform had been given in various ways.

Fourth Series of Experiments.—December 14, 1847.

Experiment 28.—A strong rabbit was killed, by causing him to inhale 3ij of chloroform. On its first application he struggled a little, but speedily lost sensation. The inhalation was continued, and the respirations became accelerated. In 2 mins. there were slight spasms of the hind legs. There were convulsive heavings of the chest, which, in 30 secs. more, ceased. The heart's action, however, continued; but, in $3\frac{1}{2}$ mins. from the commencement of the experiment, these also ceased. The animal was dead.

On examining the body four hours afterwards, the right side of the heart was found inordinately distended with dark, firmly coagulated blood. Both lungs were intensely injected, and of a dark colour. The brain and other organs were healthy.

Experiments 29 and 30.—Two other rabbits were treated in exactly the same manner, and with the same results.

Experiment 31.—3j of chloroform was injected, apparently down the throat of a rabbit, with a common syringe. He died almost immediately, without any previous spasms.

On examining the body four hours afterwards, both lungs were dark coloured, solid, condensed, exactly resembling the liver of the same animal, except at the margins, where a fringe of spongy lung surrounded the organ. On section, they yielded on pressure a quantity of chloroform. The right side of the heart was moderately distended with dark, coagulated blood. The brain was healthy. The stomach was distended with food; its lining membrane healthy. No trace of chloroform could be discovered in it.

From the results of this examination, it was evident that the chloroform had been injected into the lungs, and not into the stomach as was intended. The nature of the change produced in these latter organs was not ascertained.

Experiment 32.—3j of chloroform was injected into the right jugular vein of a rabbit. The animal rapidly became weak, and could not support himself. The respirations became gradually rapid and feeble; but when seen four hours afterwards, he was still breathing, although very exhausted. He was now caused to inhale chloroform, and expired in one minute.

On examining the body, the blood was found to be every where fluid. The heart was collapsed and empty. The lungs, brain, and other organs healthy.

Experiment 33.—3j of chloroform was injected into the peritoneum of a rabbit. It soon produced partial coma, from which he soon recovered. Four hours afterwards it was quite lively, and apparently well. It was then killed by chloroform inhalation in 3 mins. with the usual phenomena.

On examining the body, the lungs were found perfectly healthy. The right side of the heart contained firm coagula of dark blood. The brain was healthy. The peritoneum presented its natural appearance.

The results of these last experiments suggest several others, having more especial reference to the action of chloroform on the blood, heart, lungs, &c. From some experiments on animals especially, and from the post-mortem examinations of a few individuals who died soon after the inhalation of ether, it has been said to render the blood fluid. This is very doubtful.* At all events, firm

* Since the report was read to the Society, I have, with the assistance of Dr J. Duncan, performed two experiments, in order to test the influence of ether inhalation on the blood. These distinctly show that death caused in this way by ether, does *not* destroy the coagulability of that fluid.

Experiment 34.—3j of ether was poured on some cotton wadding, and applied to the face of a rabbit. He at first struggled violently. In one minute the respirations became very rapid. In two minutes the respirations were slow and laboured. In four minutes, there were slight convulsions of the hind legs,

coagula of blood in the heart were found in every case where chloroform was inhaled, except in the one where it was directly injected into the veins. The effect on the lungs also varied considerably; in some cases they were congested and engorged, in others healthy, and this under pretty near the same circumstances. Whether this be owing to the different strengths of the animals, mode of respiration, &c., is uncertain. These results, and especially the effects produced by injecting chloroform into the peritoneum, differ considerably from those obtained by Dr Glover in 1842. But the whole subject requires to be re-investigated, and too much caution cannot be exercised in drawing conclusions from such experiments.

The physiological action of chloroform inhalation seems to be precisely similar to that of ether—producing loss, 1st, of the cerebral functions; 2d, of the spinal functions; and 3dly, of those of the medulla oblongata.

Its effects on the cerebral functions are often manifested by a degree of excitement which passes into coma.¹ At other times the comatose state is induced

and the respirations again became rapid. The convulsions continued only fifteen seconds. In seven minutes, the respirations had entirely ceased, but the heart's action continued. In eight minutes, the movements of the heart also could not be felt; the cotton was removed, and the animal put aside, as it was supposed to be dead. About one minute afterwards convulsive catchings were observed in the chest and head; the respirations gradually returned. In two minutes, it became sensible on pulling its fur, and shortly after got up. It was then killed by the continued inhalation of ether for five minutes.

Examination of the body forty hours after death.—The lungs were perfectly healthy. The right side of the heart gorged with blood, well coagulated. The brain and its membranes healthy.

In this experiment spontaneous recovery took place after a degree of exhaustion had been produced, which was mistaken for death, and after the functions of the spinal cord and medulla oblongata were apparently destroyed.

Experiment 35.—A smaller rabbit was treated in exactly the same manner as in the last experiment. There was at first struggling. It then became motionless, and the respirations were accelerated. In two minutes no respirations could be perceived. The heart's action, however, continued for nine minutes, and then apparently ceased. The cotton wadding saturated with ether was then removed. The animal was dead.

Examination of the body forty hours after death.—The left lung was engorged and congested throughout, of a deep mahogany colour. The right lung partially affected with the same lesion; about one half of its spongy texture however was healthy, presenting its usual pink colour. Right side of heart and venous trunks distended with firmly coagulated blood. Brain somewhat blanched; at least, on comparing it with the brain of the other body, it was of a distinctly lighter shade.

These experiments show that death by inhalation of ether does not affect the coagulability of the blood. They also exhibit two very different actions on the lungs. This we have also seen to occur in former experiments, (*Expers.* 26 to 33); and the circumstances which induce this engorgement in some cases and not in others, after fatal inhaling doses, are still undetermined.

¹ The word *coma* has been objected too by some, although, according to our present ideas, it would be difficult to separate the state induced by chloroform from that of coma. Assuredly a condition such as has been described, in which the strongest stimuli and even cutting fail to rouse the patient, and where stertorous breathing is present, can receive no other name. *Apathy* has been proposed, but it is something more than this. The terms sleep, lethargy, sopor, and coma, are considered by some as representing different degrees of loss of the cerebral functions. We have all degrees following the inhalation of chloroform, and any of them, with exception of sleep, may therefore be employed.

directly, the previous one of excitement either not having been occasioned, or being so transient as not to be observable. It is evidently a matter of importance, and ought to be a principle in the administration of this and similar agents, to produce suspension of the consciousness of the individual, without, if possible, causing previous excitement of the cerebral functions. This, Dr Simpson has shown, may be best accomplished by insuring quietude and giving a large and rapid volume of the vapour at first. It is probable, however, that two other circumstances may operate; viz. 1st, the will of the individual, it having been shown by experiment (*Exper. 13*), that individuals can, to a certain extent, control their excitement; and 2d, the posture of the person inhaling, the recumbent position having in some experiments (*Exper. 15*) been shown to diminish excitement where it formerly existed.

As regards its effects upon the spinal functions, we have seen that in one individual (*Exper. 11*) tetanic symptoms were produced; in another (*Exper. 12*) there was involuntary evacuation of the bladder. In females spinal symptoms are more common, the unconsciousness being sometimes accompanied by sighs, sobs, partial rigidity of the arms, more or less excitement, and a group of phenomena usually denominated hysterical. In animals spinal symptoms are only unequivocally displayed when the inhalation is carried very far, and consists of twitchings, and slight convulsions of the hinder extremities previous to death.

The action of chloroform inhalation on the medulla oblongata has only been unequivocally displayed in animals poisoned with it, by the rapidity and feebleness of the respirations, and by its effects on the heart's action. It is in this way that death seems to be occasioned; namely, by producing first, coma; secondly, asphyxia; and lastly, cessation of the heart's movements, in the same manner as occurred in the experiments of Le Gallois and Wilson Philip. In all the animals examined the right side of the heart was gorged with venous blood, a fact quite in accordance with this explanation.¹

As regards its effects on sensibility and contractility, we have no evidence that chloroform or ether inhalation affects them at all, except just before death. These two properties are adherent in the nerves and muscles respectively, which we have never seen influenced directly, but only indirectly, from chloroform or ether inhalation.

To understand the physiological action of this substance, it is necessary to remember that sensation is dependent, first, on the existence of consciousness, which is a function of the brain proper (that is, all that portion of the encephalon situated above the corpus callosum); secondly, on the integrity of the spinal chord; and, thirdly, on the integrity of the expanded filaments of the nerves, which receive the impression. Loss of sensation may be caused by injuring either of these portions of the nervous system. For, if the nervous filaments are diseased, impressions cannot be received; if the spinal chord be injured, impressions are not transmitted to the brain, and, if the brain be disordered, the consciousness of the impression is not experienced. Now, the object of giving chloroform ought to be to suspend the brain's functions without affecting the spinal chord, the medulla oblongata, or the sensibility of nerves, and thus produce loss of sensation by rendering the mind unconscious of the impressions made upon nerves. It is questionable, therefore, whether chloroform or ether should be denominated anæsthetic agents, because anæsthesia is generally understood to mean loss of sensibility in a part; whereas, in point of fact, it is suspension of the faculties of mind,

¹ The action of chloroform inhalation on the different portions of the nervous system, viz. the brain, spinal chord, and medulla oblongata, are in many cases subject to the control of the medical practitioner, and may each in their turn be made available for useful or injurious purposes. Some persons are much more easily affected by it than others; and hence its proper administration can only be fully carried out by such as are acquainted with its physiological action. Its use by the public as a means of amusement, excitement, or stupefaction, ought to be strictly prohibited.

and *unconsciousness* of external stimuli, that they produce. In man, this is rendered apparent by the fact, that the functions of circulation, respiration, uterine contractions, &c. &c., proceed during the comatose state, which could not be the case if the sensibility of the nerves distributed to those organs were destroyed. In animals similar facts may at once be demonstrated by the action of galvanism, which, when they are perfectly comatose, produces convulsions, spasms, and other reflex movements.

Another physiological question also arises; viz., is the effect on the nervous system produced by the altered action of the heart, or by any change communicated to the blood? Among the most uniform results of inhalation, are the effects it produces on the heart's action, as indicated by the pulse. In a few cases it is unaltered, and in rare instances, after having risen above the normal standard, it falls below it (*Expers.* 10, 12, 13). In two of these instances it coincided with deep stertorous breathing; so that increased action of the heart seems, at first, to produce an effect on the brain, and subsequently the depressed action of this latter organ reacts on the heart, in the same manner as occurs in cases of concussion and lesions of the nervous system. This, however, we have seen to be very rare.

The poisonous action of chloroform, as observed in animals, is precisely similar to that of a pure narcotic—such as opium, for example. In some experiments I had an opportunity of seeing performed by Professor Ludwig at the university of Marburgh, in the autumn of 1846, the very same effects as those produced by chloroform were just as rapidly occasioned in animals, by injecting a small portion of a strong solution of opium into the veins. In such cases, whilst the cerebral functions were suspended, the spinal actions proceeded without interruption, and all cutting was consequently unattended with sensation. The humanity of this procedure, as contrasted with the old method of performing vivisections, is obvious, and the great convenience of chloroform renders it highly preferable to opium, or any other means of obtaining similar results.

The nature of the change produced in the brain it is somewhat difficult to analyse, because it is not easy to separate the effects of deranged circulation within the cranium from any poisonous influence communicated to the blood itself. In every case where the brain was examined, it seemed perfectly healthy, without any trace of congestion whatever. In this respect it exactly resembled the condition denominated simple apoplexy by Abercrombie. To us, however, it seems clear, that whatever toxic influence may be occasioned, a derangement of the cerebral circulation occurs in the manner and according to the laws pointed out by Monro Secundus, Dr Kellie of Leith, and Dr Abercrombie. I shall not attempt in this Society, to which the accurate experiments and observations of Dr Kellie were originally communicated, to point out how utterly the recent imperfect experiments and observations of Dr Burrowes of London have failed to shake his conclusions.¹

The effect on the blood is not yet very accurately ascertained. I have already observed, that after death in animals it is firmly coagulated, and I have only to add, that the form and colour of the corpuscles are quite unchanged.

During the primary action of chloroform on the system, different sensations seem to be experienced. In the great majority of persons, and especially among females, they are undoubtedly of a highly pleasurable description. In some cases even, where the effects to those looking on are somewhat distressing—when, for example, sobs, tears, and crying, accompany the unconsciousness—on recovery they seem anxious to have it again, and state their feelings to have been delightful, as if they were enjoying pleasant dreams. Small doses, in men, often cause the most extravagant gaiety, bursts of laughter, humorous gesticulations, and a similar series of phenomena to that produced on inhaling nitrous oxide gas. The

¹ See an admirable article disproving Dr Burrowes' conclusions in the *Monthly Journal* for August 1846.

special senses of hearing and sight are also, in some cases, more or less perverted. Humming, buzzing, roaring, and musical noises, are heard in some; whilst in others a mistiness of view, or green or yellow coloration, are given to surrounding objects.

The effects on the contractions of the iris are, of course, in connexion with the changes produced in the brain. In many cases it underwent no alteration. In a few, especially in those who breathed stertorously, it was somewhat dilated. It was never observed contracted, as in some cases of inflammation of the brain.

Diaphoresis was observed in a few cases, but appeared to be more connected with individual peculiarities than with the physiological effects of chloroform.

The time necessary for producing unconsciousness, varied from 40 secs. to 2 mins. In the great majority of cases, the full effect was produced in from 60 to 80 secs.

The medium dose of the chloroform for producing unconsciousness in man, seems to be 3j. In a few experiments less sufficed; and in others about twenty drops more were necessary. It has been satisfactorily proved that much depends upon the purity of the preparation, and the mode of inspiration.

Some persons have employed large quantities of chloroform without producing the desired effect. In all such cases it has been impure, or the substance used has only been chloric-ether. Its specific gravity ought to be 1.480. M. Soubeiran, in a late memoir read to the Academy of Sciences in Paris, tells us that a rapid method of approximating to this, is by mixing equal parts of strong sulphuric acid and water, and allowing the mixture to cool. A few drops of the chloroform poured into the fluid ought to sink to the bottom if sufficiently pure for medical use; but if they float on the surface, the chloroform should be rejected.¹ M. Mialhe has also pointed out that chloroform, if pure, and poured into water, sinks to the bottom, preserving its limpidity; but that if it contain alcohol it is rendered more or less opalescent. It is to the presence of this principle that its irritating local properties are owing.²

Again, some persons find it very difficult to take deep inspirations when required. This fact is well known to auscultators, who have often found that, from agitation, or ignorance of what is meant, individuals sometimes repress the respiratory movements, instead of encouraging them. I have often found it necessary to teach some persons how to inspire deeply before applying the stethoscope: and attention to this circumstance, when chloroform is to be inhaled, would no doubt, in some cases, favour its action, as in *Exper. 3*.

The apparatus employed was in all cases, with the exception of one, a simple pocket handkerchief. Numerous instruments, some of a very complicated nature, have been invented for chloroform inhalation. M. Charriene of Paris has been extremely fertile in such productions, and it is argued that they save an unnecessary expenditure of chloroform, and prevent its irritant action on the skin of the face—a result which has been rarely observed. These advantages are more than counterbalanced by many inconveniences, especially the expense of preserving, and trouble of carrying an instrument, which is unnecessary. Besides, the public dislike every thing in the shape of apparatus, and are prevented by their formidable appearance from using them. Indeed, it is exactly the simplicity of the means employed in administering this agent, that has always appeared to us to constitute one of its greatest merits.

The facts determined by the committee admit of many other comments, and are suggestive of other experiments, which would require a considerable time for their completion. It has therefore been thought better to present such results as have been arrived at without delay, and whilst public interest is still directed to the subject.

It was my original intention to have entered upon the history of the inhalation

¹ L'Union Médicale, Novembre 30th, 1847.

² Read to the Academy of Medicine, Paris, 7th December 1847.

of ether and chloroform. The length of this communication, however, obliges me to be very brief on these points. To whom the merit belongs of first suggesting ether inhalation for annulling pain, can perhaps only be finally determined in America, where it was originally promulgated. But I do not hesitate to say, that, as far as I am capable of judging from the facts hitherto published, it ought to be accorded to Mr Horace Wells.

As regards chloroform, no sooner was Dr Simpson's discovery announced, than this substance appeared to have been employed experimentally and used medicinally to an extent up to that time unknown.

Thus, Dr Glover of Newcastle, in a series of experiments, recorded in the Harveian Prize Essay for 1842, pointed out its analagous action on the human economy with iodoform, bromoform, &c. In one experiment where chloroform was injected into the peritoneum, he noticed that, previous to death, the animal had become unusually insensible to the action of stimuli. With him the physiological history of this substance may be said to commence.¹ In a communication published in the *Comptes Rendus de l'Académie des Sciences*, for March 8th, 1847, M. Flourens thus expresses himself, "It may be remembered that chloric ether has given the same results as sulphuric ether. This induced me to try the new body known under the name of chloroform. In a very few minutes (in six in one experiment, and four in a second and third) the animal submitted to the inhalation was completely etherized. The spinal chord was then exposed; the posterior region, the posterior roots, were insensible. Of five anterior roots tried successively, two only preserved their motor power; the others had lost it." Hence, the peculiar effects of inhaling this substance were recognised by M. Flourens in animals, but without producing any practical results.

Therapeutically it has been used as a grateful and diffusible stimulus by various persons. By Mr Guthrie of America in 1831;² by Dr Formby of Liverpool, in 1838-39;³ by Guillot;⁴ by Bell;⁵ and probably by many others. In some of these cases, more especially that of Mr Guthrie, although the substance used was called chloric ether, it is almost certain, from the mode of its preparation and density, that it was chloroform more or less impure.

We observe, therefore, in this, what so frequently happens in the history of many discoveries, that several individuals had approached it more or less nearly; but that it was reserved for Professor Simpson of this University to perfect it at once, to discover its advantages, experimentally prove their correctness, and point out its practical application for the alleviation and cure of human suffering.

As regards these applications, they have been so fully discussed before this Society, and are extending so rapidly, that it is unnecessary for me to enter upon them. I need only say, that in all those cases where the use of ether has been beneficial, and in many others not previously thought of, the superiority of chloroform has been fully proved.

I cannot conclude without, in my own name and in that of the committee, returning our sincere thanks to Dr Simpson for the facilities he has afforded us during our inquiries. To all who take an interest in the reputation of the Medical School of Edinburgh, it must be a subject of pride and gratification to reflect, that whilst it is to America we owe the first important step in annulling pain by inhalation, and whilst in other schools only unimportant modifications of the original idea have been carried into execution, that it is to this University, through the talents and energy of one of its professors, that we owe its application to obstetric practice, that we owe simplification of the apparatus,

¹ See Monthly Journal, present No. p. 514.

² Silliman's American Journal of Science and Art, vols. 21 and 22. 1832.

³ Mr Waldie's communication on chloroform, read to the Literary and Philosophical Society of Liverpool, Nov. 29th, 1847.

⁴ Annuaire Thérapeutique.

⁵ Pharmaceutical Journal.

and proper principles of administration, and that we owe, lastly, a new agent of greater power and efficacy, the invaluable benefits of which it is impossible to calculate, and with which the name of Simpson must for ever be associated.

Dr Skae stated that he had made a variety of observations upon the effects of ether in the treatment of the insane; and that, immediately after the discovery of the anæsthetic properties of chloroform, he had also made trial of it in almost every variety of insanity. He concluded from his observations, that it had the same physiological action in persons labouring under insanity as in those of sound mind. It affected them precisely in the same manner, and with the same facility; even the most violent and excited being brought completely under its influence, in periods varying from thirty to sixty seconds. He had as yet, however, found that as a curative agent it was not more useful than the ether had proved. It differed in its effects from ether in not quickening the pulse, and in seldom being followed by any increased excitement when the sopor passed off. He had used it in cases of acute and chronic mania, epileptic mania, dementia with excitement, in various forms of monomania, and lastly, in one case of puerperal mania. In all cases the patients were rendered insensible,—the profound sopor induced generally continued for periods varying, according to the quantity inhaled, from a few minutes to an hour or more; this was commonly followed by quietude of three or four hours' duration; the patient then exhibiting all the symptoms which were present immediately before the administration of the chloroform. This was the case even with the patient labouring under puerperal mania, although she was kept under the influence of the chloroform for ten hours continuously. She awoke with the same delirium with which she went to sleep. He did not therefore anticipate much benefit from this substance as a *curative* agent in cases of insanity, except, it might be, in delirium tremens, and in cases allied to it in their character, in which a sound and prolonged sleep was generally followed by a cure. In these cases, however, he had not yet had an opportunity of testing it.

Dr Skae further remarked, that in some respects his observations differed from those given in the report. He found that the pupils, after being dilated during the first administration of the chloroform, became immediately contracted when its full effect was produced, in which state they remained until consciousness began to return. The pulse also, at the same time, generally fell rapidly to forty, or even to thirty-six or thirty; and what struck him as remarkable was, that the relation between the pulse and the respiration was entirely destroyed, the respirations becoming more frequent as the pulse fell, and being not uncommonly synchronous, or nearly so, with the heart.

Dr Allen Thomson, for the most part, agreed with the observations made in the report, but was inclined to consider that greater influence was produced directly on sensibility and contractility than *Dr Bennett* had stated. He founded this belief principally on two experiments he had made on frogs, in which he had observed that, during the comatose condition, pricking and pinching the nerves gave little signs of sensibility. He had also observed in these animals a peculiar contracted and shrivelled state of the muscles, very distinct from their smooth and polished surface when healthy.

Dr G. Paterson stated, in connexion with what had fallen from *Dr Skae*, that he had heard of chloroform having been extensively tried in another large asylum in Scotland. The results, as far as the experiments had yet proceeded, appear to have been very much of the same nature as stated by *Dr Skae*, viz. a great variety of manifestations produced by the inhalation; the anæsthetic effects not produced in a considerable proportion of cases; and in none any decided beneficial effect upon the paroxysm of mania by its use. Considerable disappointment had also been caused there by the length of time and quantity of the substance required to affect the patients. He understood, however, that the experiments were still proceeding.

Dr Simpson stated, that the results *Dr Paterson* had described were merely the effects of an imperfect exhibition of the chloroform. It had been given in small and exciting, instead of full and narcotic doses. And these results might have been obtained either by exhibiting pure chloroform imperfectly, or by using an impure form of liquid. To produce the complete anæsthetic and soporific effects of the chloroform, some conditions were necessary to be attended to. Without attending to these you will have failures, as in *Dr Paterson's* cases. And, 1. The chloroform vapour must always be exhibited as rapidly and in as full strength as possible, if you desire to have its first or exhilarating stage practically done away with, and excluded; and you effect this by giving the vapour so powerfully and speedily as to apathize the patient at once. If you act otherwise, and give it in small or slow doses, you excite and rouse the patient in the same way as if nitrous-oxide gas were exhibited. 2. In order that the patient be thus brought as speedily as possible under its full influence, the vapour should be allowed to pass into the air-tubes by both the mouth and nostrils,—and hence all compression of the nostrils, &c., is to be avoided. 3. The vapour of chloroform is about four times heavier than atmospheric air. And hence, if the patient is placed on his back during its exhibition, it will, by its mere gravitation, force itself in larger quantities into the air passages than if he were erect or seated. As to the best means of exhibiting the chloroform with these indications, *Dr S.* believed that the simple handkerchief was far preferable to every means yet adopted. It was infinitely preferable to any instrument he had seen, some of which merely exhibited it by the mouth and not by the nostrils, in small and imperfect, instead of full and complete doses; and with instruments so constructed, he had no doubt whatever that failures and exciting effects would ever and anon occur. Besides, inhaling instruments frightened patients, whilst the handkerchief did not; and mental excitement of all kinds, from whispering and talking around the patient, was to be strictly avoided, if possible. As to the quantity required to be applied to the handkerchief, it had been stated, that the committee had found the average dose of a fluid drachm generally sufficient; but *Dr S.* had latterly seldom measured the quantity he used. He judged principally, as they would all come to do, by its effects, more than its quantity. He gathered his handkerchief into a cup-like shape in his hand, and wetted freely the bottom of the cup (if he might so speak), and if the patient were not affected in a minute or so, he then generally added a little more. It evaporates rapidly; and you must not (as he had seen done) wet your handkerchief, and then delay for a minute or more in applying it. You must apply it immediately. Not unfrequently, when the patient was just becoming insensible, he will withdraw his face, or forcibly push aside the handkerchief. If you *then* fail to reapply it to his face and keep it there, you will be liable to leave him merely excited. But probably two or three inhalations more will *now* render him quite insensible. The simplest test of its full and perfect effect is some noise or stertor in the respiration. Cease it as soon as this is fully set in. But you reapply it, of course, from time to time, if it is wished to keep up its effects.

Dr Bennett, in his report, had spoken of the stertor or some other symptom being “serious.” Now, this and other terms were, he believed, calculated to excite unnecessary fear. “Serious” was a relative and conventional term, constantly liable to be altered by increased knowledge and experience. Twenty years ago, travelling at the rate of thirty miles an hour would have been reckoned a very serious matter. Now-a-days every one knew it was not so. The tyro looks at first upon the symptoms of an aggravated attack of hysteria as very serious. The physician of more experience knows they are not so. The stertorous breathing, the spasms, and almost convulsive symptoms, &c., sometimes produced by chloroform, may appear serious to those who have had little experience in the use of this agent. But every one who has seen much of its effects, knows that there is not only no danger following, but no inconvenience even left by such a show of serious symptoms.

Dr S. stated that the strength and the purity of the chloroform employed were essential elements of success. Professor Gregory had examined about a dozen specimens which he had procured from various shops, here and in Glasgow. Several of them were by no means of the standard strength. A medical friend now present had, two days ago, asked him if he ever had failed obtaining the effects of chloroform upon any person. He replied, never in any one case. His friend added that he had, the night previously, been unable to influence a parturient patient, though he had given her an ounce. On inquiry, Dr S. found he had used chloroform from a shop where, according to Dr Gregory's researches, it was sold under a specific gravity of 1.200, instead of being 1.480. And Dr S. would beg further to observe, that insane patients—such as those reported on by Dr Paterson—were not more difficult to effect than others. In various trials at Morningside, at which Dr Christison, Dr Skae, and Dr Wingett were present, Dr S. had set violent patients over into a soporose sleep in a minute or less. As to what its therapeutic effects, if any, might be in insanity, we had, he believed, no power yet of judging. It could not be expected to be of any marked service—at least, in any short time—in such chronic cases as it had been tried in. But it had this effect; you could keep the patients asleep under it for a long series of hours. In this way, it had already apparently cured some cases of delirium tremens, and, he believed also, of puerperal insanity, and might yet be found useful in other forms of acute mania. At all events, it was a means of restraining a furious maniac; as powerful, and, perhaps, it would be found, far more safe, than a strait-jacket, or the grasp of a number of keepers. Once set a patient over (and that was the work of a minute), and a nurse with a pocket handkerchief and some chloroform, might keep him under perfect and complete restraint. Nor need he add how useful the same means might be in enabling a riotous and resisting patient to be removed to an asylum, or from one place to another. It would be tedious to discuss all the other diseases in which it had already been tried. Dr S. was most anxious to observe its effects in acute local inflammations, but he had, as yet, had few opportunities of doing so. He mentioned a case he had seen with Dr Dease (apparently an extra-uterine conception bursting into the abdomen), and where the accompanying severe abdominal pain, and its dreadfully depressing effects, were kept at bay by the anæsthetic and contro-stimulant effects of the chloroform. He mentioned a case of extremely severe cholera, under Dr Paterson's care at Leith, where, after all things had failed, and the patient was apparently sinking, the inhalation of chloroform had induced sleep—dispelled the spasms and vomiting—and restored the patient. In conclusion, Dr S. briefly alluded to the various claims which had been brought forward as to the discovery of the effects of chloroform.

Dr Alexander Wood narrated the case of puerperal insanity alluded to by Dr Simpson, which had occurred in his practice.

The patient had been brought to Edinburgh, a distance of thirteen miles, four days after the seizure. She was exceedingly violent and unmanageable, and had bit her husband's cheek. After the exhibition of chloroform, she was put to bed as easily as an infant. The necessary restraint applied without irritation, and a refreshing sleep enjoyed. This took place on the 1st of December, and she left town cured on the 10th.

The experience of Dr Skae entitled his opinion, as to the improbability of chloroform being a cure for insanity, to great weight; but surely it was of immense importance to be put in possession of an agent by which the most violent patients might be successfully controlled without any force whatever.

Dr W. thought it unnecessary for Dr Simpson to vindicate his claim to be the first to apply this agent. The voice of the profession had justly awarded it to him, and none of the paltry attempts which had been made to undermine his claims were likely to have the slightest influence.

Dr W. had been led to entertain doubts as to the safety of ether inhalation in obstetric practice. Chloroform seemed much more safe. Dr Simpson had formerly shown, that the more rapidly the ether took effect the less risk there was of

disagreeable consequences. As chloroform was so much more rapid in its effects than ether, by the same law it was less liable to act injuriously.

Dr Bennett rose to reply, when *Dr Macaulay* moved that the Society adjourn, —and it was adjourned accordingly.

LISTON AND DIEFFENBACH.

WITHIN four weeks of each other we have to deplore the deaths of Liston and Dieffenbach, both of whom acquired a degree of professional renown that entitle them to consideration in our pages.

ROBERT LISTON was born on the 28th of October 1794, in the Manse of Ecclesmachan, Linlithgow. His father, the Rev. Henry Liston, was the minister of that parish, and distinguished for his musical acquirements and classical knowledge. Under his tuition Robert became a sound Latin scholar; so that, on subsequently attending the humanity classes in Edinburgh, he obtained a prize for composition in that language. In 1810, he commenced the study of medicine by attending the lectures of *Dr Barclay*, who afterwards appointed him his assistant, in which capacity he distinguished himself as a skilful anatomist. During the subsequent six years he followed the usual course of medical education, and went through the office of dresser, clinical clerk, and house clerk in the Royal Infirmary. In 1816 he visited London, where he studied a year under *Sir William Blizard*, and *Mr Thomas Blizard*, of the London Hospital, and *Mr Abernethy* at St Bartholomews. He became a member of the Royal College of Surgeons in London, and subsequently returned to Edinburgh, where he established himself. He was elected a Fellow of the Royal College of Surgeons of that city in 1818, writing for his probationary essay, “On Strictures of the Urethra, and some of their consequences.”

Mr Liston now commenced his career as a lecturer on anatomy and surgery, and early became remarkable for the boldness and skill with which he operated. At that time, surgery was not so advanced in Edinburgh as it afterwards became; and many cases considered incurable by the officers of the Royal Infirmary, fell into his hands on being discharged from that institution, and were by him made subjects of highly successful operations. His rivals charged him with inveigling patients out of the Infirmary for that purpose, a charge which he denied, boldly asserting his right to operate on, and benefit those who had been considered incurable by others. One case especially excited great attention. It was that of a tumour firmly attached to the scapula; which, after a consultation of the surgeons of the Infirmary, was considered an improper subject for operation, and discharged. *Mr Liston* afterwards resolved on removing it, together with a part of the shoulder blade, an operation which he performed on the 16th of November 1819. Unfortunately the disease returned in the portion of the bone which was left, just as the wound was well; when *Mr Liston* proposed the removal of the diseased bone and arm, as the only means of saving life. No surgeon in Edinburgh, however, would assist him in the operation, and he was obliged to abandon the poor boy to his fate. He died three months after, and the surgeon in *Kinross* who examined the body, on forwarding to him the diseased part, stated, “We performed, in its removal, precisely the operation which you proposed to save the unfortunate lad; and I now regret that it was not done when he was last under your care. The disease, you will perceive, had no connexion with any of the vital organs.”

This, and several other cases of a similar kind, gave rise to great discussion in the Infirmary, and it was proposed to him on the part of the managers, that he should refuse his assistance to any person who had been a patient in that institution, and that he should abstain from visiting or attending the wards. He declined both these propositions, and, in consequence, was expelled, and never entered its gates for a period of five years. At the expiration of that time, how-

ever, in 1827, he was suddenly elected one of the surgeons, not owing to any merit he possessed as an operator, but to some private influence which he skilfully forced to be exerted in his favour.

There was now opened to him a field for the proper display of his powers, and the boldness, skill, and rapidity of his operations acquired for him a European reputation. Who that once saw Liston perform a capital operation could ever forget it? The manner in which he handled the knife had in it something remarkable. It at once put the spectators at their ease, and convinced them that the instrument would not go any where, or do any thing but what it ought. He possessed a confidence and self-possession that no untoward event ever disturbed; a readiness in suddenly applying expedients, that seemed to the inexperienced the result of forethought; and a power in his muscular arm and bony hands that enabled him to twist, depress, or elevate parts, with an ease that appeared almost magical. As a lithotomist he has never been surpassed, and his flap operations were performed with such rapidity, that the sound of sawing seemed to succeed immediately the first flash of the knife. With him the length of operations were estimated by seconds instead of minutes, and the student who unwarily turned his head to address his neighbour, on looking round discovered that what he had struggled so much to witness, had already been performed. In all this, however, there was no real hurry; it was the result of a skilful combination of movements, performed with precision and exactitude. In more complicated operations, when he wished to use both hands, he saved much time holding the knife by the handle in his mouth, instead of entrusting it to an assistant, or laying it down, a habit he was led to adopt from having carefully watched the manner in which the butchers of Edinburgh rapidly separated different joints of meat.

As an example of the readiness with which he encountered difficulties, we may relate the following incident, communicated to us by one who was present. He had amputated a leg for extensive necrosis and chronic disease of its bones. The patient was feeble, and yet, after securing many vessels, hemorrhage was still abundant from the cut surfaces. More vessels were secured, but the bleeding continued, and Professor Russell, who was assisting him, became alarmed lest the patient should sink on the table. At this moment it was discovered that the blood oozed from an enlarged vessel in the substance of the bone. There was a panic of a few seconds, and every one whispered, what was to be done? Liston immediately sliced off a piece of wood from the operating table with his large amputating knife, and rapidly formed it into a plug, which he thrust into the mouth of the bleeding vessel. The hemorrhage ceased immediately, and the patient recovered.

In 1835, he accepted the invitation of the Council of University College to fill the chair of Clinical Surgery, and left Edinburgh for London. He soon obtained a large practice, and, on the death of Sir Anthony Carlisle in 1840, was appointed member of the council of the college of surgeons. In 1846, he became one of the board of examiners, and had thus reached almost the highest honours of the profession in the capital of the empire when his life terminated.

In the spring of 1847, he complained of constriction in the larynx, a sense of choking on stooping forwards, and difficulty of deglutition. Late in July, he coughed up between thirty and forty ounces of blood. He himself hinted at the possibility of an aneurism existing; but there were no physical signs of such a lesion perceptible to his medical attendants. In the beginning of October, the cough returned, became gradually worse, and was attended with rusty-coloured sputa. On the 1st of December, he was seized with what appeared to be spasmodic asthma. Attacks of dyspnoea supervened, which, during the six following days, gradually became more frequent and intense, and produced such exhaustion that he died on the evening of the 7th. On examination, an aneurism of the arch of the aorta was discovered, pressing upon the trachea, the mucous membrane of which was perforated by ulceration in three or four places, although the apertures were blocked up by the coagula in the sac of the aneurism.

Mr Liston's contributions to the literature of his profession were not very numerous. His first publication is entitled "Memoir on the Formation and Connexions of the Crural Arch, and other parts concerned in Inguinal and Femoral Hernia. 4to, 1819, with three plates." He communicated, also, to the "Edinburgh Medical and Surgical Journal" some papers, the first of which was inserted in the 16th volume of that periodical, and consisted of cases of aneurism, and an account of a case of fracture of the neck of the femur, in which a bony reunion had taken place within the capsular ligament. Other later Memoirs were inserted in the Transactions of the Royal Medico-Chirurgical Society of London. In 1831, he published his "Elements of Surgery," and, in 1837, the "Practical Surgery," a work which has gone through four editions, and is justly esteemed as one of the best guides on the subject of which it treats, now extant.

The improvements he introduced into the art of surgery, though few, were not unimportant. To him the surgical school of Edinburgh is indebted for the simple mode of dressing wounds which at present prevails, by lint saturated in water and covered with oil silk, instead of the greasy applications, strapping, and continued poulticing, which formerly prevailed. He was a great simplifier of instruments, his profound knowledge of anatomy enabling him to guide the straight bistoury with the utmost precision in the deepest and darkest recesses of the body. The bone nippers which he invented has every where been recognised as a most invaluable instrument to the surgeon, and, in his powerful hands, did wonders. We have seen him cut through the femur of a child with them at once, without any apparent effort. His operations also materially conduced to the introduction of the flap method of performing amputations, on which subject he wrote a paper. Such, we believe, are the principal claims he has upon the regards of posterity. The fact is, that celebrity with him did not so much depend on what he did, as on how it was performed. None possessed more practical tact, formed juster conclusions, and treated a case more appropriately, than Mr Liston. All this he did intuitively, often without being enabled to give reasons for his mode of acting—a circumstance which caused him to make a better appearance at the bedside, and in the operating theatre, than in the lecture room. His medical treatment consisted principally in the exhibition of wine, antimony, and quinine, three remedies which he employed with consummate skill. We must not forget to mention that Mr Liston has the merit of having early perceived the great advantages of investigating the minute structure of tissues and morbid growths by means of the microscope. On this subject he was quite *au courant* with the present state of science, and communicated to the Medico-Chirurgical Transactions of London an interesting paper on the vessels of lymph.

In private life Mr Liston succeeded in forming many lasting friendships. It cannot be denied, however, that he frequently exhibited to those around him a degree of temper and coarseness of language which was often painful to witness. His apprentices, dressers, and clerks, however, got accustomed to this, and he had the art, by a subsequent degree of condescension, or a well-timed invitation to dinner, to make them forget his ill treatment. He was exceedingly fond of nautical pursuits, and of hunting. When in Edinburgh he frequently followed the hounds, and, although a heavy weight, made a good appearance in the field. In London he kept a yacht on the Thames, in the relaxations of which he indulged up to a few weeks of his death. He was buried at the Highgate cemetery, near which the hearse was met by upwards of four hundred of his pupils and his friends. These attended the body to the church, and from thence to the grave, where nearly three thousand persons, it is said, "were collected to pay their last testimony of respect to one whom, when living, they had been, in most instances, indebted for relief from personal suffering."

JOHN FREDERICK DIEFFENBACH was a native of Königsberg, and son of a professor of theology in the university of that city. Destined in early life for the church, Dieffenbach commenced his career as a student of theology, to the study of which, however, he seemed disinclined; for, at the age of eighteen, he volun-

teered into the Mecklenburg cavalry, and remained in that regiment during the last campaign for the deliverance of Germany. The war being ended, he resumed his theological studies, but soon devoted his talents to the study of surgery, under the tuition of Professor Walthus, in the university of Bonne.

In 1822, at the age of twenty-seven, he went to Paris and became the pupil of Larrey and Dupuytren. His inaugural dissertation before the faculty of Wurzburg, on the transplantation of parts of the human body, seems to have been the first evidence of his partiality for that branch of surgery for which his name afterwards became so justly celebrated—rhinoplastic surgery.

On leaving Paris, he established himself in Berlin, where, from some interesting works which he published, and from his talents and extreme ingenuity as an operative surgeon, he very rapidly attracted notice. In 1830 he obtained a surgical appointment in the hospital of La Charité of Berlin, and at the death of Graefe in 1840, he succeeded that surgeon as clinical professor; in the hospital attached to which professorship lay the scene of most of his bold and ingenious operations, of his professional instruction, and lastly, of his sudden death.

On the day of his demise Dieffenbach appeared to be in the enjoyment of his usual vigorous health, and at his accustomed hour repaired to the hospital, where, seated on his sofa, he was conversing with two French physicians regarding a case of aneurism, on which he had operated the day previously. Suddenly he ceased speaking, his head fell forwards, and he died instantaneously.

The respect and veneration in which this great surgeon was held, was fully testified by the vast crowds which accompanied his remains to the place of burial.

The funeral procession, headed by a large volunteer band of musicians, consisted of the army surgeons, and many officers in full uniform, bearing the honours and decorations which had been conferred on the deceased; of the city clergy, the relations and private friends of the family; the rector and students of the university; the pupils of the Friedrich Wilhelm Institution, and the medical men of Berlin. The state carriages of the King and Prince of Prussia, with a long line of private equipages, following the empty carriage of the deceased, closed the imposing and melancholy cortège. As the coffin was borne along through the streets, quantities of flowers and wreaths were showered over it by mourning citizens, who were assembled in vast numbers to pay this last touching tribute of respect to their distinguished surgeon.

In Dieffenbach the profession has lost a surgeon, whose fertile imagination, combined with neatness of execution and untiring zeal, has conferred on surgery much that has been of the greatest value; and although we have now to lament his being removed in the midst of his usefulness, still he has opened up paths in a new and till of late little explored branch of surgery, which will lead others to follow with increased zeal in his footsteps.

Admirable in all departments of surgery, Dieffenbach ever shone conspicuously in the surgical treatment of malformation, to which he devoted the greater part of his attention. He first performed the operations for the cure of squinting and stammering, and of the sub-cutaneous division of many muscles for the relief of deformities. The works which he has written, though elaborate, and containing much valuable practical matter, convey but an inadequate idea of his operative powers, and of his acute discernment in these, his favourite subjects of surgical treatment. He was remarkable for the ingenuity with which he planned and carried into execution many of the most difficult rhinoplastic operations. By those who have observed the precision with which he performed these operations; who have been eye-witnesses to the dexterity and coolness with which he would change his plan of execution in the middle of an operation, when others would have been embarrassed by the difficulties in which they had involved themselves; and by those who have watched the admirable results which have followed such operations in his hands, could Dieffenbach only be fully appreciated. No deformity ever baffled him. He delighted to restore the use of their limbs to those halt and maimed, whom others had given up as hopeless. The greater the amount of malformation, the more zealously did he strive to overcome the difficulties

which such cases presented. When foiled in his first attempts he returned with greater eagerness to a second operation, and the case indeed was hopeless which Dieffenbach would relinquish. The largest work he has left us is upon this subject, namely, an 8vo volume, with an atlas of plates, entitled, "Chirurgische Erfahrungen, besonders über die Wiederherstellung Zerstörter Thiele des menschlichen Körpers nach Neuen Method," 1829. He contributed also several papers to the periodical press. An abstract of his last Memoir will be found in the preceding Number of this Journal.

As a clinical teacher, he was much sought after, and listened to with eager and profound attention by students of every nation. It was in his *clinic* indeed that he was seen to most advantage, which consisted, according to the German plan, in interrogating his pupils, instructing them practically in the steps of an operation, and often making them perform it before the class, under his superintendence. He was on all occasions the student's friend, and their sense of his friendship, which was evinced during his life by frequent tributes of respect shown to their admired teacher, was testified in a gratifying manner by the crowds who followed his remains to the tomb.

In the accounts given of Dieffenbach's death, apoplexy is said to have been the fatal disease; but the very sudden manner in which death occurred, renders this improbable, and we regret to see no account of a *post-mortem* examination having been made.

Such is a slight sketch of the career of two distinguished surgeons; both the sons of divines; both university professors; both distinguished as the most skilful operators of the day; both largely entrusted with the public confidence in populous capital cities; both surrounded by all the honours and emoluments their respective countries bestow; and both dying at the comparatively early age of fifty-three. So far there is a very singular coincidence in their respective careers. Did we draw a contrast between them, we should be inclined to say that Dieffenbach possessed a more inventive genius, had greater perseverance, and was more distinguished as a professor than his English rival. Liston, on the other hand, far surpassed the German professor in manual dexterity, boldness of execution, and as a bedside practitioner. Both undoubtedly were great surgeons, and have exercised a most important influence on the art to which they directed their energies and talents.

PREVAILING DISEASES IN EDINBURGH.

EDINBURGH, like most large cities, has been very universally affected with influenza, followed by an unusual degree of prostration, frequently terminating in fatal attacks of serous and mucous inflammations. The mortality in the hospital from this latter cause has been very great. Among the numerous *post-mortem* examinations which occur, it is singular to observe the frequent coexistence of pleuritis, pericarditis, and peritonitis in one patient, and of pleuro-broncho pneumonia in others. The typhoid fever, with intestinal ulcerations, so common last spring and summer, has now given place to the ordinary Edinburgh typhus, the number of which cases admitted into the Infirmary are at present sensibly on the decline. Typhous deposits in the spleen are still commonly found in these individuals after death. Dysentery, which was so common during the autumn, is now rarely met with.

MR LISTON'S SUCCESSOR.

THE Council of University College have invited Mr Syme to fill Mr Liston's vacant chair. That invitation has been accepted, and the Medical School of Edinburgh will shortly lose one of its most distinguished professors. During the last fifteen years the reputation of the University Surgical Clinic has gone on steadily increasing; the continental system of practical instruction has in a great measure been introduced; and at this moment we believe that the efficiency of the

class, together with the sound and interesting instruction communicated to it, is not surpassed, if it be equalled, in any European school. This, in our opinion, is not owing merely to the talents of the professor, but to the method he has introduced,—a method which, we think, ought to be extended to all kinds of clinical instruction whatsoever. We trust that Mr Syme's successor will have the good judgment to follow his plan. True clinical instruction consists, not in displaying the knowledge of the teacher, but in stimulating and calling forth knowledge in the pupil. This is only to be done by demonstrating to them disease; by bedside conversations, and an earnest desire to secure the interest of the student by communicating to him really practical information. The Council of University College have exercised a sound discretion in inviting Mr Syme to London; and we shall be greatly deceived if he be not soon as highly appreciated by his new pupils, as he is by his present class and the large circle of friends he will leave behind him.

BOOKS RECEIVED.

1. Contributions to the Pathology and Practice of Surgery. By James Syme, F.R.S.E., &c. Edinburgh. 8vo. 1848.

2. An Essay on the Use and Abuse of Restraint in the Management of the Insane, &c. By Hamilton Labatt, A.B., &c. Dublin. 8vo. 1847.

3. A New Theory of Vegetable Physiology, based on Electricity, and substantiated by Facts, with its Application to Agriculture. Edinburgh. 12mo. 1847.

4. Elements of General and Pathological Anatomy, presenting a view of the present state of our knowledge in these branches of science. By David Craigie, M.D., F.R.S.E., &c. Second Edition. Edinburgh. 1848.

5. Researches into the Pathology and Treatment of the Asiatic or Algide Cholera. By E. A. Parkes, M.D., &c. London. 8vo. 1847.

6. A Treatise on Diet and Regimen. By W. H. Robertson, M.D., &c. Fourth Edition. Part IV. London. 8vo. 1847.

7. Speech of the Count de Montalembert, in the House of Peers of France, on the subject of Medical Reform, &c. London. 8vo. 1847.

8. Remarks on the Superinduction of

Anæsthesia in Natural and Morbid Parturition; with Cases illustrative of the Use and Effects of Chloroform in Obstetric Practice. By J. Y. Simpson, M.D., &c. Edinburgh. 8vo. 1847.

9. Answers to the Religious Objections advanced against the Employment of Anæsthetic Agents in Midwifery and Surgery. By J. Y. Simpson, M.D., &c. Edinburgh. 8vo. 1847.

10. The Principles of Nature, her Divine Revelations, and a Voice to Mankind. By and through Andrew Jackson Davies, &c. London. 2 vols. 8vo. 1847.

11. Ueber die Analyse des Blutes und die Pathologischen Krassenlehren, &c. Von Dr Gustav Zimmerman. Berlin. 8vo. 1847.

12. Die Diagnose der Bösartigen Geschwülste. Nach eigenen Untersuchungen. Von Dr Carl Bruch, Heidelberg, &c. Mainz. Small 8vo. 1847.

13. Die Krankheiten der Milz. Von Dr C. B. Heinrich. Leipsig. 8vo. 1847.

14. Lehrbuch der Pathologischen Anatomie, mit Rücksicht auf die Anwendung am Krankenbette. Von Dr Carl Ernst Bock, &c. Erste Hälfte. Leipsig. Small 8vo. 1847.

TO CORRESPONDENTS AND SUBSCRIBERS.

Communications have been received from Drs Coldstream, W. Ord Mackenzie, and Mr John Reid.

Owing to the length of the Report on Chloroform, we have been obliged to postpone the insertion of Professor Gregory's letter until our next Number.

We have received several letters, asking our advice professionally; but our correspondents have forgotten to enclose the usual *honorarium*. We have also been requested to recommend practitioners, whom we consider qualified to treat special cases, in more than one of the large towns in Scotland. It should be understood, that we cannot reply to such communications.

Owing to the length of the Medico-Chirurgical Society's report, and the number of important communications still on hand, we have been obliged to add a sheet and a half of letter-press to this Number.



Printed in colours, by Fr Schenk, Edin^r

J. H. Mole delt

Dr Glover's Communication.

ACTION
of chloroform on the stomach
OF A RABBIT.

THE
MONTHLY JOURNAL
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MEDICAL SCIENCE.

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FEBRUARY, 1848.

No. 20. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Lithotomy, as performed with a Rectangular Staff.*
BY ANDREW BUCHANAN, M.D., Professor of the Institutes of
Medicine, University of Glasgow.

[*Read before the Medico-Chirurgical Society of Glasgow, 7th Sept. 1847.*]

I INTEND to submit to the Society the results of some attempts in which I have been engaged, during the last eighteen months, as opportunity offered, to modify the operation of lithotomy, so as to render it, if possible, more simple and easy of performance to the surgeon, and, to the patient, less painful and less dangerous in its immediate and subsequent consequences. I am induced to do this, in the first place, because I think the results in themselves not unimportant: and, in the second place, because the modifications which I recommend having been, on several occasions, tried on patients in the Infirmary of this city, have thus become fair subjects of public animadversion; and I feel it due, at once to the Infirmary and to myself, to explain their nature, the reasons for adopting them, and the success which has followed them.

The attempts here referred to originated in the perusal of Dupuytren's splendid posthumous work, entitled "*Memoir on a New Method of performing the Operation for Stone.*" I was satisfied that the bilateral operation recommended by Dupuytren is, in many respects, superior to the lateral operation now commonly in use. I found, however, various difficulties in the performance of Dupuytren's operation, and my attempts were, at first, directed to obviate these. I was thus led gradually to alter some of the most important steps of the operation, and, at length, to abandon altogether the bilateral section, both of the prostate and the external parts, returning to an unilateral section of the former, not differing much from that com-

monly made, but attended with a totally different, and, as appears to me, improved method of penetrating into the bladder, and dividing the more external parts. I shall describe the mode of performing this new operation, and give my reasons for thinking it superior, in all ordinary circumstances, both to the common lateral and to the bilateral operation for stone. There are, however, certain cases in which the bilateral appears to me to be the preferable operation. I shall mention these, and the mode in which, according to my views, that operation is best performed. But, first of all, I shall describe the peculiar staff which I employ, and which I name *rectangular*, from its being bent at a right angle three inches from the lower end; because it is to the new route which this instrument has opened up into the urinary bladder, and the increased facility of access to the bladder thence arising, that the operations to be described owe their peculiar character, and the advantages which I hope to be able to show them to possess.

Instead, however, of simply describing this new staff, and the mode of using it, I think it will be better to give an account of the various trials made to improve the operation of lithotomy, in the order in which they were made, as it will be thus seen in what way one trial suggested another, and how the new staff, and new mode of operating, arose gradually out of the old by a series of successive modifications.

As Dupuytren's bilateral operation has been assumed as the starting-point of these researches, I may commence by briefly recalling to mind the principal steps of that operation. The external incision extends across the perineum in the form of a curve, of which the concavity looks backward, the summit intersecting the raphe or middle line of the perineum, at the distance of from six to nine lines from the anus, while the extremities of it are from eight to ten lines distant from the middle line on either side. The summit or middle of the curve, corresponds to the central tendon of the perineum, or tendinous junction of the sphincter ani with the other perineal muscles. The muscular fibres attached to this tendon being divided, the membranous part of the urethra is exposed and opened. The point of the double lithotome is then introduced into the groove of the staff and passed onward into the urinary bladder, where, the blades of the lithotome being expanded, it is drawn outward and downward, dividing the prostate gland, and the more external muscular parts, in its passage towards the extremities of the first incision.

The first steps of this operation must be acknowledged to be attended with some difficulty, as a delicate dissection is required to reach the membranous part of the urethra, without injury either to the bulb of the urethra above, or to the rectum below. My first object, therefore, was to simplify this part of the operation, by devising some expedient by which the knife might be made to penetrate directly, and without risk of injury, into the urethra from the

middle line of the perineum. To this attempt the position of the rectum seemed, at first, to oppose an insuperable obstacle; for it forms, immediately in front of the anus, a large sinus or bag, extending as far as the bulb of the urethra, and so covering the more internal parts of the canal, that in any attempt to carry a knife directly into it, the coats of the rectum would infallibly be twice transfixed. I found, however, that if the staff, instead of being introduced into the bladder, were carried no further than the apex of the prostate, and there made to rest on the finger of the operator introduced into the rectum, it was then easy to enter the groove of the staff without any risk; for the coats of the rectum were held out of the way of the knife by the point of the staff.

Finding that I could, in this way, at once open the membranous part of the urethra, I made this the foundation of my first modification of Dupuytren's operation. Placing the staff in the position just described, I penetrated into the groove with a sharp-pointed bistoury, making it pass from the middle of the perineum immediately beneath the central tendon, and with the edge looking to the left side. After carrying the knife to the end of the groove, I cut for about an inch outward and downward towards the left ischiatic tuberosity. I then turned the edge of the knife in the opposite direction, and, introducing it again to the end of the groove, made another incision exactly similar on the right side. An angular external wound had thus been made, and the membranous part of the urethra opened. I now passed on the staff into the bladder, and finished the operation by means of a double-edged instrument which I named a prostate knife, of which the beak was placed in the groove of the staff, and the blade cut both sides of the prostate as it was pushed onward into the bladder.

It is needless to describe more in detail an operation which has now no interest but as one of the steps which led to a more perfect method. Suffice it to say, therefore, that this operation had two great defects; first, that after the urethra had been opened, and the external incisions made, there might be difficulty in passing on the staff into the bladder, and so the operation remain half finished; and, second, that though the groove of the staff was opened with ease, yet on shifting the position of the staff by passing it into the bladder, there was no longer a correspondence between the wounds of the skin and of the membranous part of the urethra; and there was, therefore, difficulty in getting the beak of the prostate knife inserted into the groove of the staff.

To meet these difficulties, I had a staff constructed of such a shape, that when the point of it was within the neck of the bladder, another prominent part of it, about two inches further back, might rest upon the finger, in the situation just described, and serve as a guide to lay open the groove, while, into the opening thus made, the beak of the prostate knife might be at once inserted, no change of position being required. I was thus enabled to enter the urethra

with ease, and without any danger to the rectum; and, the staff being already in the bladder, there was no impediment to the subsequent steps of the operation.

The staff so constructed is straight till about three inches from the lower end, where it bends at once forward at an angle of $67\frac{1}{2}$ degrees, measured from the produced straight line; and, an inch further, it makes another but more gradual bend in the same direction, amounting to $22\frac{1}{2}$ degrees, measured in the same way; so that the remaining part of the instrument, two inches in length, is at right angles to the uppermost part. In this instrument it is easy to perceive the idea in which it originated; for, if it were introduced into the bladder, and the part in front of the second angle removed, that angle would become the point of a staff differing little in curvature from the common one, and placed exactly in the situation which I have described above. It is obvious, therefore, that the two inches at the end have been added merely to make sure that the instrument is in the bladder at the commencement of the operation. I found this instrument to be introduced with ease, and to possess the following advantages: First, That when it is introduced the greater angle of it projects in the perineum between the bulb of the urethra and the anus—or immediately above the anus, speaking relatively to the position in which the person operated upon is placed, as I shall do hereafter. The projecting angle is easily felt by the finger, and serves to guide the point of the bistoury with ease and safety to the membranous part of the urethra; for it does not require to travel further than about double the thickness of the skin to enter the groove of the staff. Second, That the extreme part of the instrument, lying nearly horizontally, keeps the coats of the rectum depressed, instead of allowing them, as when an instrument of the ordinary curvature is employed, to be drawn towards the pubis, occupying the whole angle between the skin and the membranous part of the urethra, and thus coming directly into the way of the knife, and rendering the utmost caution necessary to avoid wounding them. Third, That after the external incisions are made, the prominent angle of the staff is so near at hand, that it can be readily made to project at the external wound; and thus no difficulty is experienced in placing the beak of the prostate knife in the groove of the staff.

It will be seen hereafter that I operated once on the living subject with a staff of this form, and that the steps of the operation were nearly the same as in the method described above. Had the patient done well, I might possibly have rested satisfied with the operation as then performed; but the unfavourable result led me to consider, whether I could not devise any further improvement on the operation. It soon occurred to me, that the same means which kept the rectum out of the way of the knife penetrating into the urethra from the middle of the perineum, would be equally effective were the knife to penetrate as far down as the verge of the anus. Getting a staff constructed accord-

ing to this new view, I was enabled to dispense with the double bend in it, and gave to it the shape which it now has,—bent at a right angle three inches from the point. When this staff is introduced into the bladder, the lower or grooved branch of it lies parallel to the rectum, the angle resting on the farthest extremity of the membranous, or rather the commencement of the prostatic part of the urethra; so that, when the knife is plunged into the groove of the staff, the membranous part of the canal escapes all injury,—the incision commencing at the apex of the prostate gland, and being continued along the side of it to the bladder.

This new staff, therefore, while it possessed all the advantages of the former one, had the additional recommendation of doing less violence to the urethra. It enabled me, also, to effect several important alterations in the steps of the operation, and to simplify the apparatus for performing it.

The point at which the knife penetrated into the groove of the staff being now much lower, or immediately above the anal orifice, the direction of the external incisions was somewhat changed. They were, at first, quite horizontal, but acquired an inclination downwards as they extended to the outside, towards the ischiatic tuberosities. Further, as the knife was at once plunged into the substance of the prostate gland, dividing first the one side and then the other, at the same time that it made the external incisions, there was clearly no longer any use for the prostate knife, which I was thus enabled to dispense with. I did not, however, at first perceive this advantage, and intended to have used the prostate knife in case second, narrated below, had I not been prevented from doing so by an accident.

Last of all, as I found that a moderate incision of one side of the prostate and perineum was sufficient to extract a stone of ordinary dimensions, I abandoned the bilateral section in all ordinary circumstances, and had only recourse to it in certain cases, to be hereafter specified. It must not be supposed, however, that in thus abandoning the double section of the perineum and prostate, it was at the sacrifice of any of the advantages which Dupuytren has justly enumerated as belonging to his bilateral operation; for, if these advantages be analysed, it will be found that they depend on two causes; and that of these, that which here remains untouched is the most important and generally applicable,—the direct and short path of access to the bladder; while the other source of advantage,—the double section of the perineum and prostate,—is only important in particular circumstances of rare occurrence.

As this new staff was first constructed, it had an inferior groove, but I found this attended with several disadvantages; for, whether the groove were made deep or shallow, and whether the knife employed were spear-pointed or with a broad shoulder, and whether it had a single or double edge, it was apt, if carried deep into the groove of the staff beyond the verumontanum, to injure the seminal

duct and vesicles, and the resulting incisions were often ragged and irregular. I accordingly abandoned this inferior groove, and tried grooves of a different kind—as, a lateral groove, a groove looking outward and downward, and a spiral groove, commencing below, and twining gradually round to the side, as it passed inwards. The result of all these trials was to make me finally prefer a lateral groove having a posterior opening, so that the point of the knife might pass directly into it from the perineum.

By the use of this instrument, a new route, previously inaccessible, is opened up into the urinary bladder. The knife passes inwards, in a straight line parallel to the anterior surface of the rectum. If we suppose a triangle formed, of which the apex of the prostate is the superior angle, the anterior coats of the rectum and the membranous part of the urethra the sides, and the fore part of the sphincter ani the base, then will the line in which the knife penetrates to the apex of the prostate, be very nearly the line dividing the above triangle into two equal parts. The tissues divided by the incision are the skin, the anterior fibres of the sphincter ani, the superficial perineal fascia, and the most anterior fibres of the levator ani, where they meet under the urethra on the fore part of the rectum. The descending fibres of the levator ani, which are inserted into the side of the rectum, and which are divided in the lateral operation, remain here uninjured. It is chiefly at the outer and lower side of the prostate gland that the muscular mass above defined is incised; for the apex of the gland lies so immediately over the verge of the anus, that the knife at once plunges into the substance of the gland. There is, therefore, not the least danger of wounding the rectum; for, not only is it defended by the lower side of the groove of the staff, but the mass of the gland, progressively increasing in thickness as the knife goes inwards, intervenes; and, more externally, there is a layer of muscular substance of considerable depth. It is also obvious that the knife is far distant from all the large bloodvessels of the perineum.

The anatomist, who has been accustomed to regard the anterior surface of the rectum, not as a straight, but as a highly curved surface, may find difficulty in acquiescing in these statements: and may still be disposed to contend that a knife, penetrating directly inward from the anterior verge of the anus, will necessarily enter the rectum; and, if urged further in the same direction, will transfix a second time the coats of the intestine; and that the only mode of avoiding a wound of the rectum would be to give the knife a curvilinear course, first obliquely ascending as it goes inwards, and then obliquely descending in the same direction. He ought, however, to consider that the difficulty here stated is, in a great measure, of artificial origin; and that, though it be true that the anterior surface of the rectum is naturally somewhat curved forwards, it is converted into a straight line by the introduction of the rectangular staff, just as, by the introduction of a staff of ordinary form, the curve is preternatu-

rally augmented, and that the more so as the instrument is more elevated towards the arch of the pubis. Although contrary to received notions, it is nevertheless true, that in opening the membranous and the prostatic portions of the urethra, there is more danger of wounding the rectum if the staff be held up towards the pubis, than if it be depressed towards the coccyx and sacrum. The rule for holding the staff, in the operations about to be described, is founded on this principle, and is just the reverse of the ordinary rule; viz. to hold the staff firmly downward upon the rectum, instead of elevating it towards the pubis.

I shall now describe the two operations which are performed with this instrument. But I would first remark, to avoid the difficulties of an ambiguous nomenclature, that to all operations in which we penetrate into the bladder from the middle line or raphe of the perineum, I would give the name of *mesial*, or middle operations, to distinguish them from the *lateral* operation, in which we penetrate into the bladder by an incision on one side of the middle line. In the mesial operation, again, we may either cut both sides of the perineum and prostate, or only one. We have, therefore, two varieties of this operation, to which I would apply the names of the mesial bilateral, and the mesial unilateral operations.

I shall first speak of the latter of these two forms of the mesial operation, which seems to me to be preferable in the great majority of cases of stone in the bladder: since it is clearly inexpedient, because unnecessary, to do violence to both sides of the perineum and prostate, if an unilateral section of them is sufficient to extract the stone.

The only instruments necessary for this operation are the rectangular staff, which has been already described; and the knife, which ought to have a cutting edge as long as the groove of the staff, with the back straight, and the blade of uniform breadth as far as the point, which should be shaped like that of a scalpel, but fitted to stab as well as cut.

The operator having introduced the staff into the bladder, which is done without difficulty, places the forefinger of his left hand in the rectum, and feels for the horizontal branch of the staff lying over the prostate. He then moves the staff backwards and forwards, till he feels the prominent angle, which guides him in making the orifice of the groove project in the perineum, at the anterior verge of the anus; or just at that point of the raphe, where the skin begins to lose its proper characters, and gradually assume those of mucous membrane. Keeping the staff steady in this position, by means of the forefinger placed within the rectum, and the thumb pressing on it externally so as to indicate the orifice of the groove, he commits the staff to an assistant, with directions to keep it in the same position, and press downward with a moderate degree of firmness, of which, from the instrument resting upon his own fingers, he is enabled to judge. He now takes the knife into his right hand, holding it in

what is technically named the third position, with the palm of the hand looking obliquely upward, while the blade of the knife is horizontal, and the edge turned to the left side. He penetrates through the skin and other tissues, till he feels that the point of the knife is within the groove, when he carries it directly onward, till it is arrested at the termination of the groove. As he has now penetrated into the bladder, there is usually a little urine seen to ooze out along the blade of the knife. He finishes the cutting part of the operation, by withdrawing the knife, so as to cut first three quarters of an inch outwards and downwards, in the direction of the fore part of the tuberosity of the ischium, and thereafter three-eighths of an inch almost directly downwards.

The total extent of the external incision is thus one inch and an eighth; but, owing to the vicinity of the anal aperture, and the cut parts subsiding in that direction, the incision is quite sufficiently large to permit a stone of considerable size to be easily extracted; for full advantage is taken of the natural apertures in enlarging the wound, the aperture of the bladder coinciding with it internally, and that of the rectum externally.

It may also be remarked, that the direction of the first part of the incision is almost horizontal, and that the apparent inclination downwards is due, in a considerable degree, to the point at which the incision commences being raised above its natural level by the insertion of the finger into the rectum. Strictly speaking, indeed, the incision has three different directions, corresponding to the sides of a polygon drawn around the anus: the first part of it, formed by the knife passing along the groove, being horizontal, the second inclined downward, and the third vertical; but owing to the retraction of the skin, and to the first of these parts being less than the second, they appear to be in the same direction. If, however, a knife were employed three-eighths of an inch broad, then would the three parts of the incision be each equal to a breadth of the knife, and represent three sides of a polygon circumscribed around the anal aperture; the first part or breadth of the knife being horizontal, the second inclining downward toward the ischiatic tuberosity, and the third being vertical. Such an incision approaches, very nearly, to one half of Dupuytren's incision; only it lies much nearer the rectum, and, although little different in size, involves a larger portion of the circumference of the intestine.

A wound of sufficient size has now been made to admit of the finger being easily introduced into the bladder, which should be done without moving the staff from its place, passing the finger above the horizontal branch of the instrument. The finger of the operator is obviously the measure of the smallest possible incision of the prostate, that can be made in the operation of lithotomy. Now, it seems to me an advantage of this operation, that this smallest incision is always made in the first instance; and then, the finger being introduced, the size of the stone is ascertained. If the stone be small, or

of a moderate size, it will be easily extracted; and if, again, it be too large to admit of extraction, we are enabled, by having ascertained its exact size with the finger, which is the only way in which it can be accurately ascertained, to judge of the means necessary for enlarging the aperture. If it be merely a little too great, dilatation will probably be thought the best means to adopt; if, again, it clearly demands a more ample incision, our knowledge of the size of the stone points out to us what should be the direction and size of the additional incision.

The question, as to the direction of the additional incision, is an important one. It may be thought that, as the wound of the left side of the prostate is of less size than is usually made in the common lateral operation, it would be advisable to cut the prostate more extensively on that side. It seems to me, however, that there would, in this way, be considerable risk of infiltration of urine behind the fibres of the levator ani muscle, which are less freely divided than in the lateral operation. I would, therefore, prefer dividing the right side of the prostate, which is easily done, by shifting the staff a little to the left, passing the knife on the right side of the staff, and guiding the point of it with the finger, along the top of the horizontal branch, into the bladder. The staff being now replaced, and the knife being held in the left hand, while the finger of the right rests on the point of it within the bladder, the blade is made to advance with a sawing motion towards the right, keeping parallel to itself, so as not to make the external incision unnecessarily large. A narrow probe-pointed bistoury may also be employed to divide the prostate and perineum on the right side. After this incision, I would be disposed to try dilatation again, if there was any hope of succeeding in that way, rather than cut the prostate more extensively on either side. If, however, the stone be of very large size, then all risks must be run, and the wound of the prostate enlarged on one or on both sides, so as to permit its extraction.

In extracting the stone, it is best to introduce the forceps, and lay hold of it before removing the staff, which serves as a guide to the forceps, and prevents the neck of the bladder from receding into the pelvis before the stone be secured. Particular attention should also be paid to the position of the patient. The upper part of the pelvis should be raised by a pillow placed under the loins, so as to make the lower brim look obliquely downward; the stone thus descends toward the wound, instead of rolling back into the body of the bladder, as when the patient lies flat upon his back, with the whole pelvis raised. It is also, according to my views, right to have very little urine in the bladder at the time of the operation, so that the cavity may be contracted, and the stone may not be covered by any folds of the relaxed coats of the bladder.

The above operation may be said to be just one half of the bilateral, and its less degree of severity is its recommendation. There are, how-

ever, certain cases in which the bilateral operation is to be preferred. Such cases are of two kinds, first, the cases alluded to above, of adults having a large stone; and, second, the cases of very young children, whatever may be the size of the stone; for, in such young subjects, from the small size of the parts to be cut, a wound of one side of the prostate, capable of admitting freely the finger of a full-grown man, must, of necessity, be relatively large. In cases of the first kind, I would perform the first half of the operation exactly in the way described above; and, after feeling the size of the stone with the finger, I would cut the opposite side of the prostate and perineum to such an extent as might be required. The incision of the two sides of the prostate is thus made symmetrically, while the external incision is larger on the left side than on the right. In young children, again, I would prefer making both the internal and the external incisions perfectly symmetrical, and would limit the extent of the latter, on each side of the middle line, to about half an inch; for, from the subsidence of the anterior coats of the rectum, the opening thus made is as ample as can be required. The inclination of the internal incisions downwards is so slight, that on the healing of the wound the cicatrice appears a straight line. This is exactly the operation that was performed in case second, narrated below—only in that operation a staff with an inferior groove was employed, of which I was not then fully aware of the disadvantages. I would now prefer making use of a staff with a lateral groove, and after penetrating into the bladder and left side of the prostate and perineum with the lithotomy knife, using a probe-pointed bistoury to make the incision on the right side.

The prostate, in both these operations, is found to be cut in the very same direction as is commonly recommended in the lateral operation—outward and a little backward from the apex, or in a direction parallel to that of the vesiculæ seminales. This coincidence is not a little remarkable, considering the total difference of the mechanism by which the section is effected. In the lateral operation, the lower half of the prostate which is to be cut lies horizontally, with the staff not pressing upon but a little raised from it, the upper half of the gland being hooked up under the arch of the pubis. Now, in cutting the prostate, the handle of the knife is depressed and held to the left side, as the point of it is pushed along the groove of the staff: the consequence is, that the section is intermediate between the vertical section which would be made if the edge of the knife were held straight downward, and the horizontal section which would result from holding it, on the level of the gland, to the left side. In the operations here described, again, the blade of the knife, while it cuts the prostate, is held almost horizontally, and did the gland itself lie in the horizontal plane, the section would be directly outward; but from the lower half of the gland not lying horizontally, but being pressed obliquely downward by the staff resting on the middle of it, and thus presenting on each side a sloping surface to the edge of the

knife, the incision comes to be made in exactly the same direction as before. But though the direction is the same, the sections themselves are somewhat different. In the lateral section, the substance of the gland and the fibrous covering investing it are incised exactly to the same extent; but, in the operations here under consideration, it is only the anterior half of the gland that is cut through and through, for the section of the posterior half does not go through the whole depth of its substance, and spares the fibrous indusium and adjacent cellular membrane. This circumstance seems to me important, both as guarding against infiltration, and facilitating dilatation, should it be required.

There is one precaution, with respect to the use of the staff, which must not be passed over. On whatever point of the urethra, situated beyond the bulb, the angle of the staff is made to rest, that point is forced outward before it, and made to project in the perineum. Now, the point at which the angle ought to be made to rest is the apex of the prostate: it may rest a little farther onward, without disadvantage; but if it goes as far as the verumontanum, the ejaculatory duct will be injured; and if it be placed farther inward still, the bladder will be opened, through the substance of the prostate, without entering the urethra at all. This happened to me, more than once, in operating on the dead subject, before I understood the reason of it. I found that, without intending it, I had performed exactly the Celsian operation, forcing down the prostate before the angle of the staff, just as it is forced down before the stone in cutting upon the gripe. It is, therefore, an important rule, after introducing the staff, to feel that the body of the prostate intervenes between the finger in the rectum and the horizontal branch of the staff.

In comparing the two mesial operations above described, with the common lateral operation, I labour under the disadvantage of addressing those who, for the most part, have never seen the former operations tried, and who are prejudiced in favour of the latter by long familiarity with it; and they will be apt to think that the prejudice is on my side, when I declare, that it is impossible for me to hold any opinion more strongly than I do of the superiority of the mesial operations. I must add, however, that I here speak of them merely as mechanical operations, since the evidence of experience, by which their practical value must be ultimately tried, is yet wanting.

In the first place, I would say, that the mesial operations are more direct, and more easy and rapid of performance. The common object of all these operations is to reach the apex of the prostate gland, in order to incise it. Now, I would ask, if this is not done in the most direct and natural way in the operations described above? The point of the prostate is not above two lines distant from the posterior extremity of the raphe of the perineum; and by merely piercing the skin and fibres of the sphincter muscle, the knife comes immediately into contact with it, and, entering the groove of the staff, cuts one side of the gland as it goes along into the bladder. In the lateral

operation the prostate is also cut; but by how circuitous a route is the knife made to penetrate to the apex of the gland! In the first place, an incision, from two to three inches in length, is made along the side of the perineum; but this incision does not reach the apex of the prostate, which is almost an inch distant from the nearest part of it; so that after a second incision, dividing the fibres of the levator ani muscle, the knife is carried about an inch inward out of sight, being hid behind the skin and the anterior sack of the rectum, and then it reaches not the apex of the prostate, but the membranous part of the urethra, which it is quite unnecessary to cut at all; and, after cutting it, the knife at length arrives at the apex of the prostate, which, it has been seen, might have been reached by travelling just two lines from the surface of the skin. This is much the same as if a person in going into his own house, instead of opening the door to get into the passage behind, were to make a breach in the wall to the right hand, and, getting into the adjoining apartment, were to make his way thence through the intervening partition, taking care, however, to penetrate through the partition near the roof, so that he might descend into the passage from above.

The lateral operation was obviously devised under the idea, that the rectum presented an insuperable obstacle to penetrating into the bladder directly from the middle of the perineum. The operator is, therefore, made to travel round the obstacle from the left, and get in behind it; in Dupuytren's operation, again, he ascends over the top of it; while, in the operations here described, the obstacle being pressed out of the way, he passes straight forwards into the bladder.

In the second place, The lateral operation is unnecessarily severe. This is chiefly owing to the external incision being so far distant from the neck of the bladder; for it is only by making that incision of large size, that the finger and instruments can be introduced so deep, whence the great disproportion between the external and the internal incisions, although the latter is large enough to allow the passage of the stone. The wound of the membranous part of the urethra is also unnecessary, as already mentioned.

In the third place, There is more danger of hemorrhage in the lateral operation; for as the two first incisions are nearly parallel to the great artery supplying the perineum, and sending its branches transversely inwards for that purpose, they must run almost at right angles to these branches, and are therefore in the direction most likely to do them injury. In the other operations again, the incision is far out of the course of all bloodvessels of importance.

Fourthly, The rectum, notwithstanding the vicinity of the incision to it, is less likely to be injured in the mesial operation than in the lateral; the reasons for which I have already endeavoured to explain.

Lastly, There seems to me to be more risk of deep-seated infiltration of urine in the lateral, than in the mesial operations. This is a question that can only be decided by experience; but the size of the

wound of the prostate, and the extensive division of the fibres of the levator ani muscle, seem likely to occasion that result.

I conclude, by giving a short history of the three cases in which the mesial operation has been performed in the Glasgow Royal Infirmary.

John Bruce, æt. twenty-four, shoemaker, Ward V., *Feb. 12th*, 1847.

Has laboured under symptoms of stone for six months past, and has since then passed seven small calculi. The sound readily detects a stone in the bladder. Urine mingled with viscid mucus, and gives a copious coagulum with heat and nitric acid. Although emaciated, takes his food well, and his general health and spirits are good.

On Feb. 23d, the middle bilateral operation was performed, and six stones, varying from the size of a bean to that of an almond, removed. The staff employed was that which I have described above as having two angles. In every other respect, the steps of the operation were the same as in the first modification of Dupuytren's operation, above described.

The patient had been put under the influence of ether, and said he felt no pain. No febrile or inflammatory action followed the operation. The wound, however, never showed any disposition to heal; the urine was constantly discharged by it; and its surface looked unhealthy. A troublesome diarrhœa came on, which resisted all attempts to arrest it. The patient became emaciated to such a degree, that, for some time before his death, the muscles of his face were seen, as if dissected, playing under the integuments when he spoke. He died on the twenty-third day after the operation.

On examining the body (March 20th), the bladder was found contracted, and its muscular coat very much thickened. The kidneys were in a state of granular degeneration; and in that of the left side there was a hydatid of the size of a marble. There was also a collection of pus in the distended pelvis of each kidney, but most in the left. The wound in the prostate, and the external wound, had made no progress in healing. They were covered with a calcareous incrustation, which was observed also on various parts of the mucous surface of the bladder. Between the uppermost part of the sacrum and the intestinum rectum, there was a very large abscess containing healthy matter.

William Buchanan, æt. thirty months, Ward VI., *May* 1847.

Has laboured, during the last fourteen months, under symptoms of stone, and on introducing a sound a calculus is distinctly felt. Urine slightly albuminous. General health good.

June 8th.—Yesterday three weeks, middle bilateral operation was performed. The external wound was made first to the left, and then to the right, immediately before the anus, dividing the integuments and fibres of the sphincter ani muscle. At this stage of the operation, the rectangular staff, which had been accidentally broken the same day and insecurely soldered, gave way immediately above the angle, and the lower fragment, having a very sharp angular point, slipped into the bladder. An ordinary staff was now introduced into the bladder, and the section of the prostate made right and left upon it. After removing the stone, the fragment of the broken staff was extracted by a bone forceps, but not without some difficulty, from its sharp point having transfixed the coats of the bladder. Has since been going on well, although cure retarded by inflammatory swelling of penis and scrotum. Urine passed almost entirely by the urethra. Wound contracted. General health good.

June 12th.—Dismissed cured.

Robert Banks, æt. twenty, collier, Ayrshire, Ward V., *Aug. 9th*, 1847.

About five years ago had an attack of acute pain in right lumbar region, followed, after a few days, by the passage of a grey calculus of the size of a small

horse bean. Soon after, he began to complain of pain in making water, which has gradually increased. It is referred chiefly to external meatus, and to situation of neck of bladder. No return of lumbar pain. At present a stone, seemingly of considerable size, readily felt by sound. Urine, of natural colour, deposits a moderate mucous cloud, of sp. gr. 1.014, pretty highly acid, and not coagulable by heat. General health good.

Aug. 13th.—Mesial unilateral operation performed to-day, and two calculi extracted from bladder.

Sept. 4th.—The morning after operation had chills, followed by languor, debility, and anorexia; and next night by much restlessness, with tendency to delirium. After this his health improved; but on the sixteenth he was attacked by conjunctivitis of left side, and a herpetic eruption over face, and in two days more the integuments around operation wound became red and inflamed. The latter symptom continued, the redness shifting about on buttock, thigh, and loins, until about ten days ago, when it faded, as did also the eruption on face, and the inflammation of conjunctiva. About the same time, bowels became loose, but diarrhoea yielded in two or three days, patient having been put on milk diet, and chalk mixture with catechu. General health is now quite good; and for some days urine has been passed entirely by the urethra. The operation wound is healed up almost to a point. Wishes to go home.

Sept. 6th.—Dismissed well.

ARTICLE II.—*Case of Recovery from a poisonous dose of Strychnia; with Observations on the Tests for the Organic Alkalies.* By THOMAS ANDERSON, M.D., F.R.S.E., Lecturer on Chemistry, Edinburgh.

(Read to the Medico-Chirurgical Society of Edinburgh, December 15th, 1847.)

MR B. has been afflicted, for a considerable period, with tic douloureux, occurring at irregular intervals, and in paroxysms of excessive violence. For the relief of the pain of these attacks he has been in the habit of making use of muriate of morphia, at first in small quantities, and afterwards in gradually increased doses, until three and a half grains were necessary to produce its effect. At this time, he went into the shop of a druggist in the town in which he resided, for the purpose of obtaining some muriate of morphia, and received a quantity of a powder slightly yellowish in colour, which was stated to be that substance. On the same day, having occasion to go a short distance into the country on business, he took, previous to going out, his usual dose, $3\frac{1}{2}$ grains of the powder, which was simply placed upon the tongue and swallowed, and he remarked at the time that it was extremely bitter, and that the taste was more than usually persistent, but it did not occur to him that any thing was wrong. Shortly after, however, while walking along the street, he felt slightly indisposed, the most prominent symptom being a sense of numbness in the back of the legs, which he attributed to the effects of cold, to which he had been exposed in the early part of the morning. As these symptoms did not appear of any importance, he proceeded by a public conveyance to the village where his business lay, and returned by the next opportunity. During the whole of this time the symptoms remained precisely as they were the moment he first observed them; but as he was walking along, on his return, they sud-

denly increased, the numbness being accompanied by a sense of want of power, and a sort of dragging of the muscles of the legs, which soon became so great, that, as he describes it, he had to put his hands at the back of his thighs in order to push his legs along. This occurred at nearly two and a half hours after he had taken the dose of the supposed muriate of morphia, and, at this time, there could have been nothing remarkable or unusual in his appearance; for, on his way home, he met a friend to whom he communicated his sensations, but who laughed at his evident apprehension, and assured him that it was all imagination. As he was in the midst of describing the effect upon his muscles, and bending himself so as to show how it occurred, he suddenly overbalanced and fell heavily backwards. He was immediately raised, and, on attaining the upright position, he felt himself much in the same state in which he was before, excepting that he was excessively nervous and alarmed. The want of power in the legs did not at all increase in intensity, and no spasmodic affection was observed, although the patient was himself under the impression, that his fall was somehow connected with the previous symptoms.

The patient's fall and the nervous state into which he had got, now fairly alarmed his friend, who begged him to get home as fast as possible, and accompanied him on his way, as he experienced considerable difficulty in walking, and could not get on without support. On reaching home he felt somewhat better, and remained sitting for some time, and at length retired to bed, about five hours after the first appearance of the symptoms. Just previous to stepping into bed, in order to insure a good night's rest, of which the recent symptoms rendered him somewhat doubtful, he took a second dose of the powder equal in amount to the first. In less than ten minutes after, he was seized with a violent tetanic spasm, affecting the legs and muscles of respiration, and had only time to call out for assistance before the sensation amounted to that of absolute suffocation. Fortunately, assistance was close at hand, and he was immediately raised up in bed, with the effect of entirely relieving the sense of suffocation, and a medical man sent for. Spasms now followed each other in rapid succession, the intervals being about a quarter of an hour or twenty minutes, and the affection was confined principally to the legs, back, and respiratory muscles, the arms being comparatively unaffected. The numbness and dragging of the muscles, which had been continuous during the first five hours, disappeared entirely during the intervals of the spasms, and the patient was left without any uneasy sensations, excepting the exhaustion of the previous fit and the apprehension of its successor. During the whole of this time he was not only perfectly conscious, but his senses were preternaturally exalted, and he distinctly heard a variety of whispered observations of the physicians and his friends, which, from their tenor, were obviously not intended to reach the ears of the patient.

The paroxysms, after continuing for some time, began gradually

to diminish in violence, the intervals becoming longer, and the duration of each spasm shorter, and it was hoped that they were about to pass off, when all at once they returned in their original violence. This proved, however, to be the last expiring effort of the poison ; for the symptoms now entirely ceased, about thirteen hours after the first dose was taken. At the conclusion of the spasms, the patient was left in an excessively exhausted state, and was unable to turn himself in bed ; from this, however, he recruited with great rapidity, as he was able to get up on the evening of the next day, and on the second he walked out and went about his usual business. The most remarkable fact connected with the case is, that, from that time, the attacks of tic douloureux entirely ceased, and he has not since had any return of it. The medical treatment employed in the case was unimportant, and had not any effect on the progress of the symptoms.

On his recovery, it was immediately apparent to the patient that the supposed muriate of morphia was the cause of the symptoms from which he had suffered, and the remainder of it was sent to me for examination. On boiling it with water no solution took place, and the filtered fluid, when tested with nitrate of silver, was found to be free from hydrochloric acid. It was not therefore a muriate, and, considering the symptoms which it had produced, I at once applied to it the tests for strychnia. I found, that when treated with sulphuric acid containing a small quantity of nitric acid, and then mixed with peroxide of lead, it gave the characteristic violet tint described by Marchand.¹ A solution in muriatic acid gave, with sulphocyanide of potassium, the long needles described by Notus,² and under the microscope were obtained the characteristic crystals of strychnia, which will be described in the sequel of this paper. When treated with nitric acid it acquired a very feeble red colour. It dissolved entirely in acids, and burnt off with the exception of a very minute quantity of a white ash. From these observations, I infer that the substance was strychnia in a tolerably pure state ; in fact, exactly such as is usually made use of in medicine.

The case which I have just detailed, is interesting in several points of view. In the first place, we have the perfect authentication of the quantity swallowed, the patient having himself weighed out the doses, together with the determination of the nature of the poison. Secondly, It is so far, as I have been able to ascertain, by much the largest dose from which recovery has been recorded. In general, a dose of one or two grains, and in the case of *nux vomica* itself, of which we have the largest number of cases, a quantity corresponding to not more than one-third of a grain of strychnia, has proved rapidly fatal. Indeed, a case is mentioned by Hoffmann³ in which the death of a girl, ten years of age, was occasioned by two 15 grain doses of

¹ Journal de Pharmacie, N. S., Vol. IV. p. 200.

² Ibid., Vol. XXIV. p. 192.

³ Hoffmannus, Philosophia Corporis Humani, Part II. Cap. VIII.

the nut, which contain about 0·15 grains of strychnia. It appears, however, that both strychnia and nux vomica present very great irregularity in the quantity required to produce their poisonous effects; the dose which has proved fatal in one case, being taken with impunity in another. Dr Pereira¹ mentions a case in which he gave several one and a half grain doses of strychnia before its physiological effect was produced; and the older physicians, according to Sennertus,² were in the habit of administering the nut in doses of one or two scruples, and even in larger quantities, as a remedy for plague, a dose which would not be considered very safe at the present day. As a contrast with these quantities, I may state that the dose of strychnia taken in the case above detailed, corresponds to nearly three ounces of nux vomica.

Thirdly, The case is interesting from the slow and gradual approach of the symptoms, and the long period which elapsed before tetanic spasms supervened. In fact, the first dose of three and a half grains produced no symptoms except the want of power of the muscles, and no tetanic spasm occurred till after the second dose; and it may, I think, be fairly inferred, that if the second quantity had not been taken, the effects of the first might have gone off without producing any symptoms of greater severity than those observed during the first five hours. The instant, however, that the second dose was swallowed, the full violence of the symptoms was produced in the system, already partially under the influence of the poison. The question may be raised as to how far the action of the strychnia may have been affected by the habitual use of large quantities of opium; but I am not aware of any facts which can be directly brought to bear upon this point. The only case at all resembling it is one mentioned in the *Journal de Pharmacie*,³ in which a pharmaceutical student of dissipated habits swallowed two grammes (about thirty-one grains) of strychnia on leaving a public place where he had been drinking, and in which tetanic spasms did not make their appearance till after a considerable period; so much so, that the physician, in the absence of all bad symptoms, refused to believe the reiterated assurances of the patient that he had actually taken it. In this case, it is just possible that intoxication may have prevented the immediate access of the symptoms, exactly as it is known to do in the case of poisoning by opium.

Fourthly, The perfect cure of the neuralgic symptoms, which, I find from the patient, has been permanent. He is also impressed with the idea, that the violent action of the poison has had a permanent effect upon the nervous system, as, since his recovery, he has been excessively nervous, easily startled and affected by trifles, which he never was before. I am inclined to attribute this, how-

¹ Materia Medica.

² Sennertus, *Medecina Practica*, p. 1310, Lib. VI. Pars VII. Cap. XX.

³ *Journal de Pharmacie*, N. S. Vol. X. p. 36.

ever, entirely to imagination, and the alarm naturally produced by the violent symptoms from which he has suffered.

In examining the substance which caused the symptoms already detailed, I was, of course, enabled to infer the probability of its being strychnia, and to apply at once the direct tests for that substance, which, it fortunately happens, are much more delicate and characteristic than those which we possess for almost any of the other alkaloids. I have found Marchand's¹ test a very good one, and possessed of a considerable amount of delicacy; and, as I do not recollect to have seen it mentioned in any of our medical periodicals, although it has been known for some years, I may simply state, that it consists in pouring upon the strychnia a few drops of strong sulphuric acid, mixed with about one per cent of nitric acid, and then adding a small quantity of peroxide of lead, when immediately a fine violet colour is produced. The experiment succeeds best when the mixture is made in a watch glass, care being taken that the quantity of peroxide be extremely small, and then, on mixing with a glass rod, the colour makes its appearance in streaks. Bichromate of potass has been recommended by Otto,² and the results may be obtained by means of it; but not, so far as I have observed, so readily as with the peroxide of lead. The sulphocyanide test has been very little attended to by chemists, under the impression that the results are not to be depended upon, as some of the other alkaloids are also precipitated by that reagent; but none of them are capable of giving the immediate precipitate of long needles, which is obtained even from very dilute solutions of the salts of strychnia. In all the other alkaloids, the precipitate occurs only after the lapse of some time, and by shaking the solution, and then always in microscopic crystals, unless the solution have been extremely dilute and left in repose for twenty-four hours, when sometimes crystals, the form of which can be distinguished by the naked eye, are obtained. It is, however, desirable that some more definite means of distinguishing these precipitates, and the alkaloids generally, should be obtained; and an attempt has been made by Lepage³ to supply the deficiency, by observing the action of a current of chlorine gas, both upon the alkaloids and their sulphocyanides; but it does not appear that the reactions it affords are sufficiently distinct to enable us with certainty to discriminate them.

In endeavouring to solve this difficulty, I was led to make use of the microscope, with the view of ascertaining whether the crystalline forms of the alkaloids themselves, or of their sulphocyanides, might not afford characters by which they might be easily distinguished on the small scale; and the result of my experiments has been sufficient to convince me that their forms are, in reality, exceedingly distinct, and that they can be easily distinguished in very small quantities.

¹ *Journal de Pharmacie*, Vol. IV. p. 200.

² *Journal für Praktische Chemie*, Vol. XXXVIII., p. 511.

³ *Ibid.*, Vol. XXVI. p. 140.

The method which I have employed, consisted in dissolving the alkaloid in dilute hydrochloric acid, and mixing the dilute solution on a glass plate, with ammonia of moderate strength if the alkaloid itself is to be examined; or with a strong solution of sulphocyanide of potassium if the sulphocyanide is required, and at once placing it under the microscope. The only precaution requisite is to avoid having the solution too concentrated, as the crystals are then less well defined than if a dilute solution is employed; but a few trials easily enable the experimenter to ascertain the proper degree of dilution.

Strychnia.—The hydrochlorate of strychnia, treated with ammonia, gives an immediate precipitate, which, when examined by a magnifying power of 250 diameters, is found to consist entirely of minute prismatic crystals, all nearly of the same size, and very well defined. A large proportion of these are isolated; but there is also a sort of twin crystal very abundant, the two members of which cross each other at an angle of about 60 degrees. They present, also, a sort of sulcated depression at opposite ends, which is very characteristic. They are represented in Fig. 1. The sulphocyanide of strychnia consists of flattened needles, sometimes single, but generally in irregular groups, as represented in Fig. 2. They are terminated either by

Fig. 1.

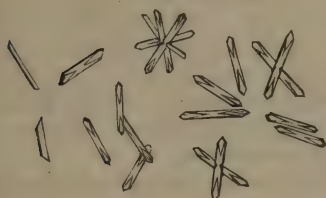


Fig. 2.



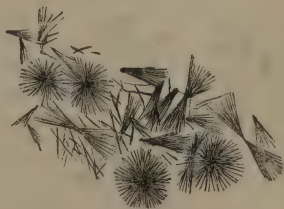
a blunt acumination or by a truncated extremity. Those which were precipitated on the large scale, were found to present the latter form.

Brucia.—A salt of brucia, in a sufficiently dilute state, mixed with ammonia, is found not to give an immediate precipitate; but,

Fig. 3.



Fig. 4.



in the course of a very short time, irregular star-like groups of pointed crystals are observed, which are represented in Fig. 3. With

sulphocyanide of potassium, in a pretty strong solution, a precipitate is obtained in tufts of extremely thin and feathery crystals, which either radiate from a centre or present a sheaf-like appearance (Fig. 4). The latter form is, however, much better marked in the crystals deposited, after some hours, from a dilute solution, which are still quite microscopic, although somewhat larger than those represented in the figure.

Morphia.—In solutions of morphia, ammonia produces no precipitate at the moment of mixture; but, in the course of a longer or shorter period, according to the degree of dilution, crystals are seen to form, which gradually increase in size, and possess the form represented in Fig. 5. Salts of morphia are not precipitated by sulphocyanide of potassium, unless the solution be highly concentrated.

Narcotine—is precipitated by ammonia in branched groups of crystals of a pointed form (Fig 6). In concentrated solutions, a precipi-

Fig. 5.

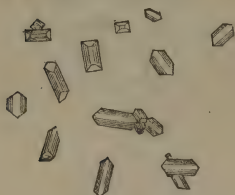
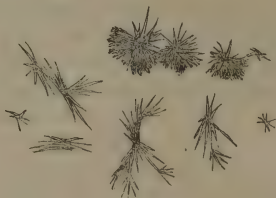


Fig. 6.



pitate is thrown down by sulphocyanide of potassium, which dissolves readily in hot water, and is deposited again on cooling. Under the microscope it is found to be perfectly amorphous.

Cinchonine—is obtained by precipitation with ammonia, in the form of minute granular masses, made up of more or less distinctly acicular crystals, radiating from a centre. It is, however, a little difficult to obtain them well marked, and they not unfrequently appear as a confused mass of granules, in which the radiated structure is very

Fig. 7.

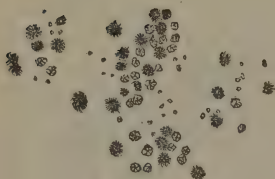
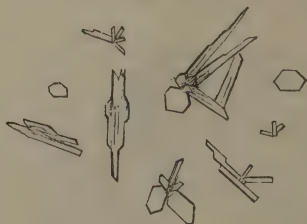


Fig. 8.

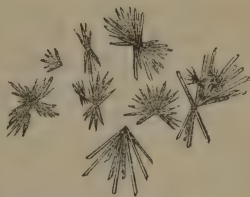


imperfectly seen. They form best when the solutions are rapidly mixed. Fig. 7 represents these crystals. With sulphocyanide of

potassium, cinchonine gives a precipitate consisting of six-sided plates, together with a variety of irregular crystalline masses, and a few rectangular plates (Fig. 8). When formed by mixing in a test tube, with agitation, and allowing it to stand for some time, the crystals are still microscopic, but much more definite, and sometimes consist almost entirely of isolated six-sided tables, of great regularity. The precipitate dissolves readily in hot water, and is deposited, as the solution cools, in irregular plates.

Quinine—gives, with ammonia, a perfectly amorphous precipitate, but with sulphocyanide of potassium it gives small irregular groups of acicular crystals, which bear a resemblance to those produced by strychnia, but are longer and more irregular (Fig. 9).

Fig. 9.



When the precipitate is produced in a test tube, and with a concentrated solution, it falls immediately as a white powder composed of extremely minute needles; but when the solution is dilute, it is deposited, after the lapse of twenty-four hours, in crystals from a quarter to a third of an inch in length. Lepage, under the same circumstances, states that he obtained a mass of yellowish green crystals, but I have always obtained them perfectly white. This is, of all those which I have examined, the salt which approaches most nearly in its characters to the strychnia compound, but it differs from it in the length of time required for its production in a dilute solution; and if we combine with its characters the amorphous precipitate which quinine affords with ammonia, we are able at once to distinguish the one base from the other.

Bebeerine—is precipitated amorphous both by ammonia and sulphocyanide of potassium. The latter precipitate is formed even in highly dilute solutions at the instant of mixture, and is a delicate test of the presence of that alkaloid, although quite inapplicable as a means of distinguishing it from others.

Atropine—is precipitated in the amorphous state by ammonia, and not at all by sulphocyanide of potassium.

It must be obvious from the observations now detailed, that under the microscope the forms of the different alkaloids examined are very characteristic and distinct, and that the combined observation of the base itself and of its sulphocyanide, may be considered as perfectly distinctive of the common alkaloids. I have not yet, however, had time to ascertain how far the method may be applicable to the detection of the substances containing those alkaloids, by extracting them with an acid, and examining the precipitates obtained from the fluid. It is unquestionable, however, that the regularity of the crystals would under such circumstances be impaired, although it is possible that their characteristics might be retained with sufficient completeness. However this may be, any addition to the tests

for the pure alkaloids is a matter of importance; and I think the present communication may not be altogether without value, as adding something to the means of distinguishing a class of substances so poorly characterised by their ordinary chemical reactions as the organic alkalies.

ARTICLE III.—*Case of Sacculated or Encysted Calculus, removed by Lithotomy.* By JAMES MILLER, Esq., Professor of Surgery in the University of Edinburgh, Surgeon to the Royal Infirmary, &c.

(Read to the Medico-Chirurgical Society of Edinburgh, January 5th, 1848.)

ROBERT DOULL, aged sixty (but looking much older), applied to me in September 1846, complaining of all the ordinary symptoms of stone in the bladder. On sounding him, a stone was detected, and he was admitted, September 4th, into the Royal Infirmary, with the intention of submitting to the operation of lithotomy.

He stated that, about four years previously, he had suffered much from pain in the loins, extending downwards into the hypogastric region; that his urine was then bloody, and that he experienced great pain in voiding it. Under medical treatment, he had been relieved from much of his suffering; but ever and anon exacerbations returned, and during the last six months the symptoms had become decidedly aggravated.

These, on admission, were;—pain in the loins, passing forwards to the pubes; an irresistible desire of making water every five or ten minutes, with pain in the glans penis immediately afterwards; bloody urine; occasional stoppage of urine; pains in the soles of the feet, &c. Urine not coagulable, and slightly acid.

The tongue was foul, and the alvine evacuations unsatisfactory. He was therefore put under blue pill and colocynth for a day or two; and, at the same time, he was directed to drink freely of a weak solution of bicarbonate of potass. This I have been in the habit of prescribing for lithotomy patients, as a preparative for the operation; with two objects in view; 1. to relieve the mucous coat of the bladder from irritability; and, 2. to render the urine less acrid, so that, in the event of infiltration, its injurious effects on texture may be modified accordingly. After some days, the symptoms became much alleviated; the intervals of micturition extending to about two hours, and pain greatly lessening. Again, however, exacerbation occurred; and not until the 22d did he seem in a fit state for the operation. On that day the ordinary lateral operation was performed.

The space between the tuberosities of the ischia was found narrow; and the perineum proved deep. The point of my left forefinger could just reach the prostatic wound, and I could feel no stone until after the staff had been withdrawn. On introducing

the forceps, a stone was seized, and without difficulty extracted. It was oval-shaped, about the size of a small egg, and smoothly hollowed on each side, plainly indicating the presence of at least another calculus. Forceps, reintroduced, found nothing; but the Searcher soon detected a second calculus. Again using the forceps, sometimes as a sound, and sometimes as forceps, I could make no seizure; often coming in contact with the stone, but never being able to include it in my grasp. I changed the forceps once and again, using different sizes and forms; but with a like want of success. At length, fearful of the patient becoming exhausted, and anxious that he should not suffer harm by unnecessary protraction of the operation, if that could possibly be avoided, I rose, and requested my colleague, Dr Duncan, then kindly assisting me, to take my place; hopeful that a fresh hand and head might prove successful. To my urgency he assented; but, after skilful and patient use of the instrument, the result was no better than before. The stone could be felt (because impinged upon), but not laid hold of. We became quite satisfied that it was firmly grasped, and protected by the bladder; either sacculated or encysted—we could not tell which; for it was far from the point of the finger, and could be reached only by the instruments introduced. Dr Duncan, Dr Dunsmure, and others around, strongly urged me to cease from further extractive efforts; and to make the case one “*a deux temps*”—a form of operation I had hoped never to see, far less to be personally engaged in. To this proposal I reluctantly gave my assent; and, sitting down, introduced the canula into the wound, with a view to the patient being forthwith removed to bed. The tube I employ happens to be larger and longer than that ordinarily used; and, in pushing it home, its end struck so heavily and plainly on the stone, that it engendered within me a desire for a renewed attempt at extraction, so strong as to be quite irresistible. Accordingly I withdrew the tube, and resumed the forceps. I got hold of the stone; but, plainly, soft parts were included in the instrument's grasp. Aware of this, I held very gently, and endeavoured to bring all down to within reach of my finger, in order that I might thereby detach the soft parts and retain the stone. Just as I thought I had succeeded, I failed; the stone slipped, and went back again to its former obscure abode. Fortunately, however, I at once obtained a second seizure; and, holding the instrument more firmly than before, I succeeded with the point of my left forefinger in detaching the soft parts from the stone, pushing them backwards, first on one side and then on the other, until I could feel the blades of the forceps in contact with the hard stone; and then, in the ordinary way, extraction was soon completed. The patient had been about twenty minutes on the table; but did not seem greatly exhausted. There was no hemorrhage. The large-sized tube, already mentioned, was secured in the wound.

The stone proved similar in form to the first; but larger, rough, and not smoothly excavated on any surface. When recent, it gave

one the impression that about four-fifths of it had been adherent to the enveloping sac or cyst.

An hour after the operation, a slight rigor occurred; but soon passed off. At night he had a composing draught of hyoscyamus and nitrous ether. He slept soundly; next morning, felt very comfortable; and, to avoid tedious repetitions, I may at once say that for the first fortnight he did remarkably well, never showing a bad symptom but once; viz., on the second day, pain and swelling in the hypogastric region, which yielded satisfactorily to a slight leeching. On September 30th the report is, "He feels quite comfortable; the wound is looking well and healing rapidly; but, as yet, no urine comes by the urethra."

On the 6th of October feverish symptoms set in, with pain in the left side of the chest, and uneasiness in the urethra. A catheter was passed carefully; and some obstructive mucus seemed to be removed from the region of the wound. The type of the fever seemed low, and he was ordered wine—in small quantities. Next day, the urine began to flow by the natural passage; but "pulse rapid and intermittent, tongue brown and dry, countenance anxious, and cough distressing."

The urine went back again, all by the wound; the symptoms of fever advanced, unchecked; hiccough, purging, and collapse came; and he sank on October 18th—nearly a month after the operation.

With difficulty the widow was prevailed on to permit an examination of the body; stipulating, however, that it should be in private, myself and my clerk only being present. On laying open the abdomen, all seemed quite sound; the peritoneum at no point showing any vestige of morbid action. The bladder lay collapsed, and of healthy appearance externally. It was cut out along with the track of wound. The latter was found firm and healthy in its walls, but very imperfectly contracted in its space; and sabulous matter, held in vitiated mucus, adhered to its deeper surface, as well as to the lining of the bladder. No abscess, or slough, or softening of texture, was any where observed. The position and extent of the prostatic wound were quite as they should have been; only there had been little or no progress towards healing. The cavity of the bladder was contracted to a very small size, and presented such structural characters as are usually observed in cases of calculus which have not undergone operation; viz. villous thickening of the mucous lining, hypertrophy of all the coats, fasciculation, and sacculation. There was no trace of injury, or of untoward inflammatory action; no ecchymosis, no laceration, no congestion of the lining membrane, no membranous exudation, no ulcer, no slough. There was no stone, either sacculated or free. And, in this changed state of the organ, it did not surprise us to find no distinct evidence of where the troublesome calculus had been lodged.

The left kidney was found extensively affected by Bright's disease; the right less so; but in it an abscess of large size (its

interior coated with hard, chronic lymph) occupied the pelvis and infundibula.

The other organs we were not permitted to examine.

I may mention that the bladder, after having been cut out of the body, was removed to the University, and was there dissected by my colleague, Professor Goodsir; who was subsequently kind enough to fit it for preservation in its present form.

My chief object in narrating this case to the Society, is to direct attention to the fact, that the mode of extraction applied to the incarcerated stone did not cause serious structural injury, if any, to the soft parts implicated; and that, therefore, a like procedure may be adopted in other cases of similar complexity, with a good prospect of success and safety. I do not, of course, mean that this mode of treatment is advisable in all cases of encysted calculus. There are cases in which the stone and sac are easily within a reach of the finger, and where detachment and dislodgement can consequently be effected without the use of forceps—by the finger and scoop, most probably; aided, if need be, by the probe-pointed bistoury. There are also cases in which the stone is so firmly lodged, and the part projecting from the narrow neck of the sac is so very small, that amplification of the space, whereby the stone is expected to make its exit, must be made by some cutting instrument, if extraction be seriously contemplated. The mode now spoken of, I conceive to be applicable to those cases in which the stone and cyst are beyond the reach of the finger, and in which the stone is not so tightly embraced by the cyst as to prevent detachment by means of a probe or finger—in which, in short, the use of cutting instruments, applied to the retaining cyst, is not essential to the stone's liberation.

Various proposals exist for dealing with encysted and sacculated stones. Littré advised seizing hold of the stone and cyst in the way I have described, and using all roughly, with the object of bruising the soft parts so as to cause suppuration in them, and disintegration of them; and with the hope of the stone (if not itself broken), becoming loosened during the suppurative stage, dropping into the bladder, and thence being extracted at another time. Garengeot, with a bistoury, divided the neck of the sac, and then used forceps. Peyronie, Le Dran, Marechal, and others, have trusted to catching hold of the projecting part of the stone, and pulling violently so as to detach it from its adhesions. Desault employed a concealed knife, or “coupe-bride,” or “kystotome,” for dividing the cyst. Some have advised the stone to be undisturbed; in hopes that the wound may heal, and that the patient may be left little worse, if no better, than when he was found. Some again advise the stone to be let alone, but only for the time; returning to the extraction in some days, during what is called the “suppurating and relaxing stage,” hoping then to find the stone disengaged and at their service—the operation “a deux temps.”

To any and all of these methods, the one followed in the present case seems decidedly superior; more satisfactory than the "deux temps;" more safe than bruising, tearing, or cutting the vesical coats; and more likely to prove successful (*quoad* extraction) than nibbling at the projecting portion of the calculus. I confess, however, that were a surgeon to catch a firm hold of the projecting part of an encysted stone, he would not be to blame were he to decline loosing that hold until he had made more than one determined effort to dislodge and bring the stone away. To exert, and much more to continue to exert, extractive *violence*, however, even in such circumstances, were wrong, as the experience of Peyronie abundantly proves. "The surface of the calculus was found covered with fleshy substances, which formed the adhesions to the bladder. The operation was painful, followed by considerable hemorrhage, tension of the belly, hiccough, cold extremities, and death."

The case of Doull seems to show satisfactorily, that an incarcerated stone may be seized firmly, yet gently, along with its investing soft parts; that all may be brought down to the prostatic wound, within reach of the finger; that by the finger's point and nail, the soft parts may be detached and reflected, while the stone is not let go; that the stone, cleared, in whole or in greater part, may then be readily extracted; and that all this may be effected without inflicting untoward injury on the vesical coats. It is not unimportant to observe, however, that the forceps employed were Liston's; flat in the blades, and in their grasping surfaces covered with calico, instead of being armed there with projecting teeth. The calico, in such circumstances, at once affords a sure hold, and avoids the risk of vesical injury.

The case further marks an important fact, viz.—that the presence of two or more excavated and smooth surfaces on a vesical calculus, does not necessarily prove the existence of more than one other stone; in as much as, if one stone be fixed and projecting, the other, loose, and often shifting, may, at different times, present different surfaces for attrition.



ARTICLE IV.—*On the Physiological and Medicinal Properties of Iodoform.* By R. M. GLOVER, M.D., Lecturer on Materia Medica in the Newcastle-on-Tyne School of Medicine.

SINCE the application of chloroform to anæsthetic purposes, every thing relating to it has assumed a popular aspect; and I think the opportunity a good one for calling the attention of the profession to an allied substance of singular constitution, and possessed, if I do not mistake, of singular and valuable properties. In the paper which I published "On the Physiological and Medical Properties of Bromine and its Compounds," and on the analogies between them and chlorine and iodine and their compounds, I called the attention

of the profession to this body, iodoform, as I also did to the remarkable physiological properties of the chloride and bromide of olefiant gas, and to those of chloroform, then all (I may say) unknown bodies. My paper was variously and favourably noticed; but that part of it which I considered the most valuable and interesting, and the most novel,—viz. the chapter headed “Physiological Properties of the Bromide and Chloride of Olefiant Gas—of Bromoform, Chloroform, and Iodoform,” was *never noticed at all*.

The following is the passage in this paper regarding the medicinal properties of iodoform:—“I have used iodoform with great benefit externally in many obstinate skin affections,—such as lepra, psoriasis, chronic eczema; and internally, with benefit, in a very old case of bronchocele, and in scrofulous enlargements of the glands. I gave it in doses of two grains, thrice a day in pill, with mucilage and bread crumb; externally, in the dose of half a drachm to the ounce of simple cerate. I believe that this may prove one of the most valuable remedies that we have.”

In the years 1838-39, I used iodoform extensively in skin diseases in the Newcastle Infirmary, in care of Sir John Fife. I expected at the time, and understood that an account was kept of the cases, and furnished several pounds' worth of iodoform. On applying for the reports of the cases, I was favoured by the house-surgeon with very much such a testimonial as Professor Holloway has the credit of advertising:—“Dear Sir,—Your ointment has performed such and such wonders.” But Sir John Fife will corroborate me when I say, that the results obtained were very surprising. Since then, I have used iodoform from time to time, both in dispensary and private practice; and some druggists have been in the habit of keeping the remedy prepared for my prescriptions. I have also repeatedly urged a trial of it upon the physicians and surgeons of our Infirmary, and upon other medical men, but without being able to get them to use it.

When we consider the constitution of iodoform, $C_2 H_8 I_3$; so that 13 grains of carbon and hydrogen in this body render about 380 grains of iodine, *organic*; change the iodine from an inorganic into an organic body of the same nature as an acid secreted by animals, and possessing relations with alcohol and ether (an assimilable substance), we cannot help being struck with the extraordinary chemical composition of the substance, and become desirous of ascertaining its physiological and (if any) its medicinal properties.

In this paper, I shall first speak of the chemical history of iodoform, state some facts with regard to its mode of preparation, and then on what is known of its physiological and medicinal properties. Iodoform was discovered by Serullas, who named it perhydrioduret of carbon; and Mitscherlich, in examining it, announced the opinion that it contained no hydrogen (*Annales de Chimie et de Physique*, tom. xxxvii. p. 84). Its true nature was demonstrated by Dumas (*Ibid.* tom. lvi. p. 113), who proved that it should be considered as

a body analogous to formic acid, where the oxygen is replaced by iodine. Since iodoform was discovered, the mode of its preparation has undergone many changes, and probably remains very imperfect. Serullas first obtained it by the action of potassium on the alloy of antimony, and potassium on the alcoholic tincture of iodine. He then prepared it by the action of chloride of iodine on an alcoholic solution of potass, and he discovered, at last, that it could be obtained with facility by mixing, until discoloration, alcoholic solutions of iodine and potass.

It is certain that the union of iodine and potass in the presence of alcohol, causes the formation of iodoform; but the theory of this formation is still very obscure. Mitscherlich, Bouchardat, and Liebig, have all given different formulas of the reaction. Bouchardat, indeed, in one paper has given two or three (See *Recherches sur l'Iodoform, &c., par A. Bouchardat, Journal de Pharmacie, Janvier 1837*), admitting, at the same time, that they are not in accordance with actual results. According to Liebig, the following would be the reaction:— $C_4 H_5 O + H O + I_3 + 6 K O (C_4 H_6 O_8 I_3 K_6) = C_2 H I_3 + C_2 H O_3 K O + 5 K I + 4 H O (C_4 H_6 O_8 I_3 K_6)$. But this formula cannot be wholly correct, because formic acid, formic ether, and iodate of potass, are formed in the process. Of course this formula would give 1 atom of alcohol, 8 of iodine, and 6 of potass, equal to, in the decomposition, 1 atom of periodide of formyle (iodoform), 1 of formiate of potass, 5 of iodide of potassium, and 4 of water. But if we mix the ingredients in the proportion indicated by Liebig's formula, we shall get hardly any iodoform.

Iodoform, according to my observations, is best prepared by carefully adding an aqueous solution of potass to the simple tincture of iodine of the Edinburgh Pharmacopœia until discoloration is effected. Care must be taken, however, to leave a slight excess of iodine. I find that 252 grains of iodine dissolved in 6 ounces and 3 drachms of rectified spirit, and 127 grains of hydrate of potass in 3 ounces of water, with the addition afterwards of 31 oz. of distilled water, gave 38 grains of iodoform. Such is the statement of a gentleman in this town, who lately performed the experiment for me. Of course, were the substance prepared on the large scale, the alcohol could be recovered by distillation, and the iodine also, either in the elementary state, or as iodide and iodate of potass, mixed with formiate of potass. Iodoform can be prepared with naphtha; but when so prepared, it has not so agreeable an odour as when obtained from alcohol.

Iodoform, insoluble in water, is very soluble in alcohol, ether, and wood spirit. It crystallizes in brilliant yellow plates from the alcoholic solution. It is volatilized at 212° , and decomposed at a somewhat higher temperature. Its smell is exceedingly powerful, and to me not disagreeable; although, in houses where I have used it, complaints have been made of the powerful smell of the ointment.

The taste is somewhat sweet, but not so agreeable as that of chloroform.

All that we know of its physiological properties, except from its analogy with chloroform (so far as direct experiment goes), is derived from an experiment recorded by Dr Cogswell in his elegant work on iodine (p. 122). He calls it sesquiodide of carbon (after Serullas). The following are the details of this most interesting experiment:—“At eleven o'clock A.M., an active, strongly made, terrier dog, was made to swallow fifty grains of the sesquiodide of carbon, concealed in a loose fold of paper. The day following, the animal was in good spirits, and seemingly quite unaffected, except so far as was indicated by a disregard for nourishment. On the third day he was indisposed to rise, and at length made but feeble efforts to crawl, the heart beating irregularly, and the belly drawn in towards the spine. For the whole of the fourth day the animal lay stretched out on the side, and gave no sign of recognition; the head extended, each respiration accompanied by a hollow moan, and followed by a universal convulsive movement, the mouth closed, eyes open, pupils natural, eyelids winked as usual. Death took place in the night.”

He found, on inspection, great rigidity of the muscles, great congestion of the lungs, and marks of irritation, with a blackish appearance in the stomach; all appearances proving the strict analogy in physiological action between this body and chloroform. Appended is an illustration of the action of chloroform on the stomach of a rabbit, which will show a similar kind of action.¹

With regard to the medicinal use of iodoform, according to Dr Cogswell, Dr Litchfield had used it with advantage in cases of enlarged glands, and in porrigo and lepra in the form of ointment, composed of half a drachm of the iodoform to six drachms of simple cerate.

I have used the iodoform most extensively in skin diseases, in psoriasis, impetigo, porrigo, and in one or two aggravated cases of itch (*grosse gale*). In all these affections it was very successful. Externally, conjoined with its internal use, in four cases of goitre. In two of the cases of goitre, the enlargements were small and of short continuance; but disappeared rapidly under the influence of iodoform. The dose was three grains thrice a-day in those cases (both young women). The enlargement disappeared in the one case in three weeks; in the other in a fortnight. The ointment of one drachm of iodoform to the ounce of simple cerate being rubbed upon the tumours.

The old case of bronchocele, referred to in my paper on bromine, was a Miss M. of Tanfield, aged thirty-eight, who had had bronchocele for more than seven years. The swelling was as large as a

¹ The blood, brain, spinal cord, muscles, etc., were impregnated with iodine. I have elsewhere noticed the singular power of permeating the tissues which chloroform possesses.

good-sized turnip. She had not menstruated to any proper extent for years; of stout habit of body, but not very regular; tongue tolerably clean. Pulse 100, when the treatment was commenced on the 12th of February 1842. The ointment of the strength above mentioned was applied externally, and two grains in pills given thrice a-day. On the 27th of April the tumour was much reduced. She had menstruated freely, but complained of being much weaker. Had made much more water. Appetite better; pulse 90; tongue clean. The treatment was continued with occasional intermissions until the 11th of June, when the tumour was reduced to the size of a walnut. I saw her about a year after; the tumour was still of the same reduced size.

Another case of goitre was a girl of eighteen; lived at Killingworth, January 31, 1844. Had a goitre for four years. General enlargement of the thyroid, but particularly the left lobe; three inches in one direction, taking the highest point of the left lobe, and about two inches in breadth. The tumour obstructed swallowing, and produced nausea after eating. She was also subject to headaches. Menstruated regularly. Pulse 120; tongue clean; bowels regular. She had 3 grs. of iodoform thrice a-day, and the ointment externally. On the 14th of next month the tumour was decidedly smaller, and her general appearance greatly improved. The treatment was continued until September, with occasional omissions. The goitre might then be said to be gone. All the functions were regular; the pulse normal, and her strength was greatly improved. The principal physiological effect noticed during the administration of the iodoform, was a great increase of the urine. My friend Mr Potter saw this case, and was desirous at the time that I should again call the attention of the profession to iodoform.

In conclusion, these remarks are offered, of course, chiefly as suggestions. That so remarkable a substance possesses medicinal powers of no ordinary character is pretty certain. I hope that some one who has extensive opportunities of trying it in practice, will avail himself of it. It appears to me to possess a union of tonic, stimulant, and alterative properties, which perhaps no other substance can have in the same manner, united with a remarkable action on the nervous system. It remains to be seen whether extended observation will confirm these ideas. The dose might probably be much greater than what I used.

Explanation of the Plate.

The plate represents the stomach of a rabbit poisoned six or seven years ago by chloroform; *a* is the cardia, *b* the pylorus. It shows the irritation which the poison has produced, and the curious blackening occasioned by all the class to which chloroform belongs, and of which I have still better specimens.

ARTICLE V.—*Report of the Surgical Operations performed at the Newcastle-upon-Tyne Infirmary, during a period of seventeen years and a half.* By SAMUEL FENWICK, M.D., Lecturer on Pathological Anatomy at the Newcastle-upon-Tyne School of Medicine and Surgery.—(Concluded from page 495.)

8. *The influence of Season on the results of Amputations.*

THERE are few circumstances whose effects upon the results of operations are more obvious, and there are few to which less attention has been directed, than the influence of season. In this part of our investigations, I can bring forward but few authorities in support of my own conclusions; and this is the less to be regretted, since, from the variations in the weather in different places at the same time, it is probable the records of different hospitals would show very opposite results. In Tables XLI. and XLII. I have given the months in which deaths took place, after amputations of the limbs, at the Newcastle Infirmary, the operations being classified according to the cause which required their performance.

TABLE XLI.

Shows the Number of Deaths which took place, after Amputations of the Lower Extremity, in each month, at the Newcastle Infirmary.

	Diseased Joints & Bones.			Other Diseases.			Traumatic Amputations.			Total.		
	Within 4 Days.	4 to 21 Days.	After 21 Days.	Within 4 Days.	4 to 21 Days.	After 21 Days.	Within 4 Days.	4 to 21 Days.	After 21 Days.	Within 4 Days.	4 to 21 Days.	After 21 Days.
January,.....	1	...	1	2	...	1	3	...
February,.....	1	1	1	1
March,.....	1	1	2
April,.....	2	2	1	...	2	1	2	4	2
May,.....	...	1	1	1	1	1	1	1	...	2	3	2
June,.....	2	...	1	2	...	1	4	...
July,.....	1	...	2	2	1	...
August,.....	2	2	...
September,.....	1	...	1	1	1	...
October,.....	...	1	1	1	...	2	1
November,.....	1	1	1	...	1
December,.....	1	1	...	1	2	1

TABLE XLII.

Shows the Number of Deaths which took place in each month, after Amputations of the Upper Extremity, at the Newcastle Infirmary.

	Diseased Joints & Bones.			Other Diseases.			Traumatic Amputations.			Total.		
	Within 4 Days.	4 to 21 Days.	After 21 Days.	Within 4 Days.	4 to 21 Days.	After 21 Days.	Within 4 Days.	4 to 21 Days.	After 21 Days.	Within 4 Days.	4 to 21 Days.	After 21 Days.
January,.....	2	2
February,.....
March,.....	1	1	...
April,.....
May,.....
June,.....	...	2	1	1	3	...
July,.....	1	1	...	1	1	...	1
August,.....	1	1
November,.....	1	1

We find, then, that the months which have been most fatal to persons suffering amputation, are April, May, and June. Of the cases of amputation of the lower extremity, 20 deaths had occurred in this quarter of the year; whilst, in no other similar period, had the

mortality exceeded 8. In the cases of the operation performed upon the upper extremity, the difference is less perceptible, an equal number of deaths having occurred in each of the three first quarters of the year.

We might have supposed, that the depressing effects of cold would have given a greater tendency to death from shock within the winter months, but this is not the case. In those cases of amputation which are least liable to sink from exhaustion, viz. in the pathological class, the only deaths which occurred within the first four days after the operation, were in the second quarter, and this is chiefly observable in the amputations of the lower extremity. Of the traumatic amputations, the greatest number of deaths had taken place within the first four days within the second and third quarters of the year, whilst only 1 patient had perished within that period in the first and last quarters of the year.

If we consider the greater number of deaths occurring between the fourth and twenty-first days as arising from inflammatory complaints, we shall find that the spring is most liable to induce these affections. Thus, according to Tables XLI. and XLII., in the second quarter 14 deaths took place, in the first 5 deaths; whilst in each of the remaining quarters only 4 deaths had occurred. June is the most fatal month, no less than 7 deaths between the fourth and twenty-first days after the amputation being recorded in it, or nearly double the number that is observed in the whole of the summer and autumn quarters.

Amongst those who had survived the operation 21 days, the greatest number of deaths took place in the first quarter of the year, and the smallest mortality was observed in the third. In the spring, 4 deaths took place, and in the autumn only 3 fatal cases were remarked after the twenty-first day.

In the following Table the mortality is shown with respect to the months in which the operation was performed, and the cases are likewise divided according to age.

TABLE XLIII.

Shows the mortality which followed amputations performed for diseased joints and bones in each month, at the Newcastle Infirmary, separating the amount of death within four days from that which occurred afterwards.

DISEASED JOINTS AND BONES.

	LOWER EXTREMITY.								UPPER EXTREMITY.							
	Under 30 Years of Age.				Above 30 Years of Age.				Under 30 Years of Age.				Above 30 Years of Age.			
	No.	Mor. in 4 days.	Mor. aft. 4 days.	Av. Cure.	No.	Mor. in 4 days.	Mor. aft. 4 days.	Av. Cure.	No.	Mor. in 4 days.	Mor. aft. 4 days.	Av. Cure.	No.	Mor. in 4 days.	Mor. aft. 4 days.	Av. Cure.
January,	4	37	2	...	2	132	1	54
February,	5	64.5	6	58
March,	4	...	4	52	5	5	2	32	2	64.5	2	42
April,	7	7	6	50.6	7	...	1.5	45.2	1	58
May,	4	36.6	3	44	1	24	1	16
June,	7	56.6	2	1	...	1	...	1	...	1	...
July,	5	45.4	1	...	1	1	36
August,	4	41.7	2	50.5
September, ...	9	41.4	1	65
October,	7	38.8	3	...	1.5	1	24
November,	5	72.2
December,	5	55.2	1	30	1	38

Of those who suffered amputation for diseased joints and bones below the age of 30, no deaths had taken place, except amongst those operated upon in March and April, as regards amputations of the lower; and none, excepting amongst those whose amputations were performed in June, as regards those of the upper extremity. This is worthy of remembrance, as it is usually in our power to select a time for the performance of the operation upon such cases. In the healthy months no deaths had taken place out of 60; whilst in the 3 months above mentioned as fatal ones, 4 deaths had occurred in every 12 amputations, or 1 in every 3.

It would appear, however, that the seasons have a very different effect upon the constitution in other places, as, according to Malgaigne, amongst the cases of pathological amputation performed in the hospitals of Paris, of those below 20 years of age, 50 per cent. died in the winter, 40 per cent. in the autumn, 36 per cent. in the spring, and 35 per cent. in the summer.

At the Newcastle Infirmary, of those above 30 years who suffered amputations for diseased joints or bones, the months of March and April are again characterised by great mortality, 5 having died in them out of 14 cases of amputation, or 1 in every 2·8; but we also find January, July, June, and October exhibiting a certain amount of mortality. When required, then, to fix a time for performing amputation for diseased joint in the case of a person above 30 years of age, we should always, in this part of the country, avoid, if possible, its performance in March, April, or October, selecting chiefly the summer or the winter months.

TABLE XLIV.

Shows the Success following Amputations, performed for Ulcers and other Diseases, in each month, at the Newcastle Infirmary.

AMPUTATION FOR ULCERS AND OTHER DISEASES.

	Jan.	Feb.	Mar.	Apr.	May.	June	July.	Aug.	Sept.	Oct.	Nov.	Dec.
LOWER EXTREMITY.												
Number of cases,.....	2	...	3	3	2	3	5	...	3	2	...	2
Mortality in first 4 days,.....	2
" after 4th day,.....	2	...	1	3	1	1·5	5	2	...	2
Average cure,.....	37	63·5	..	30	54·6	...	51·5	100
UPPER EXTREMITY.												
Number of cases,.....	...	1	1	2	1	1	1
Mortality in first 4 days,.....	1
" after 4th day,.....	...	1
Average cure,.....	49	48	44	29

Here we find the amputations performed for ulcers and other pathological causes in March, as unsuccessful as were those for diseased joints in the same month. Those performed in April have been more fortunate; but the two following months exhibit a vast amount of death, 4 having perished out of 5. The summer is the only quarter of the year in which the mortality has been moderate, only 2 deaths being recorded out of 10 operations.

In all pathological amputations, then, we discover a great effect, produced by the different seasons in which they are performed, and the explanation of the fact is well worthy of attention. But we are unable to bring forwards any theories worthy of notice. It is evident, that the occurrence of the diseases producing death must be regulated by certain laws; and yet, from want of more minute statements of the diseases causing death, the period of their attacks, and the state of the weather at the time they were first observed, we are unable to point them out. In every hospital the operations should be most minutely and carefully registered, in order that the observations made in its wards, might indicate to the practitioners of the neighbourhood the seasons in which, with least risk, these operations can be performed.

In the traumatic amputations, although we have not the power of selecting the time at which we will remove a limb, yet the study of the influence of the seasons upon their results is likely to be exceedingly valuable, both in enabling us to anticipate particular diseases, and also in directing us with respect to the propriety of immediate or secondary amputation.

TABLE XLV.

Shows the success following Amputations for Accidents according to the Months in which they were performed at the Newcastle Infirmary.

TRAUMATIC AMPUTATIONS.

	LOWER EXTREMITY.								UPPER EXTREMITY.							
	Under 20 Years of Age.				Above 20 Years of Age.				Under 20 Years of Age.				Above 20 Years of Age.			
	No.	Mor. in 4 days.	Mor. aft. 4 days.	Cure.	No.	Mor. in 4 days.	Mor. aft. 4 days.	Cure.	No.	Mor. in 4 days.	Mor. aft. 4 days.	Cure.	No.	Mor. in 4 days.	Mor. aft. 4 days.	Cure.
January,.....	5	5	4	61·5	2	78
February,.....	2	...	2	1	70
March,.....	1	94	1	44	1	57	1	77
April,.....	1	86	1
May,.....	1	1	4	...	2	52	4	...	4	69	1	65
June,.....	3	3	2	...	3	...	3	68	2	...	2	36
July,.....	3	1·5	2	71·5	3	3	...	52
August,.....	2	34	4	...	2	...	2	73·5
September,....	1	4	4	1·5	54	2	27
October,.....	1	77	1	72	1	76	1	26
November,....	1	1	2	36	2	53·5	3	3	...	40
December,....	1	96	4	...	1·33	82	2	...	2	33	2	...	2	82

In all calculations respecting the mortality following traumatic amputations, it is a matter of the greatest moment to separate the deaths according to the time after the operation at which they took place; for it is evident that those who sink from the shock of an accident, cannot equally display the influence of circumstances with those who perish from subsequent disease.

Of those who had undergone the removal of the lower extremity, and who were under 20 years of age, none died after the fourth day; but, in the case of amputations of the upper extremity, 2 deaths took place, and these occurred amongst those operated upon in the

two months we before remarked as so unhealthy, viz. in May and June. Of the cases of amputation above 20 years of age, with respect to the upper extremity 2 deaths took place after the fourth day, and of these 1 had the arm removed in June and the other in December. In the amputations of the lower extremity, in those above 20 years of age May and June seem to have been, as usual, exceedingly fatal; but the other quarters of the year do not present the same amount of success we found them to exhibit in the former Tables. Thus, if we divide the numbers contained in this class of amputations according to the quarters of the year in which they were performed, we shall find that 1 case in every 3·5 died after the fourth day in the first three months, 1 in 2·33 in the second, 1 in 2·25 in the third, and 1 in 2·33 in the fourth quarter.

From this, then, we may conclude, that although the amputations performed in the second quarter of the year, viz. in April, May, and June, are most likely to prove unsuccessful in traumatic amputations; yet in the case of the lower extremity, in those above 20 years of age, the increase in mortality will be very little above that of the other periods of the year.

It will be observed, on comparing Table XLV. with the two tables preceding it, that although the spring is by far the most fatal to all amputations; yet, that the pathological amputations performed in the earlier months of that period are more unsuccessful than those performed in June, whilst the contrary is remarked in those requiring the operation on account of accidents.

I have also classified the amputations performed at the Newcastle Infirmary according to the trades of the patients, but have not published the results for the following reasons.

1st, From the occupations of the patients not being noted either in the general registers of the hospital, nor in the books of the surgeons, I have had no means of verifying the correctness of, or detecting the errors in, the account of the operation books; and 2dly, as, in many instances, the occupation is only stated in general terms, such as mechanic or labourer, the want of exactness appeared to me to deprive the results of much of the interest which would otherwise have been connected with them.

ARTICLE VI.—*Cases in Surgery.* By JAMES SPENCE, Surgeon.

CASE I.—*Case of Ligature of the Axillary Artery for Hemorrhage resulting from a Burn of the Arm.*

DAVID HENDERSON, æt. nine years, met with a severe burn of his left arm, on the 20th December 1845, owing to his clothes taking fire.

In the absence of Mr Kerr, the usual medical attendant of the family, another medical gentleman dressed the wound with cotton

wadding; but owing to the offensive smell of the discharge, and to see the full extent of the injury, Mr Kerr removed this after some days. He then found that the true skin was very severely injured, and that the burn extended from the lower part of the axilla to near the hand, and in the upper arm was chiefly situated towards the inner side.

Stimulating applications and poultices were now used, and in a short time sloughs separated from the injured surface above and below the bend of the arm, and the sore healed kindly enough at several points. At the bend of the arm, the sloughs were very deep and long in separating, and Mr Kerr watched their separation with considerable anxiety for fear of hemorrhage taking place.

On the evening of the 6th of January 1846, a portion of slough came away, and was followed by considerable loss of blood. Mr Kerr and Dr Duncan saw the boy shortly after this, and as the bleeding was then evidently venous, they arrested it by bandaging from the fingers upwards, and placing a graduated compress over the bleeding point. This completely arrested it until the evening of the 8th January, when a deeper portion of slough separating, sudden and profuse hemorrhage took place; Mr Kerr attended almost immediately, and seeing the bleeding was now arterial, arrested it by compressing the humeral till I arrived, but the child had already lost a very large quantity of blood. On examining the arm, the whole extent of the limb, from the lower margin of the axilla to the hand, exhibited the effects of the burn, being raw and discharging freely. On removing a piece of lint from over the bend of the arm, a deep cavity was exposed, and at the bottom of it the brachial artery was seen, with a small oval opening in its anterior wall; when the pressure on the brachial was relaxed, a jet of blood came from this opening, so as to leave no doubt as to the source of the hemorrhage. From the appearance of the opened vessel, and the sloughing state of the surrounding parts, I considered it improper to trust to ligature immediately above and below the opening; whilst the state of the arm, together with the impaired vitality of the skin and other tissues, rendered ligature of the brachial in the middle, or higher in the arm, equally inadvisable. I therefore determined to tie the axillary in the third portion of its course, where it lies on the tendon of the latissimus dorsi; with this view the arm was separated from the side, and extended as far as could be done under the circumstances.

I made an incision about $2\frac{1}{2}$ inches long, commencing about an inch and a half within the anterior margin of the axilla, and carried down along the inner edge of the coraco-brachialis; the fascia was divided to the same extent, and the plexus of nerves brought into view. I next carefully separated the median and internal cutaneous nerves, which exposed the vessel, and, after carefully cleaning it, passed a ligature round it and tied it. There was a good deal of troublesome oozing of blood during the dissection, owing to the raw and vascular state of the skin at the lower part of the incision, and

considerable delay and annoyance were caused by the unmanageable state of the patient (who, I should have mentioned, was deaf, dumb, idiotic, and epileptic). At the commencement of the operation, he thought proper to amuse himself by blowing out the candle, and when I was about to pass the ligature round the artery, he took an epileptic fit.

After the vessel was tied, the edges of the upper part of the wound were brought together by points of suture, and the ligature allowed to hang out at the lower part. After waiting a short time, slight bleeding was noticed to take place from the lower part of the opening in the brachial artery; I therefore placed ligatures above and below the opening, as compresses seemed to give rise to much irritation, and as I trusted there might be sufficient vitality of the coats of the vessel there to sustain the ligatures for a day or two till more permanent changes at the part, and in the collateral circulation, had taken place. The burned surface was then dressed with a stimulating lotion.

Next day I found that the patient had passed a very restless night, constantly moving the injured arm, but no further bleeding had taken place. On the evening of the 11th January, the ligatures above and below the opening in the brachial artery came away, but no more hemorrhage took place. The ulcerated surface at the bend of the arm healed kindly.

The edges of my incision at the lower part, where it had reached the injured skin, looked sloughy; but the upper part of the incision, corresponding to the point where the vessel was tied, had united, and I removed the stitches. As the patient still continued restless and unmanageable, constantly moving the arm and disturbing the dressings, it was found necessary to muffle the opposite hand and secure the injured arm towards the side. The sloughy appearance of the lower part of the incision increased for some days, and gave me some anxiety; stimulating dressings were applied to it, and, on the ninth day after the operation, a slough separated which relieved our fears, as it was found not to extend deeper than the skin, and the parts beneath were healthy in appearance; from this time every thing went on well in spite of the unfavourable nature of the case, and the unmanageable state of the patient.

The ligature from the axillary came away on the sixteenth day after the operation, and the incision healed rapidly. After this I did not attend him, but Mr Kerr informs me that cicatrization of the whole surface was not effected until the end of February, and that now the contraction of the hand and fore-arm towards the shoulder is very great. He also mentions that, strangely enough, the first time the boy was left alone he ran to the fire-place (fortunately empty), and thrust his injured arm between the bars of the grate.

Remarks.—In this case it will be observed, that whilst the injured vessel was situated (as regarded retrograde bleeding) much in the

same circumstances as an artery wounded by a cutting instrument, it differed from it in this material point, that the vitality of its coats was impaired, and partook in the weakened action of the surrounding parts from the effects of the burn; so that I could not have trusted to ligature above and below the opening, because the action of the ligature on the vessel would have proceeded too rapidly, and the impulse of the circulation would, in all probability, have caused secondary hemorrhage in a day or two, on the decidence of the ligature. On the other hand, ligature of the arterial trunk higher up, whilst it arrested the flow of blood immediately from the upper part of the vessel, would not have sufficed to arrest the retrograde bleeding; and hence I adopted the practice of tying the main trunk high up, where it was healthy, and situated amongst healthy parts, to divert the impulse of the blood from the canal of the wounded vessel; whilst the ligatures above and below the opening at the bend of the arm, prevented the retrograde bleeding sufficiently long to allow of consolidation of the surrounding textures, and permanent obliteration of the vessel at that point, and also by avoiding the necessity for bandaging and compresses; the necessary dressings to the general burned surface were not interfered with. Of course, the nature of the agent causing the sloughing must be taken into consideration, as this may materially influence the line of practice.

Whilst writing out these remarks, I was called to a young gentleman in the country, who, by applying nitrate of silver over the course of the radial artery, had opened that vessel; the eschar separated whilst he was out shooting, and he had lost a very large quantity of blood. In this case I merely cut down upon, and tied the vessel a short distance above and below the wounded point; because I knew that the adjacent structures, after the separation of an eschar caused by nitrate of silver, are generally in a state of healthy action, and not in that condition of impaired vitality which characterises the tissues implicated in a severe burn by fire.

CASE 2.—*Amputation of Arm for the secondary effects of a Burn.*

I was consulted by Mr Falconer of Loanhead, in the case of a patient of his, a girl aged eleven years, whose health was sinking under the secondary effects of a burn. About two years previously she had been very severely burnt in the arm whilst playing near the fire. She recovered from the first effects of the injury, although with great contraction of the wrist and elbow. After the burned surface had apparently healed for some weeks, the newly-formed skin gave way at several points, and a discharge took place from the ulcerated surface. Various remedies were had recourse to for the purpose of checking the discharge, and promoting the cicatrization of the parts, but all had ultimately failed. The ulceration had increased rapidly, and the patient's health had gradually sunk under the long-continued discharge and irritation. When I first saw her, she was apparently

in the last stage of hectic, and worn almost to a skeleton, with rapid weak pulse.

The injured arm, from the wrist to the middle of the upper arm, presented an almost continuous ulcerated surface, which was discharging profusely, and so irritable, that she screamed out whenever the dressings were touched. Over the posterior part of the elbow, there was a cicatrix, corresponding to the position of the ulnar nerve, between the olecranon and inner condyle, which, she stated frequently, gave her excruciating pain; her mother stated, that she seldom slept at night, and when she did, was bathed in perspiration, and that her appetite had latterly almost entirely failed.

Under these circumstances, I said, that I considered amputation the only chance of saving life; and that, although she seemed so much sunk, I would still recommend it as a means of relief from her sufferings. To this, both the parents and the patient readily consented, and I accordingly amputated the arm below the insertion of the deltoid by double flap. The operation seemed to afford almost immediate relief from the constitutional irritation; for she slept well the night after it was performed, which she had not done for months previously, and the wound healed almost entirely by the first intention. As she resided some distance in the country, I did not see her after the first fortnight until about six weeks from the date of the amputation, when she called upon me in town perfectly restored in health.

On dissection of the amputated limb, which had been previously injected with size by my friend Mr Goodsir, the muscular tissue was found quite altered in structure, of a pale, almost white, colour, and scarcely showing any appearance of fibrous texture. The ligaments were thickened, and so contracted as scarcely to admit of the slightest motion, even after the division of the other soft parts; but the synovial membrane and articular cartilages were perfectly healthy, and presented no appearance of ulcerative absorption at any point. The ulnar nerve was found involved, and apparently compressed, by the consolidated tissues corresponding to the cicatrix behind the elbow. All the deep-seated parts of the limb seemed much less vascular than usual, probably owing to the consolidation of the tissues through which the vessels passed; but this appearance might also, in some measure, have been caused by the injection escaping more readily from the open vessels of the ulcerated surfaces.

CASE 3.—*Injury of Axillary Artery from Fracture of the Neck of the Humerus.*

Mr J. B——, aged sixty-three, in coming down-stairs from the drawing-room, tripped on the carpet, and fell down with his whole weight on the elbow and point of shoulder. I was sent for on the part of Dr Lawrie, who attended the family, and found the humerus broken at the neck, the broken end of the shaft considerably displaced inwards to the axilla, and the elbow tilted outwards.

Mr B—— was exceedingly sick and faint, having only a short time previously recovered from a severe attack of rheumatic fever, and suffering, at the time of the accident, from bowel complaint.

The fracture was readily adjusted, scarcely any force whatever being necessary to extend the shaft of the bone, and withdraw it from the axilla. I put it up in the usual manner, with a soft compress in the axilla, placing the fore-arm in a sling, and confining the arm and elbow to the side by means of a broad soft shawl. I then ordered him an opiate to arrest the diarrhoea, and left him.

Next morning I saw him with Dr Lawrie; he had passed a very restless night from the bowel complaint, and also from pain at the fractured part, and numbness in the hand. On loosing the bandages, the hand and fore-arm were found not much swollen, but they were much colder than those of the opposite side, and no pulsation could be felt either in the brachial, radial, or ulnar arteries at any point, but distinct and full pulsation could be felt in the axillary, immediately above the fracture; there was considerable ecchymosis and bruising over the point of the shoulder and deltoid, also some swelling along the inner side of the biceps, but there was no pulsatory movement in the swelling, nor any appearance of its communicating with the artery. It was evident, however, that the axillary had suffered, that circulation through it was obstructed, and that the treatment of the fracture must be considered secondary to that of the injury of the vessel. I accordingly did not replace the pad in the axilla, but got a soft cushion made sufficiently long to reach from the axilla to the hand, jointed at the elbow, and thicker above than below. This I placed on the inside of the arm, and another soft cushion and thin pasteboard splint on the outside of the arm and fore-arm; the elbow was kept only slightly bent, the splint and cushions secured with slip-knots, and the whole limb covered with flannel.

This plan, whilst it interfered but little with the restoration of the circulation through the collateral branches, kept the broken ends of the bone pretty well in position. I continued it for the first fifteen days; when, although I could feel no pulse in the larger vessels, I was satisfied, from the restored heat and sensation of the part, that the circulation was re-established sufficiently to allow of the ordinary apparatus being used. Accordingly, I replaced a soft elastic hair cushion in the axilla, and a leather splint, well padded, along the outside of the arm and fore-arm (from the point of shoulder to extremity of the fingers). The fore-arm was supported in a sling, and the arm kept to the side by a soft shawl.

Under this treatment the cure went on favourably, except that, about the end of third week, he complained much of severe pain in the part, and of a sense of suffocation, and of a feeling of weight across the chest, which he attributed to the position of the fore-arm across the body. The sling was lowered so as to change this position, but still the feeling continued; alterative medicine was given with some relief.

As it became of great importance for him to get home to London at this time, and as the union of the bone seemed sufficiently firm to warrant his removal, he left Edinburgh, with my consent, on the 17th of August. Since then, I received the following accounts of his progress from his usual medical attendant, Dr Bower of Hatton Garden, and subsequently from Mr Liston, to whom I had requested him to apply, but whom he had not consulted till 1st October.

“As Mr B—— has now been under my watching more than a month, I am able to give you an account of his progress more satisfactorily than if I had written sooner. The callus around the fracture is completed, and he begins to move his arm somewhat, and in time doubtless it will turn out all right. I cannot, however, say as much for the circulation through the fore-arm and hand. I can only distinguish a very faint pulsation in any artery beneath the fracture. At the wrist there is none either in the radial or ulnar, and therefore I suppose the circulation through the hand must be by means of the interosseous or carpal. As you may suppose, the hand is not properly supplied, and much colder and more useless than it otherwise would be. I have adopted gentle friction and warm clothing to remedy this, but I expect more benefit from time than from either.

“From this statement I think you will agree with me, that, considering the severe nature of the injury, he is doing well; and, as I do not perceive any symptoms of disease or aneurism in the brachial artery, I trust he will go on uninterruptedly to recovery.”

“October 1st, 1847.—Yours, dated 17th August, only came to hand this morning.

“Mr B——’s case is a very interesting one, and one that must have required anxious and careful management. It is a capital cure; there is pulsation in the brachial, and a certain thrill at the wrist; there is a good deal of œdematous swelling on the inside of the arm, and in the sheath of the biceps, but I can detect nothing wrong in the axilla; no tumour, no unusual beating; so far so good. I should not apprehend any formation of aneurism now.”

Remarks.—The complication of fracture of the neck of the humerus, with the untoward injury detailed in the foregoing case, is fortunately of very rare occurrence, and I cannot help attributing it, in the present instance, more to the state of the vessels in this gentleman, than to the effects of direct injury of the artery by the broken end of the bone; in other words, I consider that the internal and middle coats of the vessel, softened and altered in structure by the previous rheumatic affection, had given way, at the time of the injury, from indirect violence, whilst the cellular coat had been stretched and twisted so as to obstruct the flow of blood through its canal. I am led to this conclusion by the following circumstances in the case:—1st, There

was no unusual swelling or appearance of extravasation in the axilla at the time of the accident, as would have occurred had a large vessel been torn through by the sharp fractured end of the bone. 2d, Though the displacement of the shaft was considerable, it was not greater than I have frequently seen in fracture of the neck of the humerus, and, owing to the faintness of the patient, there was little or no force required to extend it, and produce coaptation. 3d, No false aneurism has subsequently formed, which would also have probably been the case, had all the coats of the artery been torn by the broken end of the bone.

The method of treating the fracture which I adopted, was had recourse to on the spur of the moment, the principal point kept in view being not to interfere with the circulation; it answered the purpose sufficiently well, though, I daresay, many improvements could be suggested.

Part Second.

REVIEWS.

Researches into the Pathology and Treatment of the Asiatic or Algid Cholera. By E. A. PARKES, M.D., &c. 8vo. London: 1847. Pp. 250.

The Cholera not to be arrested by Quarantine, &c. By GAVIN MILROY, M.D., &c. 8vo. London: 1847. Pp. 51.

Le Cholera Asiatique. Annales de Thérapeutique. Decembre, 1847.

THE Asiatic Cholera.—What are its causes; how is it propagated; what is its nature; and how should it be treated? These are questions which, for very sufficient reasons, are once more seriously engaging the thoughts of the people of this country. Medical practitioners are, of course, the only persons from whom any information on the subject is to be gained. And the next question which naturally arises is, are the members of the profession themselves much enlightened on these essential points? To this question we have no hesitation whatever in replying in the negative. Notwithstanding all that has been written and said on this important and terrible disease—although the most talented and experienced physicians scattered all over the world had ample opportunities fifteen years ago of observing it—nay, though more recent and limited epidemics in our Indian territories, as in those recorded by Dr Parkes, have permitted medical men to watch the phenomena accompanying it—we will venture to affirm, that every thing connected with its true pathology and treatment is as yet unknown. The reason of this, however, is by no means difficult to

discover. So-called "practical men" had every facility afforded them, during the last epidemic, of deriving advantages from empirical modes of treatment. The study of symptoms, though carried as far as it could well go, and the attempts at relieving them without a knowledge of the pathology of the disease, could not lead to correct principles of practice. Cholera, once fairly established, it was found that astringents and enemata did not stop the diarrhœa; carminatives and opiates did not check the vomiting; anodynes and antispasmodics failed to relieve the cramps; stimuli were useless during prostration; and the deadly cold was unconquered by heat. As empirical means then have failed, we must seek for some rational method of guiding our treatment. For this purpose investigations must be instituted into the ultimate changes produced in the tissues and in the blood; new facts must be arrived at by the researches of the organic chemist and the histologist; the modern chemico-physiological mode of investigating disease must be pursued at the bedside, and all the therapeutic appliances of the present day summoned to assist the practitioner. Whilst we consider that such ought to be the methods of investigating the cholera in future, we are far from considering that no advantages are to be derived from the experience of the past. On the contrary, we feel satisfied that the value of the facts already elicited, although for the most part negative, is not the less real; and that a knowledge of them, by teaching us to avoid what is hurtful, and by pointing out what has already been proved to be useless, may be the indirect means of enabling us to approximate towards, if not actually reach, satisfactory principles both of prevention and cure.

That another epidemic of Asiatic or malignant cholera is approaching, we shall see there can be little reasonable doubt. In order, then, that our readers may be prepared to receive it with a perfect acquaintance of all that is known on the subject, we purpose laying before them a short account—1st, of the epidemic of 1817, which reached this country in 1831–2; 2d, of the present epidemic which seems to be approaching; 3d, of what is known of the nature and pathology of cholera: 4th, of its mode of transmission; and, lastly, of its treatment.

In drawing up this article, we have availed ourselves principally, although not exclusively, of the three works placed above. Dr Parkes' book gives an account of the disease as he witnessed it in India, during two severe epidemic attacks in 1843 and 1845, when he was serving as assistant-surgeon in one of H. M. regiments. It is very ably executed. Dr Milroy's pamphlet gives a short history of the late and present epidemics, with forcible arguments to show that cholera cannot be prevented by quarantine. He also makes remarks on the treatment. The article in the *Annales de Thérapeutique* is an exposition of the views of Professor Giacomini of Padua, as published by him in 1836, and confirmed by the later writings

of MM. Biaggi and Gargnani in 1844-45. Although the general pathological doctrines of Giacomini, in our opinion, scarcely merit a moment's consideration; in matters of fact and in clinical observation his writings well merit attention. It seems possible to us also, that his notions regarding inflammation of the vessels, as the cause of chlorosis and many other diseases, and among the rest cholera, may only appear to us absurd from a misinterpretation of the words he employs. At least he has applied the terms arteritis, phlebitis, &c., to many affections which we are in the habit of considering to originate in the blood, independent of vascular lesion; and it has occurred to us, that this difficulty removed or explained away, will enable us to derive from his labours much that is valuable and instructive. This we shall endeavour to do when speaking of the pathology of the disease.

HISTORY OF THE EPIDEMIC CHOLERA OF 1817.

By some authors, Asiatic cholera is stated to have existed in India as early as 1774. Curtis and Johnston especially have given very exact descriptions of it, the one as it appeared in Trincomalee in 1782, the other as observed by him in the naval hospital at Madras in the course of the same year. The epidemic, however, which later caused so much alarm, and spread over Europe, broke out suddenly in the summer of 1817 in various towns and localities in the delta of the Ganges. Some have stated that it commenced at Jessore, the capital of the Sunderbunds, and distant from Calcutta about sixty English miles. This, however, has been denied by many good authorities; and the map drawn up by Mr Orton, within two or three years after its first appearance, giving the dates of its commencement in various towns of the Indian peninsula, shows that it broke out in several places, some at great distances from each other, at the same time. Be this as it may, it seemed first to attract the attention of government when in Jessore during the August of 1817. Jessore is a crowded, uncleanly place, surrounded by impenetrable jungles, and consequently exposed to all the horrors of a malarious and ill-ventilated atmosphere. On the 28th of August it was reported to the government, that a malignant disorder had broken out, attacking all classes of natives promiscuously, and destroying from twenty to thirty inhabitants every day. So destructive was it in mortality, so unexampled in character, that, to use the language of the report, "The inhabitants, astonished and terrified at the unaccountable and very destructive inroads of the pestilence, fled in crowds to the country, as the only means of escaping impending death. So unforeseen and appalling was the attack, that the functionaries, in extreme consternation, closed the civil courts of the district, and business of every description was abandoned for a time." In the course of a few weeks, 10,000 of the inhabitants perished in this single district.

The disease reached Calcutta early in September, and, pursuing more especially the course of the Ganges and its tributary streams, it reached the interior of the country, and, on the 6th of November, attacked the grand British army, under the command of the Marquis of Hastings, on the banks of the Sinde, a tributary of the Jumna. The mortality was here appalling; 5000 men perished in five days, and the deaths on the whole amounted to 19,000. According to an account in the 16th volume of the Asiatic Journal, the invasion was so sudden and violent, that the horsemen were stricken from their steeds, and in vain attempted to remount, and the roads were covered with the dying and the dead. No new case occurred in the camp after the 18th of December, but the disease continued to spread to all the towns and villages of the district. In the month of March 1818, 10,000 Indians were destroyed by it in the town of Banda and its environs alone. Kotah is built upon a solid rock, along the east side of the Chumbul; yet here 100 persons perished daily for such a length of time, that, struck with dismay, the surviving inhabitants abandoned the city.

In April and May 1818, the disease spread through the middle and northern provinces of Hindostan. In the city of Guruckpore alone, 30,000 persons became its victims. In the north it was ascertained, that elevation of temperature exercised little influence on its severity. The high mountains which separate Hindostan from the Nepaul were invaded, as well as the high table-lands of Patum and Rhatgoun, which have an elevation of, at least, 4000 feet above the level of the sea. In a few months the whole Indian peninsula was affected; and if we take into consideration that in every place it was marked by the same destructive effects as in those already alluded to, there will be little reason to doubt the probability of the government calculation, which estimated the mortality in Hindostan at the awful total of 20,000,000 of individuals.

During the period which elapsed between the years 1819 and 1824, the cholera passed over the gulfs and arms of the ocean which wash the littoral boundaries of the Indian peninsula, and excited the utmost alarm in neighbouring nations. In this way it visited Ceylon, the Birman empire, and the Indian Archipelago, and spread from the coast of Malabar to the shores of the Persian Gulf. In the latter locality, it attacked, among other ports, that of Bender Abassi, and in a short time one-sixth of its inhabitants were cut off. Human misery, perhaps, never showed itself in more terrific colours than on this fatal occasion. The bazars were closed, the houses abandoned, the unburied dead lay heaped in the streets, and the surviving population sought safety in flight. At Shiraz, 100 leagues distant, the haram of the Prince Royal of Persia was almost the first arena for its fury. In this city, one-eighth of the inhabitants was destroyed, and the British consul, Mr Rich, a man famed for his zealous and able cultivation of science, was among

its earliest victims. From thence it spread through Persia, and passed into Syria and Mesopotamia. At Bassorah 18,000 persons, being one-third of the population, were swept away in eleven days. It continued its progress along the western coast of Syria; and the violence of the disease may be inferred from the following fact related by the British consul, Mr Barker. Twenty peasants of Swedia, robust, vigorous, and in the flower of life, were labouring at the harvest work, when, on the 9th of July, at noon, one was suddenly attacked, and the others in a short time showed symptoms of the disorder. In three hours the entire band were exhausted; before sunset many had ceased to live, and by the morrow there was no survivor.

Up to the years 1829 and 1830, cholera ravaged the provinces of Persia, and in the middle of the latter year attacked the Russo-Asiatic provinces of Schirran and Bakou. From thence it passed through the defiles of the Caucasus to Teflis, the capital of Georgia, and apparently by sea from the port of Bakou to Astrakan, in which city 4043 persons perished. It now proceeded through the Russian territories, extended itself along the course of the Volga, and reached Moscow in the middle of September, where, between this period and the 10th of November, it attacked 5451 persons, and killed 2876. It continued in Moscow all the winter; entered Poland in the beginning of 1831, and was very destructive at Warsaw in April and May. It now spread through the duchies of Posen and Silesia, and entered Prussia. Dantzic was attacked on the 26th of May; Petersburg in the month of June; and Berlin on the 31st of August. From Berlin it reached the Elbe, attacked the towns on its banks in succession, and arrived on the 7th of October at Ham-burgh. In the mean time, it had extended through Galicia and Hungary to Vienna, and other parts of the Austrian dominions. Smyrna and Constantinople were also affected, and Egypt was not spared. At Cairo especially, a dreadful mortality took place. The first officially declared case in this country, occurred in Sunderland on the 26th of October. Three or four weeks subsequently, it appeared at Newcastle, and in December at North Shields, Gates-head, Tynemouth, and other adjacent places, as well as at Had-dington in Scotland. In the spring of 1832, it reached London and Edinburgh. In March it was reported at Calais, and a fortnight later in Paris, in which city alone, 18,000 are reported to have sunk under its attack. According to a table drawn up by Dr Merriman, there died in England 14,807 persons; in Scotland 10,650; in Wales, 498; in the Isle of Man, 146; in London and its vicinity alone, 5275; and in Ireland, 21,171.

Proceeding further westward, cholera appeared at Quebec, and Montreal, and New York, in June 1832. "In July" (we now quote from Dr Milroy) "it spread to Philadelphia and several other cities of the United States, and thence over nearly the whole of the American continent. In the early part of 1833, it was at

the Havannah, and some others of the West India islands. From the New World, it seems to have wheeled round upon its march; for it was not until the following year that Spain, and, according to one account, Sweden also, was visited by the pestilence. It is worthy of notice that there was a partial and slight return of the disease experienced in this country in the course of the year 1834. In 1835, it re-appeared in the south of France, and, passing along the southern coast, attacked Genoa and some other towns on the shores of the Mediterranean; but it did not visit Rome or Sicily,—at least, with any degree of severity—until 1837, when it proved very destructive for several weeks in the Eternal City, carrying off, during the height of the epidemic, as many as 300 in the course of a day. In the course of that year, also, the disease again manifested itself at Dantzic, Berlin, and other parts in the north of Germany; and subsequently, namely, in the month of October, occurred that singular, isolated, and transitory manifestation of it on board the Dreadnought hospital-ship in the Thames, which has been so well described by Dr Budd and Mr Busk in the *Medico-Chirurgical Transactions* for 1838. After that time, it ceased to be heard of in any part of Europe.” It continued to exist, however, in India, the place where it originated, not producing such dreadful effects as occurred in 1817, although still very mortal, and usually appearing in the summer months.

Such is a brief history of the late remarkable epidemic of cholera. During its continuance it presented features peculiarly its own, never previously observed in similar disorders. For instance, it braced the opposing powers of every atmospheric condition,—the burning heat of a Bengal sun, as well as the cold of a Moscow winter. It manifested the same fury on the dry and calcareous plains of Persia, and the parched sands of Egypt, as it exhibited in the isles of the Indian Ocean, and the swampy deltas of the Ganges, Euphrates, Volga, and the Dneiper. It was unaffected by elevation of region, as it swept chains of mountains—the Gauts, the Caucasus, and the Himalayas—with the same violence as it did the low jungle and the morass. Geological formation, or terrestrial peculiarity, offered no obstruction to its progress, as it traversed with equal ease the sandy plains of the Yemen, the basaltic declivities of the Mauritius and Bourbon, the steppes of Tartary, and the banks of the Euphrates, the Tigris, and the Burampooter. Nor, lastly, have varieties in the human race, nor differences in human rank, had the least influence in its extension or diminution.

HISTORY OF THE PRESENT EPIDEMIC, WHICH SEEMS TO BE APPROACHING THIS COUNTRY.

Our accounts of the present epidemic are, up to the present time, very unsatisfactory. Cholera existed in Persia sporadically, with considerable severity in 1842 and 1843, and was heard of there at intervals during the following two years. In 1843, Dr Parkes

witnessed an epidemic of it in the Tenasserim provinces, a portion of Burmah ceded to the Company in 1826. In the spring of 1845 it prevailed with great violence along the banks of the Indus, and was very destructive in Affghanistan. It now traversed Persia from east to west, and spread northwards into Tartary, and southwards into the Pachalic of Bagdad. In September 1845, according to the *St Petersburg Gazette*, it passed from Herat to Samarcand, and in the following November entered Bochara. Another account states that the epidemic commenced in Mushed, in the north-east of Persia. We hear nothing of it during the winter of 1845-46; but in the following May it broke out with extreme severity in Teheran, carrying off 300 persons a-day for several weeks, and killing, on the whole, at least 20,000. According to the description given—"Those who were attacked, dropped suddenly down in a state of lethargy, and at the end of two or three hours expired, without any convulsions or vomiting, but from a complete stagnation of the blood, to which no remedies could restore the circulation." According to Dr Milroy, at the very time the pestilence was thus raging in Teheran, it burst forth with great fury at Kurrachee, near the mouth of the Indus. On the 14th of June, as the soldiers of the 86th Regiment were proceeding to church, a sudden and rapid storm threatened the building. At the same hour cholera appeared. Before midnight, nine soldiers were dead, and more began to be brought into hospital in such numbers, that it was difficult to make arrangements for their reception. On the following morning, the disease had spread through the town, and cut off fifty persons. For the next five days it raged with appalling fury; it then abated in its intensity, but continued to hover about the place for another week. Within less than a fortnight, 900 Europeans, including 815 fighting men, were swept away. Besides these, 600 native soldiers, and 7000 of the camp followers and inhabitants of the town, perished.

From Teheran the cholera seems to have spread in two different directions; one to the S. W., in the line of Ispahan, Shiraz, and Bagdad (which suffered most severely), and the other towards the N. W., in the line of Tauris or Tabreez. It reached the latter city about the end of September, where upwards of six thousand became its victims in the course of a few weeks. At the same time the disease existed at Reshid and other towns along the southern shores of the Caspian. In the middle, or towards the end of October, a few cases of cholera were observed at Salian and Lankeran, frontier trans-Caucasian towns of Russia, and about the same time at Khoi, Makan, and Bajasid, in Armenia. In the south, it had extended from Bassorah to Mousul and Diarbekir, on the Tigris, and in December reached Mecca, where it raged with great fury. Early in 1847, it appeared in the west of the Caucasus, and committed great ravages in the Russian army acting against the Circassians. By the middle of May it was at Teflis, and also at Astrakan, at the

mouth of the Volga, where it reached its greatest intensity about the end of July. In August it broke out at Batoum, on the eastern shore of the Black Sea, and soon afterwards at Erzeroum and Trebizonde, to the southward, reaching the last-named city about the 9th of September. Shortly after, it appeared at various towns on the Sea of Azoff, and near the mouth of the Don, subsequently spreading northwards towards the more inland provinces of Charcow and Kiev. On the last day of September it appeared at Moscow, and, about the same time, at Odessa and at Perecop, on the north-eastern shores of the Black Sea. At Moscow, a recent account says, that between the 22d November and the 6th December, 506 cases occurred, 229 of which had terminated fatally. The average number of cases, consequently, was 35 per day, and of deaths, 16. In the middle of October, we learn from official reports, that without counting Georgia, the Caucasus, and the country of the Cossacks of the Black Sea, the disease existed with greater or less severity in sixteen different governments of the Russian empire. It also broke out again in some parts of the north of Persia, as Tabreez, Khoi, &c., and in Bagdad. In the second week of November, the St Petersburg Gazette stated, that "the most western points the cholera has yet reached are the town of Alexandroff, in the government of Kherson, and the districts of Olgapol in Podolia," which is only thirty miles from the Austrian frontier. To the northward it had been travelling from Moscow to Novogorod, in the direction of the capital, and also in a course due west to Dwinaberg, a short distance from Riga, and within forty miles of the Prussian territory. A letter from Vienna of the 20th, announced that some cases had occurred in the circle of Tarnapol in Galicia. News have since arrived, that N.W. the disease has reached Petersburg, and S.E. Constantinople, and several other isolated instances have been observed at Cracow, Keil in Homberg, Paris, and even in this country. In London a few cases have already been noticed, and three well-marked cholera attacks have been observed in individuals at Leith, the port of Edinburgh.

Such, so far as we are enabled to trace it, has been the progress of the present epidemic. When we observe that it follows nearly the same N.W. passage as the great epidemic of 1817; that it is now not far distant from our shores; that in 1831 the cholera in this and other European countries was preceded by dysenteries and influenza; and then remember how common dysentery was last autumn, and how prevalent the influenza has lately been, the highest degree of probability exists, that this country will soon be visited by the dreaded disease. Under these circumstances it is satisfactory to know, that, although in some places in the east it has exhibited great fury, the mortality in western countries has been by no means so great as during its former visitation.

SYMPTOMS—MORBID ANATOMY AND PATHOLOGY OF CHOLERA.

The pathology, or essential nature of cholera, can only be determined by conjoining a knowledge of its symptoms and morbid anatomy, with investigations into the altered structure and chemical constitution of the blood. We do not consider it necessary to enter into any great detail on these subjects; it will be sufficient for our purpose to give only an outline of each, in order to render this article complete.

Symptoms.—The earliest symptoms of a case of cholera, says Dr Parkes, appear to be merely premonitory, and, after their occurrence, health may be regained without any subsequent symptoms. This is often seen during an epidemic, when many persons suffer from the precursory symptoms, to which the developed stage never succeeds. The premonitory symptoms are diarrhoea, usually of watery stools, colicky pains, trembling, dizziness, tinnitus aurium, perhaps syncope, slight deafness, a small quick pulse, and a cool pale tongue; in many cases there is some acceleration of the breathing, and a sensation of tightness across the lower part of the chest, and a burning sensation at the epigastrium. At other times they consist of violent vomiting and watery purging, arising suddenly, and attended by cramps and abdominal pains, and by a copious secretion of pale urine, with a warm skin and tongue, and perhaps even a rapid and tolerably full pulse. These cases are frequently considered as true cholera, although they may be cut short by treatment, a circumstance which has given rise to many mistakes concerning the efficacy of certain modes of treatment. No doubt they are highly dangerous, but only from their tendency to pass into true cholera. This, once established, invariably runs a certain course, no matter what its degree of severity may be.

According to the description of Giacomini, we may admit two degrees in the symptomatology.

In the more violent cases, the individual is seized, in the midst of the most perfect health, with slight indisposition. Then he at once loses all power, as if he had been violently beaten, or absorbed a violent poison. A mortal paleness and cold skin, giving to the touch the sensation of cadaveric humidity; feeble disordered pulse, about 100 in the minute, and afterwards imperceptible; livid eyelids, and sunken eyes; cold tongue, giving to the feel the sensation experienced on touching a serpent or frog on the back, while its surface is clean or slightly white; breath cold. In violent cases, the vomiting and diarrhoea are not in general manifested so promptly as in ordinary cases, or they are altogether absent, and death follows in some hours. It has been remarked that the first vomitings cause to be discharged the ingested matters taken some time previously, for instance, eight, ten, or twenty-four hours, which have undergone no change. The successive fits from the bowels present liquid serum, with flocculi of coagulated albumen, or of mucus. Spasms now occur in the fingers, limbs, and abdomen. There is an acute burning feeling felt in the precordial region, which produces an intense desire for cold water, notwithstanding that the tongue and skin are cold to the touch. Anxiety, weight on the chest, dyspnoea, sepulchral voice, and suppression of urine. These symptoms precede or accompany the lead, bluish, or reddish coloration of the face, fingers, and toes. The extremities contract and lose a third of their volume.

The course of the superficial veins is marked by striæ of an intense black colour. The blood, if it can be abstracted, is black, pitchy, does not separate into its elements, and presents oleaginous drops on its surface. The intelligence is perfect. Death is ordinarily preceded by a calm in the symptoms; the pulse, however, continues absent, together with the heat, and there are often convulsive movements. All this is accomplished in the space of from four to eight hours.

In the more common form, the disease generally appears with nausea, slight vomiting, and some liquid alvine discharges. In some cases, frequent stools precede for some days the other symptoms, without exciting great attention. There now appears a sensation of heat at the epigastrium, of constriction at the chest, of weight and insupportable oppression in the precordial region, with prostration of force, and a peculiar expression of the countenance; increase of the evacuations, accompanied by spasms in the legs, at first slight, then strong and insupportable, sometimes with immobility and paralysis of the limbs; progressive diminution of the pulse; cold; cyanosis; peculiar hoarseness; suppression of urine, of the saliva, and of the tears; extreme anxiety and inquietude, or apathy, with serenity of, or more frequently confusion or hesitation in, the ideas. Such are the phenomena which succeed the invasion. After some time, if death do not supervene, the pulse re-appears and rises, as well as the heat of skin; the spasms diminish, or are succeeded by hiccough. The patient has some moments sleep; a febrile condition, more or less marked, appears, with susurrus in the ears, and a slight pain in the head; the urine reappears, causing more or less heat, and is of a bilious tinge; the stools become sanguinolent or bilious; there is slight transpiration, with an exanthematic irruption, similar to that of urticaria, and the disease then terminates, leaving a general lassitude and weakness, which is more or less prolonged.—(*Annales*, pp. 328-29.)

Morbid Anatomy.—The appearances observed after death in those who die of cholera, have been very well described by Dr Parkes. The summary he gives is drawn from a comparison of 46 dissections in males. Of these, 39 were of persons dying in what has been called the blue or collapsed stage, and 7 of persons dying of the subsequent febrile affections. As these dissections were evidently made with great care, and constitute the latest account published of the morbid alterations in cholera, we shall collate the individual summaries he has given into one consistent whole.

The most usual appearances found in the head, are accumulation of blood in the veins of the dura and pia mater, and in the effusion of serum or of blood consequent upon this. This congestion is sometimes as great in recent cases as in those of consecutive fever with head symptoms. In one case, there was a collection of adhesive mucus in the laryngeal ventricles; in several, there was frothy mucus in the trachea, and slight redness in the same situation, general or confined to the bifurcation. In the majority of cases, the parts were unaltered.

The most common appearances in the lungs, were the presence of blood in the large vessels, chiefly or solely; the collapse of the parenchyma, and deficient crepitation, arising from the more or less complete absence of air and blood. In other cases, there was more blood in the capillaries, a corresponding dark colour of the lung, and an amount of frothy serum which bore an inverse ratio to the degree of collapse.

The right side of the heart, and the pulmonary arteries, were generally filled, and in some cases distended with blood. The left side and aorta were usually empty, or contained only a small quantity of dark blood.

In some cases, there were well-formed coagula in the heart, of more or less firm consistence; whilst, in others, the blood seemed wholly incoagulable. Even when coagula (so called polypi), reached into the pulmonary arteries and venæ

cavæ, the remainder of the blood was dark, viscid, and permanently fluid. Whether coagulated or not, the blood was usually of a dark colour, described by authors as "carbonaceous," "tarry," "unoxxygenized." When brought in contact with the air in thin layers, it generally acquired an arterial tint. Nitrate of potash, chloride of sodium, &c., added to the blood, communicated to it a bright arterial hue. The serum presented the appearance of a thickish dark fluid, which gradually, after the lapse of twenty-four or thirty hours, deposited a loose cloud of colouring matter. It contained abundance of albumen, and always coagulated firmly on the application of heat. It contained, also, some of the salts, as it never failed to give, with nitrate of silver, a dense curdy precipitate, becoming dark on exposure to light.

In the liver, blood was accumulated in the larger branches of the vena portæ and hepatic vein. The gall bladder was moderately full, and the bile thick and viscid, but not stringy, as in some cases of dysentery.

The spleen and kidneys presented nothing unusual.

The stomach was often distended with a watery, pale, subalbid, yellowish, greenish, or greyish fluid. This was not coagulable by heat, but gave, in several experiments, a dense curd of precipitate with nitrate of silver. The contained fluid often smelt strongly of medicines which had been taken even a considerable time before death. When less distended, the stomach was corrugated, and the mucous membrane was as rugose as the stomach of a dog.

The small intestines appeared generally dilated, and in every instance contained more or less of a peculiar fluid presently to be described. The agminated and solitary glands were enlarged in the majority of cases; this enlargement, however, differed exceedingly in degree. They were most conspicuous in the least severe, and in the prolonged cases, in which there is the greatest amount of purging. There was no ulceration or sloughing of these agminated glands in any case, and in two only was there ulceration of the solitary glands. In about half the number of cases the colon was contracted. The degree to which this was carried was sometimes extraordinary, but bore no relation to the amount of purging. The glands were visible in every case, sometimes presenting dark central spots or dots.

The fluid effused into the alimentary canal, was generally found in the small intestines, and was one of the most constant after death appearances. In colour it was white, subalbid, or of a light grey yellow, or chocolate tinge. The fluid consisted of a thinner and a thicker portion, and its consistence varied, according to the quantity of these component parts. The thicker portion was flaky, stringy, curdy, or clotted: it was not spread uniformly over the surface, but lay in masses here and there, as if it had been deposited from an agitated fluid: some of these masses, or bundles, often adhered with considerable firmness to the mucous membrane, and were detached with difficulty from between the valvulæ conniventes. It had a peculiar albuminous caseous smell, more developed by heat. In amount it varied greatly. In one case three pints were measured, but there was sometimes much more than this, and sometimes less. The thin serous fluid became cloudy and turbid by heat in some cases, but in the majority of instances it resembled the thin serous stools in being incoagulable. It precipitated nitrate of silver in every case. Occasionally there was an alkaline reaction: but this was not constant. When the clotted substance was separated from the thin fluid, and heated in a solution of liq. potassæ, or of carbonate of soda, it dissolved with greater or less facility. In ten trials the solution was almost or quite perfect; in a few other cases a portion was left undissolved. Nitric and sulphuric acids immediately gave a copious white precipitate when added to the alkaline solution. It was only occasionally soluble in acetic acid. There can be little doubt that the rice-water stools consist of the thinnest parts, which pass off, while the thicker clotted portion is retained, or appears in small quantities in shreds or flocculi.

It seems probable that the peculiar stools consist of part of the water and salts of the blood mixed with a considerable amount of fibrine, and it is im-

possible to avoid the conclusion, that they consist principally of the ingredients which have been found diminished in the blood.

The urinary bladder was found contracted in every case, sometimes to an excessive degree.—(*Parkes*, pp. 7—47.)

Pathology.—When, from the facts just detailed, we endeavour to deduce a sound opinion as to the essential nature of cholera, we at once fall into a crowd of difficulties, from which it is evident that other facts and more extended researches can alone extricate us. In the absence of any thing positive, therefore, we shall content ourselves with giving an analysis of the views of Giacomini, published in 1836, and of those in the recent work of Dr Parkes, from which it will appear that two independent observers, educated in two different schools, and with pathological ideas much opposed to each other, have arrived at pretty nearly the same conclusions, from a study of the same facts.

The Italian professor considers it necessary, in the first place, to separate such symptoms as are proper and essential to the disease, from those which are only accidental or relative to circumstances. The former are those which are never absent, and must be present in order to constitute cholera. The latter are those which, if they do not exist in any case, however severe, are considered to be secondary. Thus the symptoms which most attract the attention of the superficial observer, namely, the rice-water stools, is not an essential symptom; it may be said that their amount is an inverse ratio to the violence of the disease. In like manner, the abdominal pains, as well as the cramps and spasms, notwithstanding the torments they occasion, are not constant, and consequently not essential. It is an error to suppose that cholera is a spasmodic disease. Among the essential or characteristic symptoms, on the other hand, there are placed the sensation of heat, anxiety, and oppression at the precordial region; the involuntary sighs and slowness of the respiration; the irregularity, depression, and disappearance of the pulse; the cold of the external and internal surfaces, with the exception of the intestinal mucous membrane; the suppression, complete or incomplete, of the urine; the mortal paleness, followed by blue coloration of some parts or the whole of the body; the enlargement of the superficial veins; the rapid emaciation of the face and person, and lastly the asphyxia.

Dr Parkes also, after giving a general description of the symptoms, says that they may be considered as essential or dependent. The algid symptoms are essential, the abdominal symptoms dependent, because, although they are both produced by the same cause, and in this sense are both primary, yet one is manifested only during a particular condition of the other. The essential symptoms, according to him, are “the loss of animal heat; the loss of the voice, proceeding from the diminished volume of air in the lungs; the colour of the surface, and the arrest of the circulation, coupled with the collapsed state of the lungs after death; their defi-

ciency in blood (although blood may be in the large vessels, and frothy serum in the pulmonary texture and bronchial tubes); the contraction of the left cavities of the heart, with the empty arterial system, and the dilatation of the right cavities, with the distended venous system."—P. 105.

From these facts, both writers deduce the opinion that a stoppage of the circulation, commencing in the venous system, is the proximate cause of cholera. The cause of this stoppage is attributed by Giacomini to a universal phlebitis, by Dr Parkes to some change in the blood itself.

Although the term universal phlebitis appears to us, at first sight, wholly misapplied to the actual changes going on in the veins in cholera, it will be seen, on studying the explanation of Giacomini himself, that he really means a congestion of the vessels in question. Thus he says, by universal phlebitis we understand a primitive affection, situated in the ganglionic nerves, distributed to the walls of the veins. The affection so produced, is not manifested to the senses by material alterations of the tissue, nor by functional disorder of the nerves themselves, but rather by visible alterations, and altered functions in the organs to which they are distributed. Now, if, as he says has been shown in another work, that the vital property of veins is to distend and dilate their walls, it follows that, when inflamed by the introduction of a powerful miasm, their dilatation or diastole should become more active, more durable, and lastly, altogether permanent. This state of the venous system, according to him, explains all the essential phenomena of the disease; thus:—

The various changes produce immediate and rapid absorption of the fat, and other fluid or semifluid matters of the subcutaneous cellular tissue. Hence the oleaginous principle found in the blood removed; the rapid emaciation; the sudden dryness of the patient, who loses up to a third of the periphery of the tissues; the hollow peculiar visage and sunken eyes, which scarcely allow the patient to be recognisable. Pathologists know that morbid contractions and relaxations of the numerous muscles of the face, communicate in general to the physiognomy of those affected peculiar expressions of pain, anger, fright, delirium, the hippocratic countenance, &c. In cholera, however, there are none of these expressions, as muscular action has nothing to do with the peculiar physiognomy produced, which entirely depends on the drying up and poverty of the cellular tissue. In proportion as the veins inflame, and their dilatation tends to become permanent, they are filled without the power of emptying themselves into the right cavity of the heart. Hence the sensation of general prostration which suddenly strikes the person. The right heart and lungs, loaded with the excessive quantity of venous blood, produces that feeling of oppression and weight, and sometimes also the pain in the precordial region, the involuntary sighs and groans, which some authors have erroneously attributed to the abdominal region. This sensation of oppression and inquietude becomes more extreme, as the right ventricle of the heart makes fruitless efforts to receive only a little blood from the auricle and dilated vena cava. At the same time, as the left ventricle and arteries cannot receive a sufficient quantity of blood, the pulse becomes weakened, and gradually disappears; the surface of the body becomes pale, cold, contracted, exanguine, and icy; the kidneys are destitute of the material necessary for the secretion of urine, and the disease

rapidly marches towards syncope and asphyxia. Meanwhile the veins rest evidently in a permanent state of dilatation ; their volume is increased in the trunks near the skin ; for the cyanosis, the lead and blue coloration, depend on no other cause than the arrest of blood in their capillary extremities. This is further confirmed by the stoppage of the movements of the blood, which only escapes by drops on opening a vein, and by the alteration of this fluid, which is deliquescent and black, like melted pitch, qualities which it acquires in the veins themselves, or which are effected as the result of a natural crasis.—*Annales*, p. 351.

On the other hand, the vomiting and purging indicate to Professor Giacomini that the digestive system is the one least affected in cholera. In it the arterial circulation continues, and the functions proceed, although occasionally, whilst in the other organs they are altogether suspended. This he explains by the peculiar relation of the intestinal veins to the vena porta, into which they are readily emptied. The liver, on the other hand, being essentially a venous organ, ought to be greatly affected, which it is ; not secreting a drop of bile. In the same manner the veins of the brain permit the rapid passage of blood into the larger sinuses, whereby its tissue and functions are little altered.

Such, then, are the views of Professor Giacomini ; and if we put aside the term phlebitis, that is, the inflammation of the venous texture itself, and call the state he has described one of intense congestion of the veins, gradually producing arrestment of the whole circulation, such a condition seems to us tolerably consistent with the known facts observed in cholera.

Dr Parkes holds very nearly the same pathological views as Professor Giacomini, considering that there is some impediment or arrest of the circulation in the capillary system generally, and in the pulmonary capillaries in particular. He is of opinion, however, and we think with more reason, that the cause of this impediment is not to be sought in any primary change in the veins, but in a morbid condition of the blood. The nature of this morbid condition is not exactly known, but he thinks it due to a chemical change in the fibrine.

The fibrine, then, in cholera, seems to lose its solubility ; it appears in some cases to be completely got rid of in some unknown way. It may, and probably really does, form part, or the whole, of the thick pasty substance effused into the intestinal canal. But it is also possible that gradually, as it becomes insoluble, it may be deposited in the capillaries or in the small arteries, and either form there a mechanical impediment to the passage of blood, or by causing the blood to be thus loaded and physically altered, render it impossible for it to pass along the smaller vessels, or if it in part still passes through the capillaries, it may be incapable of undergoing some or all of the usual respiratory changes. That there is some impediment or arrest of the circulation in the capillary system generally, and in the pulmonary capillaries in particular, appears almost certain, and it is by no means improbable, from the whole bearing of the facts, that this is due to a chemical change in the fibrine, and in its mode of combination, consequent on the direct action of the active cause.—P. 113.

Again, he says,—

It might be supposed also, that if there were a deposition of fibrine in the

capillaries or minute arteries, the lungs would be always engorged, perhaps enlarged, and would universally present a more condensed or semi-hepatized appearance than could be accounted for by the mere condensation of the tissue and approximation of the molecular particles. In fact, they ought to present in *all* cases the appearances witnessed in some. I admit the force of this objection, but I do not think it conclusive in opposition to the strong reasons for believing in the deposition or physical alteration, in some way, of the fibrine. It is not to be supposed that, in a disease so rapid as cholera, there would be time for great accumulation of solid proteine compounds in the pulmonary capillaries; in fact, the hypothesis does not suppose the accumulation to be considerable, but merely to be sufficient to impede the circulation, and to hinder the respiratory changes. Besides, in the cases of extreme collapse of the lungs, which are generally the most rapid and fatal, it may be presumed that the alteration of the fibrine is almost simultaneous throughout the body, and then of course the lungs will not show a greater indication of this than other parts. Or, the obstruction may be regarded as partial, and more importance may be attributed to the incapacity of the blood, although still in some degree circulating, to enter into combination with the atmospheric air.—Pp. 121, 122.

Dr Parkes gives a good illustrative case, showing how death was only to be explained by arrestment of the circulation at the lungs. It was that of a strong English soldier, who died in eight hours and a half, in whom both lungs were found much collapsed an hour after death, whilst the contractility of the heart remained. Death did not take place from vomiting and purging, for these were trifling: he did not die from any paralysis of the heart, for this acted well during life, and contracted after death: he died because the blood did not pass through the lungs, as evidenced by the blood in the pulmonary arteries, and this was most probably owing to lesion of the blood.

Dr Parkes concludes his discussion of the pathology of cholera with the following passage:—

In favour of the view that the poison of cholera is inhaled into the lungs, and acts chemically on the blood in the pulmonary capillaries, it may be alleged that the first effects of the poison are often of the kind which would be produced by some impediment to free respiration; slight dyspnœa, tightness across the thorax, a feeling of anxiety from some lessening of aëration, an acceleration of the respiratory movements, are very usual symptoms. The necessity also for admitting that the active cause of cholera exists in the atmosphere, and moves with bodies of air, seems to accord with such a view. But whether this be not too mechanical and easy an explanation, and require assumptions almost as great as the hypothesis of nervous derangement, is a point for future discussion and determination.—P. 129.

From what has preceded, it will be seen that Professor Giacomini and Dr Parkes are so far of the same opinion, that they consider the essential phenomena of the disease to be dependent upon venous congestion, and arrest of the circulation. The cause of this in its turn the former considers to arise from permanent dilatation of the veins, caused by an alteration in the ganglionic nervous system; whilst the latter attributes it to some morbid change in the blood, produced by a miasm floating in the air.

Here we leave our authors for the present, purposing to enter upon the subjects of the mode of transmission and treatment of cholera in our next Number.

On Poisons, in relation to Medical Jurisprudence and Medicine. By
ALFRED S. TAYLOR, F.R.S. 12mo. London: 1848.

THE merited approbation with which Mr Taylor's concise manual of medical jurisprudence has been received by the profession, might have been sufficient apology for any further contribution on his part to legal medicine. The chief reason given, however, for the publication of the present work, is the late fearful increase of the crime of poisoning, and the consequent need of a convenient book of reference for barristers and medical practitioners, in order to further the effective and safe administration of justice. That the crime of poisoning has been more frequent of late years is undoubted. It appears that, in 1837 and 1838, the average number of cases of poisoning in England for each of these years was about 270; whilst in 1840 it was 349. We regret that, when treating of the statistics of poisoning, Mr Taylor does not offer some more decided opinion on the means to be taken to prevent the increase of this crime, his only remarks on this being a casual observation in the chapter on arsenic. The recorded opinions of the author of a systematic standard work, would have more weight than a score of the best anonymous articles that ever appeared in the columns of a newspaper. In our opinion, the first party in the state who have a duty to do in aiding the suppression of the crime of poisoning, is the legislature. From them we have a right to expect speedily some measure restricting the sale of poisons. Whether this should constitute an enactment *per se*, or form part of a bill for the regulation of the education and practice of chemists and druggists, it is foreign to our present purpose to discuss. We cannot, however, see the difficulty of making it penal to vend certain drugs without the signature of a medical practitioner, or a testimonial to the character of the applicant from two householders. If this or some such plan were adopted, the facilities for tracing out the poisoner would be so much increased, that we should have a check put to the daily suicides and murders perpetrated by means of poison got upon the stale plea of rats in the house; and, moreover, it would deal a strong left-handed blow at such pernicious habits as opium-eating in adults, and drugging with Godfrey's cordial in children.

But there is another party by whom something might be done to prevent the increase of the crime of poisoning,—we mean medical writers on toxicology. Not so much in the way of increasing our knowledge of the means of detecting poison, as in curbing that spirit of chemical refinement, in respect to sources of fallacy, which so much characterises the works of most modern writers on poisons. The French have been the most flagrant offenders in this respect, and Mr Taylor himself is very far from being sinless.

The following general observations on fallacies, we extract for
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the sake of the important remark contained in the latter sentences :—

One of the most serious difficulties to an analyst, is that which arises from the fallacies to which the application of the best tests is exposed. I do not now refer to the presence of impurities in the tests employed, but to the important fact that many substances, poisonous and not poisonous, yield the same or similar results with the same test. Hence, if one test or one process be relied on as the basis of chemical evidence, the medical witness would fall into a dangerous mistake. We are perhaps hardly yet acquainted with all the fallacies to which *individual* tests are exposed ;—the extension of chemical science is daily adding to their number, by bringing out an analogy of properties where it could not have been suspected to exist. It is thus that selenic or fluosilicic acid may be mistaken for the sulphuric—the racemic or paratartaric for the oxalic acid—cadmium or antimony for arsenic—uranium for copper—bismuth for lead, with many other examples of the like nature. The usual means of avoiding a difficulty of this kind is to employ *several* tests or processes, and never to rely upon the action of *one*. It must be remembered that if the poison be really present, not a single test ought to fail in its reaction except from circumstances which it would be easy to understand and explain. Under each poison the objections hitherto known as applicable to the tests will be stated, and the means of avoiding fallacy from their use, explained.—P. 141.

The objections, on the score of sources of fallacy, to chemical tests as evidence of poisoning, ought undoubtedly always to be most carefully considered ; but the extent to which this is now carried in many instance is ludicrous. It seems to be the fashion to ransack the archives of chemistry, and when a substance can be found presenting a reaction common to it with the poison, to set it down as a fallacy, without considering the probability, or, in many cases, the possibility, of its really being in the way. When this is found laid down in a work on medical jurisprudence, it is at once pounced upon by lawyers, who, in profound ignorance of the real value of the so-called objection, bring it forth in cross-examination, —sometimes with the effect of puzzling a medical witness, unexceptionable as regards his evidence on all material points, but who, not being *ex professo* an analytical chemist, may not have all these out-of-the-way reactions at his fingers' ends,—sometimes unsettling the minds of an ignorant jury, by a pompous display of sources of fallacy which have no real existence, and thus indirectly encouraging the cold-blooded poisoner, who, in his calculations of the chances of escape, (and what poisoner does not make such calculations?) sees these avenues opened to him by the very hands that ought to shut them up against him. Orfila's now extinct normal arsenic in bones, and Sir Fitzroy Kelly's "apple pip" defence of Tawell, are genuine specimens of the spawn of this medico-legal refinement. To Dr Christison's work on poisons we must assign the merit of being free from this fault. We defy any one who follows his instructions correctly, to lead to the condemnation of an innocent person by erroneous chemical evidence as to the presence of arsenic in a suspected case ; and yet he gives us no long detail as to cadmium and uranium as sources of fallacy in arsenical cases. Mr Taylor, on the contrary, gives us nearly a quarter of a page about cadmium under

the head of the reduction process. It is true that he winds up by saying, that "oxide of cadmium is very rare,—it is difficult to meet with;" but in a short while he recurs to it again, and gives us half a page about it under the sulphuretted hydrogen test. All this is worse than useless. Mr Taylor might have settled the whole cadmium question in a few lines by simply saying, that cadmium was no real source of objection; because, although it offered two reactions which have some resemblance to the corresponding reactions of arsenic, yet that it wanted all the other, and these the most distinctive, characters of this familiar poison. In fact, the very rarity of cadmium might set it aside without cavil. There is only one source from which it could come into a stomach or its contents; viz. if sulphate of zinc had been used as an emetic. Now the impure oxide of zinc, in which Strohmeyer discovered cadmium, contained only three per cent. of this metal. Of the oxide of zinc, the sulphate contains at most only 33 per cent.; consequently, had a patient taken two drachms of sulphate of zinc as an emetic (a full dose certainly), and had all the cadmium which it might have contained remained in the stomach in spite of the vomiting (a most unlikely thing assuredly), and had all the cadmium been successfully extracted in an analysis, it would amount only to a single grain, and he would be a curious witness who could get from this a series of reactions which could be mistaken for arsenic.

Again we have the reactions of persalts of uranium put forth as sources of fallacy in the detection of copper. This might be all very well if pitchblende, the common source of uranium, might reasonably be expected to find its way into any article likely to form the subject of a medico-legal analysis. But being, as it is, a mere mineral curiosity in the cabinets of collectors, Mr Taylor's notice of it is little else than a piece of medico-legal foppery. It is from the absence of these wire-drawn niceties that we value so highly Dr Christison's work. Its plainness, perspicuity, and essentially practical character, make "Christison on Poisons" the work on toxicology to which we would trust in a puzzling case rather than to any other book with which we are acquainted.

Let it not be supposed, however, that we undervalue Mr Taylor's book. We think it an excellent and valuable manual. We predict for it a very favourable reception by the profession. There is a great mass of solid information, and many valuable observations of his own, contained in its pages; but it

" * * * Hath bubbles, as the water hath,
And these are of them."

Mr Taylor has copiously illustrated his various statements by reference to actual cases. Most of these are highly valuable, though some of them are detailed with uncalled-for minuteness, and unnecessarily add to the bulk of the volume. It is, however, so compact and reasonable in price, that we must not complain of this.

We should like to offer our readers a survey of the contents of

Mr Taylor's work; but a treatise on poisons is of necessity so full of individualities and special details, that we cannot do this within the limits of an ordinary review. We must refer our readers, therefore, to the work itself for much useful and valuable information on individual poisons, which our limits forbid us to notice.

The first part of the work, which treats, in fourteen chapters, of poisoning generally, will repay an attentive perusal.

The introductory chapter is chiefly occupied with defining what is a poison. He points out the looseness of the definitions which have commonly been given of the term poison, and offers the following as the most comprehensive:—

A poison is a substance which, when taken internally, is capable of destroying life without acting mechanically on the system.

He would suggest to Mr Taylor, that, without essentially altering his meaning, he might express this a little better. If he were to say that a poison is a substance which, when *introduced into the system*, is capable of destroying life without acting mechanically, he would obviate the objection which he himself raises to his own definition in the succeeding lines, when he says—

Some substances may, however, act as poisons by absorption when applied to the skin, or a wounded surface; while others, again, as the poison of the viper and of hydrophobia, may have their fatal effects limited to those cases in which they are introduced by a wound.

The subsequent chapters are devoted to the physiological investigation of the *modus operandi* of poisons, their classification, the general and special evidence and diagnosis of poisoning. Then follow two chapters on the treatment of poisoning, not of a very encouraging character, for he holds most antidotes very cheaply; and then a chapter on the rules to be observed in investigating a case of poisoning, which, from its plain practical style, we recommend to the attentive perusal of all young practitioners. There is no time for studying rules amid the hurry, bustle, and excited feeling attending a suspected case of poisoning; and a want of attention to the due collection and preservation of matters which may afterwards be the subject of analysis, may vitiate the whole of the subsequent proceedings. The rules for conducting analyses and making reports, given in the subsequent chapters, are excellent. We extract the following judicious remarks on the

Danger of premature opinions.—During the examination of a suspected substance, a practitioner is often pressed to give an opinion respecting its nature before the steps of the process are complete. This may arise from the anxiety or curiosity of those who are interested in the proceedings. There is a rule, however, which it appears to me should always be followed on these occasions; namely, that no opinion whatever should be expressed until the *whole* of the analysis is complete. It often happens in the hands of the ablest analyst, that the last steps of a process lead to a result very different from that which was anticipated at the commencement. The truth is, it is not by one character, but by many, that a poison is identified; and, therefore, a suspicion derived from a few incipient experiments, is very likely to be overthrown by continuing the investigation. In the *Boughton case*, Dr Rattray gave an opinion in the

first instance, that the poison administered to the deceased was arsenic ; but he subsequently attributed death to laurel-water. A case occurred, within my knowledge, where arsenic was pronounced to be present when sulphuric acid was really the poison. In another case, tried at the Kingston Assizes in 1832, the medical witness admitted that, at the coroner's inquest, he stated the poison to be arsenic, but by subsequent experiments he found that it was oxalic acid. And in a case which has but recently occurred (February, 1845), the poison was at first stated to be oxalic acid, but on a more careful examination, it was shown to be arsenic ! Coroners are not sufficiently careful in selecting persons to conduct analyses of this kind ; hence it is by no means surprising that such mistakes should be frequently made.

This mistake respecting the nature of a poison not merely impedes the course of justice, by throwing a doubt upon evidence which ought to be, beyond all question, clear and satisfactory ; but it seriously affects the reputation of a witness. It entirely arises from his giving an opinion before he is justified by the facts in so doing. It is, I think, a well-marked line of duty to be pursued on these occasions :—1. That no opinion should be formed from a *few* experiments : and 2. That no opinion should be expressed until the analysis is *completed*. It is obvious that if a man be compelled to admit, in cross-examination at a trial for poisoning, that he has once been mistaken on a question so important, and requiring so decided an answer, a jury may be easily induced to believe that the witness may have made a second mistake, and that his then positive opinion is of no more value than that which he first expressed, and afterwards retracted.—P. 139.

Want of space prevents us from enlarging our observations on the general part of Mr Taylor's work. Before leaving it, however, we would impress upon our readers, more decidedly than our previous remarks would seem to convey, our high opinion of this portion of it. We have pointed out a few faults ; its many excellencies will be learned from a perusal of these first fourteen chapters. We particularly recommend them to the study of our younger professional brethren. The special details of individual poisons are not half so important a subject of study as the general principles of toxicology. In conducting an analysis, recourse can always be had to some standard book as a guide to the *minutiae* of the processes to be followed. But in treating a case of poisoning whilst living, or in making the examination *post-mortem*, and forming an opinion as to the real cause of death, which is often required to be done where the individual poison may be unknown, or where it may not be known that there is poisoning in the case at all, the man who is armed with a good general knowledge of the subject will be little likely to commit serious errors, although he may be too little of a chemist to make a nice analysis without "Christison on Poisons" or Mr Taylor's work at his elbow.

We do not propose following Mr Taylor into the details connected with individual poisons ; but we may briefly inform our readers of the manner in which the individual poisons are treated by Mr Taylor. We take the chapter on arsenic as an example.

He first gives a general account of the nature and properties of arsenic—its taste—its solubility (a subject recently examined ably by M. Bussy), and the weights of familiar measures of the poison in powder, a useful guide in estimating the quantity which may

have been swallowed. Then the symptoms, and the varieties in this respect, which occur in ordinary and anomalous cases, which he has fully illustrated by reference to individual examples. Then the *post-mortem* appearances, and the period required for the poison to produce particular pathological conditions. Then the quantity required to destroy life, and the period at which death takes place; and next, the treatment. A fresh chapter is devoted to the chemical analysis, in which all those minute objections to the tests are given upon which we have already commented, along with many valuable remarks on more really practical sources of fallacy; and, lastly, some accounts of the various arsenical preparations which may, by accident or design, give rise to poisoning by it.

Our readers will perceive that they will find abundance of detail in Mr Taylor's work. It contains all that kind of information which a medical man will be glad to have access to, when he has the prospect of appearing in the witness box; and on this account, we doubt not, it will speedily find a place on the book-shelves of most medical libraries.

Speech of the Count de Montalembert in the House of Peers of France, the 5th June 1847, on the subject of Medical Reform. With Notes of the Translator. London: 1847.

THE translator of the above speech has been induced to lay it before the public of this country from the conviction, that it is one of the very few publications upon medical reform which contain "a plain and unflinching statement of the true principles which ought to form the basis of all medical legislation." As we concur with him in his very high estimate of this remarkable document, we seize the opportunity of introducing it to the notice of our readers.

The occasion of the speech was the introduction of a bill into the French House of Peers, with a view to organize the medical profession of France, by forming it into an exclusive and privileged class, placing it, at the same time, under the immediate direction and control of the government, and restraining, by the severest penalties, every description of unlicensed practice. The bill itself is little known in this country to the general medical public; but it is described as being an exceedingly long and complex document, and a remarkable instance of the meddlesome legislation which has recently prevailed so much among French governments.

It is against this excessive centralization that the attack of the Count de Montalembert is primarily directed. He is opposed to all "excessive, corrupt, useless, minute, and vexatious" exertion of power. He thinks it equally dangerous to the interests of the public and of the government, that the latter should stretch out its controlling arm over the scientific institutions of the kingdom, and assume the responsibility of deciding for the public in the choice of medical attendants, or of limiting the scientific bodies in the selection

of associates. He deprecates the attempt to form the medical profession into a gigantic corporation, more or less directly in the service of the state; and argues with great force against the idea, that the attendance on the sick ought to be viewed as a public function.

My Lords, I believe it is no such thing. Medicine is what our forefathers, in their simple and truthful language, called it—a liberal profession. There is no question here either of priesthood or magistracy, or other state function. It is a liberal art, freely exercised and freely accepted by ourselves and our families. I think, my Lords, you will admit with me the necessity of re-establishing the real meaning of the word, and of destroying an illusion which would betray us. The physician's office is in the bosom of our families; he has nothing to do with the State.

It may, indeed, be well that physicians should be selected to fill certain offices, and it may also be consistent with the well-being of the State and the dignity of the medical profession, that the Government should delegate a portion of its authority and power to a physician-inspector, or the physician of an hospital. But that a physician engaged in private practice should be viewed in the light of a public functionary, a magistrate, or a kind of priest, is quite impossible, without a pernicious change in the very nature of this useful profession. Yet this, my Lords, is one of the pretensions of the Bill before the House. In its preamble we find the formal declaration, that medicine is a priesthood, and physicians are officers of State.—Pp. 10-11.

But while the Count de Montalembert thus deprecates the far-stretching aims of the Minister of Public Instruction, he is not less opposed to the narrow views of certain medical men, who conceive the main object of medical legislation to be the protection of the profession against the competition of unlicensed practitioners; or of others who, in a still narrower spirit, maintain the interests of the profession, as a whole, to be so bound up with those of certain corporations, that medical legislation must be addressed principally to the wants of these corporations, and the increase of their power and influence. He contends for the existence of title and grades in the profession, and thinks that these should be granted after severe examinations; but maintains that “these gradations should be an honour, a recommendation, a guarantee of capacity, but neither an arm of offence nor a means of oppression against those who are unable or unwilling to take so distinguished a title.”

It seems to me that there is a very strict analogy to be established between the titular grade, which is the sign of the amount of science and erudition attained, and the manufacturers' mark, which is now demanded to indicate the purity, solidity, and genuine quality of our manufactured products. All men would reject with indignation the idea of compelling our manufacturers to use such marks. In like manner, I cannot comprehend in science any other system. Let physicians and surgeons have their titles placed under the protection of the laws, and severely forbidden to those who would usurp them. Nothing can be more proper than this. But to compel all those who would offer advice in matters of health to assume such titles, is a most unnecessary encroachment on individual liberty, and a coarse limitation of the freedom of the human mind.

As to penalties, I not only admit them, I claim them,—but in the spirit of our laws. I ask even severe penalties,—but against what? Only against the illegal assumption of titles. Should any one offer advice and assume a name which the law has not conferred on him, let him be severely and surely

punished. Nothing is more natural, nothing more just than this. But that the simplest and most easily understood offices of a physician or surgeon, when practised by either men or women, without imprudence or ignorance, should be punished as misdemeanour or felony by severe enactments, this is what I do not understand, and shall never admit to be either reasonable or just.

I do not, then, allow any other penalties, than such as are applicable to the usurpation of a professional title, or to such abuse of a professional position as would argue imprudence, unskilfulness, and immorality on the part of a physician.—P. 13.

After some instances, not the most happy that might have been found, of the evils of public prosecutions of unlicensed practitioners, he proceeds :—

I shall probably be told that I would protect and give impunity to quacks. My reply is, that there are quacks every where ; and, in proof of it, I cite the opinion of those who are most interested in favour of the proposed law, and most hostile to me—I mean the published opinion of the permanent Committee of the Medical Congress, of which so much has been said. This permanent Committee, in the observations it has addressed to us, declares that the titled and legal quacks inflict the deepest and most painful injury on the medical profession. This, my Lord, is the opinion of the physicians themselves—of those who, under an allegation of quackery, have asked for and obtained such severe penalties against the unlicensed practice of physic.

* * * * *

But I confide in the good common-sense of the nation. I wish to see public opinion less cribbed and confined. I believe it to be as good and efficacious a safeguard as the authority and power of a government. But I perceive with great pain, that, in the present tendency of affairs among us, the whole human race is looked upon as unfit to manage its smallest and most customary interests, while the little and pitiful minority of appointed agents of the Government are assumed to be infallible. It seems to be a settled maxim that all men, except public functionaries, are to be treated either as children or enemies, whose normal condition is either a state of pupillage or a state of suspicion.

I know not where we are to stop in such a career, for there is no profession, no occupation, in which there are not to be found men able and very willing to take advantage of public credulity. The Government, consequently, will never be without excuse for the intrusion of its power, under the pretext of saving the public—substituting its own meddling, and oftentimes bungling, for the safe and wholesome direction of public opinion. I cannot, for instance, understand why the business of a banker, which concerns so much the comfort and well-being of families, should not be submitted to the same regulations and restrictions as that of a physician.—Pp. 15-16.

We have the greatest pleasure in giving currency to these excellent and nervously-expressed sentiments, at a time when the clamour for measures to prohibit unlicensed practice seems to indicate, in some sections of the profession in this country, an ignorance of the principles of medical legislation, and a blind *esprit de corps* quite as striking as that which is deprecated by the speaker as existing in France. Indeed, the attempt in this country to revive and put in force laws which had, by almost universal consent, fallen into disuse, is more disgraceful to the section of the profession from which it emanates, than any resistance to the repeal of these laws in a country which has not had the benefit of our experience as to their uselessness and hardship.

On the subject of medical education, the Count de Montalembert

advocates the views of M. de Talleyrand, promulgated in the name of the Commission of Public Instruction in 1791. He holds that there ought to be perfect liberty of teaching, the student being entirely unrestricted as to his mode of acquiring knowledge, and only examined as to the result. That all the restrictions which operate for the sole benefit of individual teachers, should, in justice to the public, be swept away, we are most ready to admit. At the same time, it seems to us doubtful whether a system of examinations could, under the most perfect management, be made so practical and efficient as to do away with the necessity for all regulations as to the time of study and the fitness of teachers for their office. There seems, moreover, to be no serious objection to this preliminary test, if it be so contrived as not to serve the purposes of individuals and corporations instead of that of the public, and if it be not made to bolster up an imperfect and limited system of examination, as is too often the case with us. On this subject there are some excellent remarks subjoined by the translator, with some of which we conclude our notice of this pamphlet.

Though the period of probation be in itself an excellent thing, the examination previous to the license is the all-important step to the community. The more searching and minute it is, the more varied and frequent, the stronger the guarantee to the public. But it is not for the advantage of the public, and is unjust to the pupil, to make his degree or diploma depend on the issue of one examination, in which chance decides much for or against him. A series of examinations, on the contrary, conducted with sufficient minuteness, and without unnecessary rigour, extending over the whole period of his studies, would be a most beneficial measure.

Our practice is the reverse of all this. Let any one examine the present state and past history of the laws and regulations, *statuta solemnia*, &c. &c., of our universities, colleges, and corporations, and say whether there is not a more eager desire to benefit the lecturer than to improve the pupil. There has been, indeed, a great extension given to the curriculum of study. This is an unquestionable benefit to the teacher, whatever it may be to the public; but, as far as I can learn, the examination itself is little, if at all, altered. It would be no difficult matter to give a very painful, yet a plausible explanation of all this. I do not believe, however, that there has been any design in it; but it is easy to perceive that improvements become much more easy when they concur with men's interests, and that defects in the examination are met by severe exactions in the curriculum, which are often not beneficial, but a tax on the time, the patience, and the pocket of the pupil. The truth is, that were either the business of teaching, or the labour of examining what they ought to be, men, absorbed by the daily duties, and crushed by the gnawing cares of practice, could not undertake them.—Pp. 30-31.

Elements of General and Pathological Anatomy, presenting a View of the Present State of our Knowledge in these Branches of Science.
By DAVID CRAIGIE, M.D., F.R.S.E., &c. Second Edition, Enlarged, Revised, and Improved. Edinburgh: 1848. Pp. 1072.

It has too often been our unpleasant duty to point out the fact, that works which, some years ago, were distinguished as giving an

excellent account of the then state of knowledge, become almost useless in subsequent editions, from their authors not having kept pace with the progress of science. We regret to say that such seems to be the character of the work before us. During the twenty years which have elapsed since the first edition was published, no branches of science have made such rapid advances as those of general and pathological anatomy. Indeed, the doctrine of growth by cells may be said to have completely revolutionized them. Yet to all this Dr Craigie seems to have paid little attention; so that, in point of fact, this ponderous volume represents general and pathological anatomy as it existed ten or twelve years ago at the latest, and not "the present state of our knowledge in these branches," as is set forth on the title-page. True, some extracts are inserted from modern writers on a few subjects, which, however, only serve to show the extraordinary confusion which must result from an attempt to mingle together the past and the existing doctrines of pathology. It would be amusing enough to follow the author in his quotations from the books of the last and commencement of the present century, with the contents of which no one can be more familiar, and then to contrast him hesitating, floundering, and gravely talking about corpuscles, nuclei, and cells, of which he knows no more than did Willis, Vieussens, Morgagni, or the rest of his favourite writers. We leave to others, however, what would be to us a very painful task, while we simply state what the interests of medical literature demand from us; namely, that although the modern doctrine of pathology is not even approached in this book, as a specimen of learning, and as a complete epitome of its past history, it may be considered unrivalled.

An Essay on the Use and Abuse of Restraint in the Management of the Insane. By HAMILTON LABATT, A.B., T.C.D., &c. Dublin: 1847. 8vo. Pp. 76.

THIS is one of the rejected essays, which were submitted in competition for the "Sugden prize" of last year. The author appears to have disqualified himself for competing, by divulging his name to one of the judges previous to the adjudication of the prize. This circumstance, together, we presume, with the persuasion on the part of the author and his friends, that his essay would otherwise have obtained the prize, have induced him to publish this essay, although advocating principles diametrically opposed to those defended by Dr Macadam, the successful competitor. Dr Macadam's essay has not been published; we believe that, with a laudable modesty, he will not even allow extracts of it to appear in any review,—it remains at the College of Surgeons at Dublin, for the exclusive benefit of the fellows. We understand that he advocates all kinds of restraint, "moral," "legal," and

physical; and of physical almost all degrees, from simple separation from friends to the strait waistcoat, and "fetters and manacles;" and not only as means of preservation and cure, but as *penal* measures!

Mr Labatt, on the other hand, equally scorning a middle course, wages war with all bodily restraint. Neither he, however, nor Dr Macadam, enter into the real points at issue between the advocates of restraint and non-restraint. We do not therefore propose to follow where they do not lead. The questions they discuss have been long since settled by every intelligent physician. The questions they have not discussed we must defer entering upon until a future opportunity. We would merely remark that, with the admission incidentally made by Mr Labatt, that personal restraint may be admitted in some cases to facilitate the treatment, few will be found who will combat with the rest of his essay; while still fewer will be found at the present day, who will take the trouble to refute the wholesale restraint practice of Dr Macadam.

Outlines of Physiology for the use of Students. Part I. By ALLEN THOMSON, M.D., Professor of the Institutes of Medicine in the University of Edinburgh. Edinburgh. Small 8vo. Pp. 180.

WE seize the earliest opportunity of strongly recommending to every medical student the excellent little work of Dr Allen Thomson. So much has lately been published on physiology in this and foreign countries, that no small degree of perplexity is often experienced by the beginner, in selecting the work from which his elementary notions are to be derived. And yet we know nothing of more essential importance to the mental culture, future career, and after success of the medical practitioner, than that first step which is to initiate him into an acquaintance with the functions of the animal, and especially of the human body. To every student, then, this work will be acceptable—containing, as it does, a clear exposition of all that is really known, arranged in a methodical and often original manner, together with a list at the end of each subject of those books which the physiological student should more particularly consult. We need not say that those who attend the author's lectures will find it a valuable text-book, which, when interleaved, may be extended by notes.

The contents of such a work do not admit of analysis. One portion of it, however, has attracted our attention especially, because it has not been attended to in other English works on the subject; namely, "Employment of the Microscope in Physiological Research." The following excellent passage is an example of the author's style:—

Nearly all the structural elements of the textures are of microscopic size; and some are so minute as to have eluded search, until they had been observed

by the best instruments : the phenomena of the passage of the blood through the minute vessels which intervene between the extreme arteries and veins, cannot in any animal be seen with the unassisted eye ; the existence of the blood and lymph corpuscles, and the distribution of the small bloodvessels through the various textures and organs, require also the use of microscopes ; the circulation of fluids in the close cells, proper and sap vessels, and other parts of plants ; the minute changes which occur in the original formation of cells, in their conversion into the structural elements of the textures ; the continued growth of the laminar and other textures ; the morphological phenomena of secretion as they occur in the cellular elements of the glands ; the whole modern doctrine of the analogy and differences in the organization of vegetable and animal bodies ; the development of the ovum ; the existence of the spermatic filaments, their motions, and the phenomena of fecundation ; the existence of cilia, and the phenomena of ciliary motion ; the minute structure of muscular fibre, and the changes which it undergoes during contraction ; the relation between the tubular and cellular elements of the nervous texture ; the existence of infusorial animalcules, and the structure and vital phenomena of most of the inferior members of the animal and vegetable kingdom ;—these are a series of facts and views due to microscopic research, which could not have been ascertained without it, and which all now acknowledge to be essential to the scientific character, practical utility, and further progress of Physiology. As the microscope may also be made a most important assistant in the investigation of disease, the systematic study of the anatomical and physiological information which it affords, is imperative on all those who desire to attain to professional excellence.—Pp. 83, 84.

The London and Provincial Medical Directory. London : 1848.
Small 8vo. Pp. 430.

WE can highly recommend this work, which deserves the patronage of the profession. Its new features for the present year are—1st, A medical diary, which cannot but be of the greatest service to the practitioner ; 2d, The local lists, wherein, by reference to any town or district of England and Wales, the names of all the practitioners resident in that locality will at once be found ; 3d, A list of the poor-law medical staff.

We do not know whether the names of lecturers in the different schools of London and Dublin be correct or not. If, however, they are as carelessly inserted as those belonging to the Edinburgh school of medicine, this portion of the work is only so much waste paper. The editors have copied them from a list about ten years old, and, consequently, more than half the names mentioned are not to be found among those at present teaching, whilst the names of those who do, are for the same reason not to be discovered. Considering that a correct account can always be obtained from the students' numbers of the weekly journals, such errors are scarcely excusable.

Part Third.

MEDICAL NEWS.

LETTER FROM PROFESSOR GREGORY ON LIEBIG'S CHEMISTRY OF FOOD.

To the Editor of the Monthly Journal of Medical Science.

SIR,—I have just now seen your notice, in the September Number, of Liebig's Chemistry of Food, and, as translator and editor of that work, I beg to point out one or two inaccuracies in that notice. I do not mean to complain of the tone of the review, although somewhat unfriendly, because you are entitled to your own opinion; but I cannot think you would knowingly mis-state facts, with a view to create an unfavourable impression. I conclude, therefore, that the erroneous statements I refer to are the result of haste or of imperfect information.

First, You blame Liebig for not mentioning Heintz's name in reference to the presence of kreatine in urine. But I can assure you that Liebig's experiments were finished before Heintz had published any thing on the subject, and that Liebig's work was printed and in my hands several months before it was published in Germany. I do not mean to doubt in any way the originality of Heintz's experiments; but I am sure that both he and Liebig published in ignorance each of what the other had done.

Secondly, You say that Liebig ought rather to have published his work as a contribution to a scientific journal than in its present form. Now, if you had seen the *Annalen der Chemie und Pharmacie* for June last, you would have known that the German work appeared in that Number. In fact, I translated the work chiefly from the proofs of that Number of the *Annalen*, which were printed several months before the time of publication. The English edition is therefore a verbatim translation of an article in a journal, such as you recommend, which, however, is too voluminous to have been translated as an article in any English journal, and was therefore printed separately. I am not aware that any separate German edition has appeared.

Thirdly, You state that Liebig attaches, in the latter part of his work, an exaggerated importance to the function of kreatine and kreatinine as constituents of muscle, while, in your opinion, they are in all probability products of decomposition destined for excretion by the kidneys.

Now, I am not aware of the existence of any passage in the work in which any importance, exaggerated or otherwise, is attached to the function of those bodies, simply because, in Liebig's opinion, that function is yet unknown. Nay more, in the only passage in which the subject is alluded to, it is expressly stated that, if they perform any function in the system, it can only be in virtue of that portion which is not expelled in the urine. It is obvious to Liebig, as to all, that these bodies must be products of the change of the substance of the muscles or other parts of the body, or of the analogous matters in the food; but even supposing we could at present trace their precise origin, which we cannot, this would not prove that they have no function to perform as ingredients, and constant ingredients, of the juice of flesh, at least (as Liebig has pointed out) in so far as they are not expelled in the urine. All this is matter for investigation; but surely your account of Liebig's views on this point is not accurate, nor, as it stands, just to him.

It is certain that kreatine and the other bodies described by Liebig are important, because a knowledge of them is a step in advance in the investigation of the great series of changes which the food undergoes, from the time that it enters the body till it is expelled in the form of products of decomposition of effete tissues; but I cannot find any trace of any other or undue importance ascribed to them in Liebig's work.

Fourthly, You state that, according to Liebig, in the putrefaction of urine, kreatine disappears, being converted into kreatinine. Now, Liebig does not state this. He merely says that fresh urine contains both compounds, but that in putrid urine kreatinine alone is found. Of course, the kreatine has been destroyed by the putrefaction, but there is no proof that it has been converted into kreatinine.

Fifthly, In speaking of the controversy between Liebig and Mulder, you state that Mulder's results were over and over again confirmed in Liebig's laboratory.

This is a mistake. Mulder's results were accepted by Liebig, as well as by Berzelius and others, from a conviction of his accuracy.

But up to the present time no one of all those who, in Liebig's laboratory, endeavoured to repeat Mulder's experiments, has succeeded in obtaining that which forms the very foundation stone of his theory, *proteine free from sulphur*. It was the numerous failures to obtain this substance, coupled with the observation, that in all proteine compounds much more sulphur exists than Mulder had found, and that this sulphur exists in two forms, which led to the rejection of the proteine theory. For myself, I have only to confess that I adopted Mulder's views, as did Berzelius and Liebig, from confidence in his accuracy; but that, since the question has been agitated, I have in vain tried to procure, by the processes given by Mulder, a proteine free from sulphur, and that my experience on this point, as well as on that of the amount of sulphur present in albuminous substances (and, I may add, on that of the amount of nitrogen in them, which is also greater than Mulder gave out), agrees with that of Liebig and his pupils, Laskowski, Verdeil, Rüling, and others.

I can no longer, therefore, admit the proteine theory as taught by Mulder, up to the time when the controversy began (and as generally adopted by chemists on his authority), simply because his results have *not* been confirmed. It is entirely a question of fact, and I have never been able to see why Mulder should have taken offence because Liebig stated in his Journal, that he and his pupils had not been able to procure proteine free from sulphur, by Mulder's processes, or by any other process. This statement was made in courteous and respectful language, accompanied by a request that Mulder would describe his method so minutely as to enable others to obtain the same results. Instead of doing this, Mulder became angry, and, up to this time, has not proved the existence of proteine free from sulphur. On the contrary, it appears that he not only admits the presence of sulphur in his proteine, but seems to assert that he had always known and admitted it. Now I, for one, must say, that I always understood the very turning-point of his theory to be the existence of proteine free from sulphur, nor can I find any indications of a different view in his numerous papers up to the time of the controversy.

I beg to apologise to you for this long communication; but while I desire to see the most perfect liberty of remark enjoyed by reviewers, I am equally desirous that no review, especially when its tone is unfavourable in some points, should rest on statements of fact which are erroneous; and, I am sure, you will not object to my having drawn your attention to the mistakes which I have mentioned.—I remain, Sir, your obedient Servant,

WILLIAM GREGORY.

COLLEGE, 1st Dec. 1847.

We have inserted the foregoing letter with the omission (for the sake of brevity) of one unimportant paragraph, and this contrary to our usual practice of not noticing such communications, in deference to the position of the author as a chemist, and as the exponent of Liebig's views in this country. We may, however, be permitted to object to some of the professor's observations; to deny that our review was dictated by an unfriendly spirit; and more especially to show, that our observations were not erroneous, and neither misstatements of facts nor the result of imperfect information.

With regard to Dr Gregory's first observation, we have to state that Heintz published his discovery of a new principle in the urine in 1844 (Pog-

gendorff's *Annalen*, vol. lxii. page 602), *previous* to the publication of Pettenkoffer's paper, and that the analysis by which that substance was ascertained to be kreatine, was published in Poggendorff's *Annalen* for March last, three months before the date of the Chemistry of Food. His name must, therefore, have been well known to Liebig, and ought in justice to have been mentioned in connexion with a substance which he was the first to discover in the urine.

Secondly, The June Number of Liebig's *Annalen* did not reach Edinburgh till after the publication of our review, and, even if we had then seen it, would only have served to confirm us in the opinion, that the English translation, like the German original, ought to have been published in a scientific journal; and we venture to assert, that it would have been willingly received into the pages of Taylor's Scientific Memoirs, a periodical expressly intended for such articles.

Thirdly, Dr Gregory appears to have somewhat misunderstood our observations, which were intended to refer to the exaggerated importance attributed to kreatine as a nutrient principle in the food; and it is clear, from various passages in Liebig's work, that he believes it to possess some such function, although, as Dr Gregory states, its special nature is not defined, and for the simple reason that the experiments detailed are totally insufficient to determine it. Now, our statement was, that the facts militated against the existence of any such function, and rather tended to strengthen the opinion of Heintz, that kreatine is an excrementitious substance, and Dr Gregory has not presented any arguments to induce us to alter the opinion expressed in our review.

Fourthly, We readily admit that a mistake, though a trivial one, has been made in attributing to Liebig the statement that, in putrid urine, kreatine is converted into kreatinine. But if not, where has it gone?

Fifthly, We beg to reiterate the statement, that Mulder's researches were confirmed in the Giessen laboratory, and refer Dr Gregory to his own translation of Liebig's Animal Chemistry, second edition, page 322, where he will find no less than seven analyses of proteine made at Giessen, and compared with those of Mulder, in order to show the identity of the results; and if we are to infer from Dr Gregory's observations, that the proteine employed in these analyses was not obtained free from sulphur, their publication, without notice of that fact, would constitute a case of *suppressio veri*, of which we believe Liebig to be totally incapable. In the same work there are also contained upwards of forty Giessen analyses of albuminous substances, in all of which the nitrogen agrees with that obtained by Mulder, although we are now told that that quantity is too small. These analyses were made by Lyon, Playfair, Varrentrapp, and Will, Scherer, Boeckmann, and others, both by the old and new methods; and it seems impossible to suppose that all those skilful chemists should have made errors, and more especially errors exactly equal in amount.

We are not parties to the proteine question, and shall not discuss it with Dr Gregory further than to observe,—that if we have rightly understood the matter, it was exactly the *ill-natured* courtesy and *mock* respect of Liebig's celebrated "Question to Mulder," which exasperated the Utrecht professor, and has been the main cause of that acrimonious controversy.

NOTE ON THE TREATMENT OF CHOLERA BY THE CHINESE. By JOHN COLDSTREAM, M.D. Fellow of the Royal College of Physicians, Edinburgh.

IN a manuscript translation of a modern Chinese work on the treatment of diseases, lately forwarded to this country by Mr Lockhart, medical missionary at Shanghai, there occur the following prescriptions for the treatment of cholera; which, as coming from a quarter where the ravages of that scourge are frequently and severely felt, may be deemed not altogether unworthy of attention, especially at the present time, when every thing relating to cholera is matter of interest. It is remarkable, and, doubtless, to be regarded as something more than a mere accidental coincidence, that the chief agent in these prescriptions is the same that attracted considerable notice in various parts of Europe; and, latterly, was almost the only remedy employed by many prac-

tioners in this country, during the epidemic of cholera in 1831-32, namely, *common salt*.

The following are the prescriptions:—

“In cases of vomiting and purging, where there are dizziness of the head and confusion of vision, with coldness of the limbs, if the administration of medicine be at all delayed, the patient will be lost. You ought instantly to administer pepper, quince, and salt, of each five drachms; mix them with three bowls of water, and make a decoction to be administered, which will effect a cure. Also, use mint leaves and orange peel, of each five drachms, and make a decoction for use. Thus, if the patient be even in a state of collapse, he will be instantly revived. Also, take soot from the bottom of a cooking-pan half a drachm, and soot from the top of a chimney one drachm and a half; mix them with hot water for a dose. In cramp, with violent retching, whenever the limbs are cold, with ineffectual retching and severe straining without any result, drawing up of the knees to the abdomen, excessive perspiration, cold breath, respiration almost extinct, take of hot salt one ounce, ivory black one drachm, and make a decoction for use, which will produce vomiting, and the energy of the stomach will be restored, and the patient will be cured. Or, take two spoonfuls of salt, boil with boy's yellow urine for a dose, which will be efficacious. Or, take of pepper three or four ounces, with several ounces of salt, make the whole warm, and apply it over the navel as a fomentation, which will effect a cure.”

Neither in the curious work whence these prescriptions are extracted, nor in the quotations from Chinese medical authorities given by Du Halde, can I discover any other notice of the medicinal use of salt. It seems to be, in China, peculiarly the medicine for cholera. Its employment in the treatment of this disease is made all the more remarkable by the fact, that its too free use as a condiment is particularly condemned by the Chinese writers on dietetics. Thus, in the Tchang-seng, an excellent work on the art of procuring health and long life, quoted by Du Halde, the following occurs:—“What is most to be avoided in seasoning, is an excess of salt. Salt slackens the motion of the blood, and occasions a difficulty of breathing: salted water flung into the blood of a creature just killed, immediately thickens and congeals it. Thus it is seen that those whose common food is salt meat, have a pale complexion, a slow pulse, and are full of filthy and corrupt humours.”

HOMŒOPATHIC VERACITY!!—By GEORGE W. BALFOUR, M.D., Corstorphine.

In my report of the homœopathic practice in the Gumpendorf Hospital, Vienna—*British and Foreign Medical Review*, Oct. 1846—I have more than hinted that, with respect to the published statistics, “all is not gold that glitters,” and have “proposed to establish facts, which, if established, would certainly tend to throw considerable doubt on the truth of Dr Fleischmann's statement.”—(*British Journal of Hom.* Jan. 1847.)

Now, I have just obtained a copy of Fleischmann's report of that hospital for the year 1846—(*Ester. Zeitsch. f. Hom.* 3 band, 3 heft)—and I proceed to collate a few of the statistics there so ostentatiously paraded, with those contained in the above-mentioned report for three months—May, June, and July, of the same year.

1st, *Pneumonia*.—All the cases, nineteen in number, in every report, had the diagnosis written in chalk at their bed-head; three of them died. The details of the section of two are given, the third was not dissected;¹ but from the notes given there can be no doubt as to the diagnosis. Of nineteen, therefore, admitted in three months, three died, 15 per cent.; while, according to Fleischmann's report, of sixty-four admitted during twelve months, *but two died*, 3 per cent.

¹ From some mistake of the printer, the original report of the case No. VI. requires correction.—*Vide Brit. and For. Med. Rev.*, April 1847, p. 611,—*errata*.

2d, *Intermittent fever*.—All the cases are reported by Dr F. as discharged cured, except two dead; at p. 576 of my report, one case is mentioned as having been discharged, not only unimproved, but actually worse, and that case I well remember.

Ophthalmia.—One case, p. 591, the day after her admission was not to be found. Was she cured of acute catarrhal ophthalmia in less than twenty-four hours? All three are reported by Dr F. as cured, except one remaining.

I have reported one case of *general dropsy*, and two of *pleuritic exudation*, as having been discharged; the one but slightly, the others not at all improved. Under these heads all are reported cured by Dr F., except one case of general dropsy, dead.

These errors, especially the first, are too glaring to be lightly passed; Fleischmann's tables have been hitherto implicitly trusted and quoted as valuable documents, surely they will be so no longer.

CHANGE OF POLITICS IN THE LANCET—MR SYME'S APPOINTMENT.

In some recent articles on Mr Syme's appointment to the Chair of Clinical Surgery in University College, our London contemporary has suddenly exhibited one of the most startling changes in political views we ever remember to have witnessed. Formerly the *Lancet* was an enemy to every thing exclusive, and a friend to the opening up of institutions, and the nomination of men of merit to important posts. Now, the choice of Mr Liston's successor must be limited to the pupils of the school; he must not be a British, but a London, or at least an English surgeon. In short, the system which is ruining the old London hospitals, is to be applied to those of University and King's Colleges. We sincerely trust that our contemporary may be enabled to explain to the satisfaction of the profession this desertion from what we cannot help thinking, after all, to be the correct opinion on these points.

The subject of hospital appointments, and of clinical chairs, is a much more important one to the profession and to the public than many suppose. It is only from the vast field for observation that an hospital furnishes, that we can expect the production of contributions to the art of medicine on a sufficiently extensive scale to be useful. By excluding men who might advantageously cultivate that field, and appointing others who, from ignorance or indolence, leave it a sterile waste, immense injury is done to medicine, to the profession, to the public at large, and especially to the unfortunate poor for whose benefit such institutions were founded. In a medical school every thing depends on the hospital; for if this do not contain able and energetic physicians and surgeons, no past reputation will serve to maintain its fame, or attract students to its wards. It ought to be the effort of a liberal profession to support the cause of knowledge *versus* ignorance; reputation *versus* mediocrity; and of public talent *versus* private interest. If the London colleges uphold this doctrine, they will soon exhibit their superiority to the older universities; and their hospitals, recruited from time to time from the most distinguished teachers in other medical schools, will not only deserve to be considered, but actually constitute the NATIONAL seminaries for sound professional education.

NON-REMUNERATION OF SCOTCH PRACTITIONERS FOR IMPORTANT AND DANGEROUS SERVICES.

IN our December Number we made some observations on the hardships and injustice to which Scotch practitioners are exposed, as compared with their medical brethren in England and Ireland. We have been glad to find that those observations have found their way into some of the public papers, because we feel satisfied that the evil has only to be known to be in some degree remedied. The following facts, brought before the Statistical Section of the

British Association, at the Southampton meeting, by Professor Alison, will speak more powerfully than any thing we could say on the subject.

"An association of medical practitioners was formed at Edinburgh in November 1845, to collect information as to the remuneration of medical practitioners for attendance on paupers. Returns were obtained from forty-two towns, exclusive of Edinburgh and Glasgow, from which it appeared, that in sixteen of these towns there was absolutely no *required* medical relief, either from the public authorities or from voluntary subscriptions. In four, an occasional payment, never exceeding a few shillings, had been made on special occasions. In Campbeltown, L.10 was allowed to the professional men during the epidemic fever. In Kirkintulloch a similar sum was given, but by a private individual. In Dundee, during the same fever, L.5 each was allowed to six dispensary surgeons. In some other places L.2 was given to a surgeon; and in others a small allowance was made for drugs. In anticipation of the new poor-law, L.10 has been allowed annually for medical relief in Alloa. In Dunbar L.6, 6s., but this includes the supply of drugs. In Dunfermline L.20 a-year, not including drugs. In Greenock, L.25 per annum has been paid to each of three district surgeons. In Kilmarnock, L.10 each to three surgeons. In Wick, L.15 is divided between two surgeons. In Dumfries, L.10 to one surgeon. The unrequited medical labour is stated by twenty-five gentlemen, and ranges from L.5 to L.220 annually in value, giving an average of L.40 per annum. But this is not the only tax levied on the charitable feelings of medical men; in ninety per cent. of the cases they had to furnish wine, food, &c., out of their own substance; and in thirty-three of the forty towns brought under review, no change has been made in this system.

"The number of returns from rural districts made, amounts to 325; but of these, 94 have received some remuneration, but only 39 annually. Of these 39, only 13 have received sums above L.5; 26, above L.1 and less than L.5; and 9, under L.1. Two are paid by the bounty of private individuals; and of these, one is paid L.60 by a nobleman, and another L.40 by a landed proprietor; both, however, have the charge of extensive districts, and as there is no fund on which they can draw for drugs or necessaries, there are large drawbacks to be made from the remuneration. Twenty-three have received gratuities for their services, chiefly during the prevalence of epidemics. In one case this gratuity amounted to L.20, and in 14, it was under L.5; in two cases it was only 3 shillings. In one of these cases, this 3 shillings was the only remuneration for 12 years' attendance on paupers averaging 70 constant, and 13 occasional patients; in the other, the 3 shillings was a remuneration for passing paupers of other parishes, and *nothing was allowed for 21 years of attendance on resident paupers*, averaging 44 constant on the district roll. 211, or above 60 per cent., have never received *any remuneration of any kind* for their professional attendance on the parochial poor, or for the drugs which they have deemed it necessary to supply to them; and 208 add, that they have had occasion to give wine, food, &c., from their own limited funds, and that they have had occasion to defray all travelling expenses when they made distant visits. 136 have estimated the money value of the unrequited labour which they have bestowed on the parochial poor: it amounts to L.34,447 annually, or on an average of L.253 each. The complaints of inattention to sick paupers, by the parochial authorities, are very general; and when applications were made for the repayment of different outlays, they were almost invariably refused."—*Report of the 16th Meeting of the Brit. Assoc. for the Advancement of Science*, 1847.

From this it will appear that Scotch practitioners are very differently situated from their brethren in England and Ireland. There they contrive to get paid, however inadequately; whilst we, in the great majority of instances, get absolutely nothing. The late Croydon case, in which a woman in labour was deserted by the Union practitioner, on his discovering that she had not an order, has excited a great sensation; and we now find the *Times* lending its powerful aid in support of our ill-used profession. The question, however, in England is *insufficient* remuneration, and not *absence* of remuneration. How much more powerfully, therefore, the following passages apply to the totally unpaid practitioners in the northern division of the country:—

"We are perfectly prepared to admit the extreme difficulty in which a surgeon having the care of paupers will be placed, if he would, at the same time, preserve a good name, and get a living by his profession. It is too much to expect of any man in business that he should neglect profitable employment for the purpose of exercising a gratuitous humanity. There is no reason why medical, beyond all other professional men, should be expected to practise a sort of pecuniary martyrdom for the purpose of saving humanity in general from the shocks to which it is naturally sensitive. Parochial surgeons may well demand from society at large a participation in those sacrifices for the sake of the poor, which are now thrown too exclusively on those with whom the poor are brought into immediate contact. If we cannot bear to read of pauper patients dying of neglect, we ought

not to bear the existence of laws so inadequate to secure sufficient medical relief that nothing but a self-sacrifice on the part of the surgeons can protect suffering poverty in many heart-rending cases from the chances of miserably insufficient help or positive abandonment. We should very justly deem it a monstrous proposition if it were to be declared that every keeper of an eating-house is an inhuman wretch if he declines to throw open his establishment gratuitously to the hungry and destitute; yet such is the nature of the sweeping accusation of cruelty to which surgeons are liable when they refuse or hesitate to attend pauper patients without a prospect of payment for the time and medicine that will be required.

"There can be no doubt of the entire hopelessness of the attempt to provide for the medical relief of the poor by paying for it at the lowest rate at which it is tendered. Nothing can be supplied honestly and permanently for less than it is worth; and with the skill of the professional man, as with the commodities of the tradesman, underselling must either be carried on by fraud, or be brought to an end by ruin. Parochial surgeons must be properly paid before parochial patients can be properly attended; for in nineteen cases out of twenty, a desire to prove by experiment the very cheapest rate at which a duty may be performed, ends in ascertaining the highest cost at which it may be neglected. It is folly to expect liberal and generous conduct in return for mean and shabby treatment. Nor can we be surprised, therefore, at finding among parochial surgeons many instances of behaviour calculated to bring discredit upon an honourable and humane profession."—*Times*, January 20, 1848.

In connexion with this subject, we give the following extract from the letter of an anonymous writer in the pages of the same newspaper:—

"The Union doctor leads a hunted life; he is ever on the run that he may live at all; if he has time to stop, to gain one moment's ease—if he can earn, in his breathing-time, the means which may save him from sinking altogether, is he to be grudged it? In bed and out of it, on foot, on his horse, in his gig, he is an object of chase; regular orders, urgent irregular requests, charitable petitions, weeping relatives, philanthropic ladies, bustling clergy, are ever on his track; and through what a country is he driven, to what scenes do they urge him! disease in rags; suffering of the worst kind, to be tended amidst dirt of every the most disgusting nature; science expected to combat its foes in a field in which neglect, oppression, ignorance, and depravity have raised up difficulties which defy his skill, as they disgust his very sense. He works on, receiving but little reward for all the good he may effect, a butt of common attack for every mischance or possible imputable case of neglect.

"A man may be a brute that leaves a woman in child-bed as soon as he finds she cannot get an order; a man may be a monster who drives by, and leaves unhelped some sufferer from accident, because he can get no promise of pay for his services; but I, for one, think the legislator a more brutal monster who deliberately plans a system which is for ever assaulting the charity of one class of professional men, who have little to give but time, to whom time is bread, and of whose whole time a large proportion is exacted of them at a price which will not even give bread.

"The whole system of 'orders' needs revision—the medical officer ought to be paid the real value of his services; competition in his case is no fair criterion; he should be made subordinate, in the first instance, to a board of medical inspection; he should be paid for attendance and *prescribing*, not for drugs and appliances; he should have these liberally supplied him from sources which would secure their being the best of their sort, or at least as good as his purposes required. The present system gives us no security either for proper attendance or the administration of proper remedies; it makes boards of guardians authorities in matters they do not understand; it leaves the pauper to a process of obtaining first an order, then the doctor, then his medicine, which in each stage is one in which success is too much a matter of chance; it satisfies no party, and continues to excite, from time to time, public obloquy by the exposures of neglect and cruelty to which it is ever liable."—*Times*, January 21.

During the discussion of these matters, we have to impress upon Scotch practitioners the necessity of looking after their own interests. In our next Number we shall continue this subject, by inquiring narrowly into the operations of the recent poor-law, and to what rights they are entitled from its enactment. We must again repeat, that a fair arrangement, between the practitioner and the authorities, is *entirely in his own hands*. In the mean time, we hereby promise that, if the members of the profession in Scotland be only true to themselves, we shall not abandon this question until it is satisfactorily settled to their advantage.

BOOKS RECEIVED.

1. On Poisons in relation to Medical Jurisprudence and Medicine. By Alfred S. Taylor, F.R.S., &c. London. Small 8vo. 1848.
2. A Manual of Elementary Chemistry, Theoretical and Practical. By George Fownes, F.R.S., &c. Second Edition. London. Small 8vo. 1848.
3. The Pocket Formulary and Synopsis of the British and Foreign Pharmacopœias, &c. By Henry Beasley. Fourth Edition. London. 12mo. 1848.
4. The Cholera not to be arrested by Quarantine, &c. By Gavin Milroy, M.D., &c. London. 8vo. 1847.
5. Ventilation illustrated: a Tract for the Schools of Rich and Poor. London. 8vo. 1848.
6. A Popular Essay on Anæsthetic Agents, &c. By William Henry Mortimer. Surgeon-Dentist. London. 8vo. 1847.
7. Practical Observations on Midwifery, and the Diseases incident to the Puerperal State. By Alfred H. McClinton, M.D., and Samuel L. Hardy, M.D., &c. Dublin. 8vo. 1848.
8. A Treatise on Fractures in the vicinity of Joints, and on certain Forms of Accidental and Congenital Dislocations. By Robert William Smith, M.D., &c. Dublin. 8vo. 1847.
9. A proposed Bill for regulating the Police, and for improving the Sanitary Condition of the City of Edinburgh.
10. Pocket Dispensatory and Therapeutical Remembrancer, &c. &c. By John Mayne, M.D., &c. London. 12mo. 1848.
11. The London and Provincial Medical Directory. 1848. London. Small 8vo.
12. The Retrospect of Medicine. Edited by W. Braithwaite, Esq. July to December. London. Small 8vo. 1847.
13. Outlines of Physiology for the Use of Students. Part I. By Allen Thomson, M.D., Edinburgh. Small 8vo. 1848.
14. The Sanitary Condition of Great Yarmouth, a Lecture, &c. By C. Lockhart Robertson, M.D., Yarmouth. 12mo. 1847.
15. Eighth Annual Report of the Crichton Royal Institution for Lunatics. Dumfries. 11th November 1847. 8vo.
16. Abstract of the Proceedings of the Obstetric Society of Edinburgh for 1846, 1847. Edinburgh. 8vo. 1847.
17. Handbuch der Rationelen Pathologie. Von Dr J. Henle. 2 Band. Specieller Theil. Erste Abtheilung. Pathogonie. Braunschwig. 8vo. 1847.

TO THE EDITORS AND PUBLISHERS OF JOURNALS.

THE altered arrangements of the Journal have induced us to revise our exchange list. We have struck out several periodicals, either from their arriving irregularly, or from their being of little service to us.

We have to request the particular attention of our German friends to the statements under mentioned. Unless their Journals be forwarded more regularly the exchange must be stopped, as it will in future answer our purpose better to obtain a few of them by purchase, than wait so long. They should be forwarded to the House of Hartman in Leipsic, for the Editor of the MONTHLY JOURNAL OF MEDICINE, care of Messrs Williams and Norgate, Henrietta Street, Covent Garden, London.

1. *The British and Foreign Medico-Chirurgical Review.*
2. *The Dublin Quarterly Journal of Medical Science.* Received regularly.
3. *The Obstetric Record.* Received regularly.
4. *The Provincial Medical and Surgical Journal.* Received regularly.
5. *The Chemical Gazette.* Received regularly.
6. *The London Medical Gazette.* Generally received three or four days after publication.
7. *The Medical Times.* Received regularly.
8. *The Dublin Medical Press.* Received regularly.
9. *The American Journal of the Medical Sciences.* Received regularly.
10. *The Medical Examiner*, edited by Robert M. Huston, M.D. Last number received is for June 1847. If not sent regularly, exchange will be stopped.
11. *The Southern Medical Journal.* We accept the exchange, but can find no means of transmitting our numbers.
12. *British American Journal of Medical and Physical Science.* Received regularly.
13. *Archives Générales de Médecine.* Received regularly. The last numbers for November and December 1847, have just come to hand.
14. *Annales de Thérapeutique.* Received regularly, generally a month after publication.
15. *Annales Medico Psychologiques.* Received very irregularly. Last number is for November 1847.
16. *Journal de Médecine et de Chirurgie Pratiques.* Received with tolerable regularity, generally about a month after publication. Last number received is for November 1847.
17. *Gazette Médicale de Paris.* Received regularly.

18. *Gazette de Strasbourg*. Last number received for December 1847. The numbers for January, April, and May 1847, have not come to hand.
19. *L'Union Médicale*. Received regularly.
20. *Bulletin de l'Académie Royale de Médecine de Belgique*. Received very irregularly. Last number received is, No. V. 1847. Numbers I. and II. have not come to hand. If not sent regularly, exchange will be stopped.
21. *Prager Vierteljahrschrift für die Praktische Heilkunde*. We have sent our numbers regularly for eighteen months, and only within a few days have received Nos. 13, 14, and 15, for 1847. If not sent more regularly, exchange will be stopped.
22. *Zeitschrift für Rationelle Medizin*. Von Henle and Pfeufer. We have sent our numbers regularly for eighteen months, and have only received one number, 5 Band 2 Hft. in return. If not sent regularly, the exchange will be stopped.
23. *Zeitschrift der K. K. Gesellschaft der Ärzte zu Wien*. We have sent our numbers regularly, but have only received in return up to September 1846. If not sent regularly, the exchange will be stopped.
24. *Medicinische Jahrbücher der K. K. Österreichischen Staates*. Last number received is for September 1847.
25. *Archives für Physiologische und Pathologische Chemie und Microscopie*. Last number received is Heft 3, 1847. Heft 2, 1847, has not come to hand.
26. *Wochenschrift für die Gesante Heilkunde*. Last number received is No. 39, for September 25, 1847.
27. *Gazetta Medica di Milano*. Last number received is No. 51, for December 18, 1847.
28. *Bibliothek for Læger*. Last number received is for June 1847.

TO CORRESPONDENTS.

The Reports of Societies have been received too late for insertion this month.

A communication has been received from Dr Mercer.

Owing to press of matter, an extra half sheet is given gratuitously in the present number.

We feel much obliged for the reports of the case occurring in the Greenock Hospital, but we have always made it a rule not to mix ourselves up with such proceedings.

TO OUR READERS.

THE experience of the past eighteen months has convinced the Conductors of THE MONTHLY JOURNAL OF MEDICAL SCIENCE that it would be impossible to continue it in its present form, without seriously affecting its efficiency. The number of important original communications with which it has been, and continues to be, honoured, have greatly encroached upon the Review and Periscope departments; so that, whilst we have been the means of furnishing other periodicals with a quantity of most valuable matter, we have been unable to borrow from them to a like extent. The *Journal*, therefore, has been unable to fulfil what has hitherto been its object; viz. a Medical Periodical, in itself complete for all the purposes of the practitioner. Within the last few years, also, two half-yearly Retrospects have been published, which, without any direct pretensions to literary merit or originality of their own, copy wholesale from our own and other pages, and sell the products at a low price. It is impossible for us, without doing a serious injury to Medical Literature, to curtail the communications of our contributors. It is essential that Medical men should have respectable mediums, through which the productions of their pens should appear before the public, unmutilated and free from the risk of being misrepresented. Such a medium ever has, and we trust

always will be, afforded by *The Monthly Journal*. It seems to be impossible for us, however, to keep off parties who seize upon and condense what is most valuable. Many of our contemporaries, whilst they sometimes borrow in this way from our pages, furnish us with original matter in return, and thus there is an exchange of obligation. The half-yearly Retrospects, on the other hand, may be considered as true parasites, who fatten upon our best productions—who yield nothing in return; and at length so eat into our vitals as to destroy our very elements of existence.

As far as we ourselves are connected, it will be perceived, that the respectable character we have endeavoured to earn for this Journal, and the consequent accession of important contributions, by diminishing the space set apart for Reviews, and abstracts from other and more especially Foreign Journals, enables us less and less to cope with our adversaries. Our country friends expect us to contain every thing; but the very circumstances which prove our popularity prevent this being done, and thus our strength is a cause of weakness.

To remedy this defect, we purpose appending to the Journal twenty-four extra pages of letter press, printed in a reduced, but most readable type, set in double columns. This addition will be devoted exclusively to translations, abstracts, and condensations from the whole Medical Press. It will be paged separately, and form a volume annually, under the name of the MONTHLY RETROSPECT OF THE MEDICAL SCIENCES. Both *Journal* and *Retrospect* will appear together,—price 2s. per Number.

In thus increasing the bulk and consequent usefulness of our *Journal*, we expect to obtain many additional subscribers, more especially when our plan is understood and seen in operation. The four sheets and a half, hitherto forming the *Journal*, will now permit us to extend the length and increase the number of our Reviews, and will also increase the interest of the Medical News department. In this manner, we pledge ourselves that *The Monthly Journal* and *Retrospect* combined, shall be so complete as to lessen the dependence of our country friends on any other periodical.

As regards the *Retrospect*, the most extensive arrangements have been concluded for rendering it perfect. Each section will be under the direction of gentlemen who have especially devoted their attention to the subjects it embraces, which will be classified as follows:—

I. Medical Physics, Chemistry, and Natural History.

Under this head will be placed all those communications in the sciences farthest removed from Medicine properly so called, in which, however, many important facts are continually brought forward of the highest value. Of this, the lectures of Professor Matteucci offer an example. Here, then, will be recorded, 1st, All the facts connected with Optics, Acoustics, Hydraulics, Electricity, &c., connected with Medicine. 2d, Organic Chemistry, now so important to the study of Physiology and Pathology. 3d, All that is interesting in Natural History as tending to elucidate the functions of man, or bearing upon the other branches of medical science, will find a place, embracing Geology, Botany, Mineralogy, &c.

II. Anatomy, Physiology, and Physiological Chemistry.

As Medicine advances, the more evident does it become that a well-grounded

knowledge of Anatomy and Physiology is necessary for the medical practitioner. All works and publications styled "practical," if by that term be meant (as it too frequently does) the mere subject of treatment, independent of theory or principle, cannot be too highly condemned by the judicious practitioner. Physiology is now founded on minute or structural Anatomy, and Organic Chemistry (Physiological Histology), the cultivation of which is of modern date, and consequently practically unknown to the majority of the profession. But as we are convinced that the only true mode of advancing the practice of Medicine, is furthering, in the first place, Physiology, we shall continue to devote our best attention to this subject. *The Monthly Journal* has taken a leading part in advancing and propagating this branch of Medical inquiry, and we trust that our labours in this direction will constitute a strong claim on every member of the profession who does not support a pure empiricism.

III. Morbid Anatomy, Pathology, and Pathological Chemistry.

Morbid Anatomy and Pathology have, within the last few years, been successfully cultivated to a large extent. The progress of Physiological has led to the advance of Pathological Histology, and the minute structure and chemical constitution of morbid products, are now looked forward to as the great means of introducing an exact diagnosis, and a rational treatment of disease. In this branch of inquiry, the German School of Medicine has taken the lead; and an exposition of its principles, as detailed in the numerous periodicals of that country, will constitute another of the leading features in our *Monthly Retrospect*. The increased number of pathological writers have furnished numerous reports, of which we shall also avail ourselves in collecting for this section.

IV. Practice of Medicine.

Under this head will be more particularly recorded all that relates to the Diagnosis and Treatment of individual diseases. Diagnosis can only be rendered exact by submitting the physical changes in organs to the cognisance of the senses. The different methods of enabling us to do this by means of the Pleximeter, Stethoscope, Speculum, Microscope, Test-tube, &c. &c., will obtain especial notice. The various British, American, French, German, Italian, Danish, and other journals will be minutely examined, in order to glean from them the results of clinical observation in all parts of the world, and every thing considered worthy of attention at once transferred to the pages of the *Monthly Retrospect*.

V. Practice of Surgery.

The same care will be taken to obtain every kind of information relating to the practice of the Surgeon. All improvements in operating, and especially methods which render operations unnecessary, will be particularly dwelt upon.

VI. Midwifery, and Diseases peculiar to Women.

This section will comprehend every thing that relates to Obstetric Practice and Female Diseases. The latter class of disorders are so connected with the functions of child-bearing, and form a class of affections so distinct in their nature and treatment, as in our opinion to warrant this association. We do not, however, conceive this to hold good with regard to diseases of children, which will be treated of under the head of Practical Medicine.

VII. Psychological Medicine.

Under this head will be placed such information as belongs to the peculiar Management and Treatment of the Insane, Arrangement of Asylums, Notices of Re-

ports from Institutions, &c. &c. While, properly speaking, all that belongs to this department is only a division of practical medicine, there are such peculiarities connected with it as fully justify its being considered in a separate section.

VIII. Materia Medica, Pharmacy, and Therapeutics.

This division of the *Retrospect* will comprehend a description of whatever relates to substances employed for the Cure of Diseases, their Mode of Preparation, and the general principles which should regulate their Administration. Great attention will be given to this department, under the conviction, that, however important the former sections may be, unless we derive from them good means and correct methods of treatment, the most essential part of the healing art will not be arrived at. The study of Therapeutics has in recent times made decided progress, and there is every reason to believe, that, owing to the cultivation of Organic Chemistry, a new era is commencing in which it will lose its hitherto empirical character, and rest upon a more rational basis. To the practitioner engaged in the discharge of his professional duties, a Monthly Summary of its progress cannot fail to be of the highest utility, presenting him, as it does, with the newest information applicable to bedside practice.

IX. Forensic Medicine, and Toxicology.

Medical Jurisprudence has always, from its importance, demanded a considerable space of the Periscope in *The Monthly Journal*. Under the new arrangement this will be considerably extended, so that every fact of importance may be included. The subject of poisons will be especially attended to, and is so intimately connected with Forensic Medicine that we have associated them together.

X. Hygeine and Dietetics.

One of the strongest evidences of progress in these branches is furnished by the attention now paid to both, by all classes of the community. We shall endeavour, under this our last head, to re-unite, from time to time, all that is important connected with them, including Medical Police, satisfied that our *Retrospect*, which professes to give a complete survey of the Medical Sciences, would otherwise be imperfect.

It may not be in our power to comprise all the Sections in every number of the *Retrospect*, although we shall endeavour to give each a length corresponding to its importance. After a sufficient time has elapsed, short systematic *resumés*, embracing the leading facts published both in the *Journal* and *Retrospect*, with opinions as to their importance in individual departments of Medicine, will be given. In this manner we hope to present our Subscribers annually with a complete Review of Medical Literature, combining the attractions of original matter, critical analyses of new works, medical news, and retrospective summaries.

THE
MONTHLY JOURNAL
OF
MEDICAL SCIENCE.

No. LXXXVII.

MARCH, 1848.

No. 21. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Cases of Hernia in which the Stricture was divided External to the Sac.* By JAMES DUNCAN, M.D., Lecturer on Clinical Surgery, and Senior Ordinary Surgeon to the Royal Infirmary, Edinburgh.

(Read to the Medico-Chirurgical Society of Edinburgh, February 16th, 1848.)

I HAVE brought the following cases before the Society, because, in operating upon them, I have followed the practice of dividing the stricture external to the sac—a practice which I believe to be much safer than the one generally pursued; and because I feel convinced, that it is one which ought to be, and most certainly will be, more generally adopted than it now is.

The practice was first proposed by Petit in 1718, and was shortly afterwards strongly advocated by Monro. It was likewise favourably spoken of by Sir A. Cooper as applicable to a certain very limited class of cases; but its principal advocate has been Mr Key, who, in his able memoir published in 1833, has so fully discussed the subject, as to leave little or no room for additional remark. Mr Luke of London has, I believe, frequently and successfully followed the practice, and Mr Liston informed me that, for some few years, he had been in the habit of attempting it in all cases of recent strangulation, particularly of femoral hernia.

The proposal, however, has never received the consideration it appears to me to have merited. The objections urged against it have been too readily received as conclusive; and perhaps, too, those surgeons who had the best opportunities of testing the practice, having in their own experience found the ordinary operation by no

means a fatal one, preferred continuing that which they had already found tolerably successful, to trying one which, they had been taught to believe, was in some cases attended with much difficulty, and in not a few with considerable danger. Such was exactly my own feeling, until I was led by accident to follow the practice. I had found the old operation, particularly when had recourse to early, by no means a fatal one, and I hesitated trying another which I had been taught to believe to be both difficult and dangerous.

I am perfectly willing to admit that the usual operation, when performed under favourable circumstances, is a very successful one; but at the same time, I am of opinion, that if the modification recommended by Petit were adopted, it would be rendered much more so. It cannot be denied that the danger of wounds involving the peritoneum is great; and every surgeon must have seen cases of hernia, operated upon under the most favourable circumstances, go wrong, and terminate fatally, in consequence of inflammation following the operation. I have myself seen not a few terminate in this manner, in which no appreciable amount of inflammation existed at the time of the operation; and in some on which I have myself operated, I cannot but believe that the result would have been more satisfactory had I acted differently. If the danger be such in cases where the peritoneum is as yet unaffected, it cannot but be greatly increased when inflammation is already established, involving as it does the exposure of the parts to the external air and diminished temperature, and, in many cases, to not a little direct handling of the gut when there is any difficulty in its reduction. If these dangers can be avoided without incurring risks of greater magnitude, as I believe they can be in the great majority of cases, it is undoubtedly the duty of the surgeon to do so. There is no doubt that one great cause of the fatality of the operation for hernia, may be fairly ascribed to delaying its performance too long, and the consequent continued exposure of the intestine to the strangulating cause. Mr Hey observes, "that when he performed the operation late, he only saved two patients out of five; whereas, when he performed it early, he lost two in nine." It has been remarked by Mr Luke, that there are two causes which influence the mind of the surgeon, tending to create, on his part, a temporising and procrastinating practice—these are, the general knowledge which he has acquired of the unfrequent fatal termination of hernia when returned by the taxis, and the frequent fatal sequence of operations. If, says the same surgeon, that presumption be correct, the best and most useful endeavour will be to strip the operation of its terrors, by substituting in its place one simple in its execution, and devoid of the ascertained dangers to which it exposes the patient. By the adoption of Petit's operation in place of the ordinary operation, I think this beneficial substitution is attained; an opinion which, if equally impressed upon the minds of surgeons in general, will doubtless remove the chief obstacle preventive of the early recommendation of operation.

CASE I.—*Oblique Inguinal Hernia—Strangulation for twelve hours—Division of Stricture outside the Sac—Cure.*

The first person on whom I operated in this manner, was an old man, aged seventy, whose case I formerly inserted, amongst some others, in the *Monthly Journal of Medical Science*.

The case was one of large inguinal hernia of the right side of long standing. Symptoms of strangulation had existed for twelve hours when I saw him. Attempts at reduction, made by Sir G. Ballingall and myself, having failed, the nature of the case was explained to the old man, and he readily submitted to the operation. The tumour was large, tense, and exceedingly tender. An incision of three inches in length was made over the neck of the tumour, and the tendon of the external oblique exposed. It was at once seen that the external ring was the seat of the stricture. The constriction was exceedingly tight, and the edges of the ring were completely concealed by the projection which the tumour formed around them. I determined upon dividing it if possible, without interfering with the sac. This was accomplished with facility by dividing the constricting parts with the point of the bistoury, these, from their state of tension, giving way at the slightest touch. The only difficulty arose from the edge of the ring being concealed by the bulging which the tumour formed round it. This, however, was easily overcome by drawing down the tumour, and compressing it with the point of the finger immediately below the point where it was wished to divide the stricture. After four or five lines of the tendon were thus divided, it was at once seen that the constriction was relieved, and the intestine was returned without the slightest difficulty. The patient made a rapid recovery, the greater part of the wound healing by the first intention.

CASE II.—*Femoral Hernia—Strangulation eighteen hours—Operation—Cure.*

Mr M., aged sixty—Oct. 10th—had been affected with femoral hernia of the right side for some years. It had always previously been reducible. I was called to see him at five A.M., and found that he had been labouring under symptoms of strangulation for about eighteen hours, the bowel having descended when he was walking up from Leith on the previous morning. The symptoms, when I saw him, were urgent. The tumour was of considerable size; but it was difficult to say whether it was an inguinal or a femoral one.

The attempts at reduction, made by Dr J. Brown and myself, having failed, the operation was at once proposed, and readily submitted to. An incision of three inches in length was made in the long axis of the tumour, parallel with Poupart's ligament. On cutting through the adipose cellular tissue, some irregular-shaped fatty tumours were exposed, much resembling enlarged appendices epiploicæ, and behind these, but attached to them, was a small hernial sac, which was felt to contain a portion of intestine. It was now at once seen to be a femoral hernia, and, by gently drawing down the tumour, the edge of the ring was exposed embracing it tightly. I immediately saw that the stricture could be readily divided without interfering with the sac. This was done by means of a probe-pointed bistoury, and the bowel returned, the sac being retained between the fingers while reducing it. The symptoms were at once relieved. The edges of the wound were brought together by several points of suture, and a compress and bandage applied in the usual manner. The bowels were freely moved four hours after the operation. The patient continued to do perfectly well until the fourth day, when some erysipelatous inflammation made its appearance around the edges of the wound, which had all united with the exception of a small portion of about half an inch in length. This continued to extend for some days, unattended with constitutional disturbance, and terminated in a small slough of the integuments. The patient made a good recovery.

CASE III.—*Femoral Hernia—Strangulation for eleven hours—Operation—Cure.*

This case was admitted under the care of Mr Miller in the Royal Infirmary, who, being at the time confined by indisposition, requested me to attend to the patient. I extract the particulars from the hospital books.

Eliza Mainger, æt. thirty-five, admitted 13th Jan. 1847. This patient had been affected with hernia for about four years. During that time the bowel had descended very frequently; but, until the present occasion, she has always been able to reduce it herself. The first indication she has of its having descended, is severe twisting pain in the umbilical region, which ceases immediately that reduction is effected.

About one o'clock on the day of her admission, she felt pain in the abdomen, and then observed that the bowel had protruded. She was uncertain whether it had come down at that time, or previously. She was unable to reduce it as usual, and, becoming alarmed, sent for assistance at five P.M. She was then put into a warm bath, and attempts at reduction made, but without effect.

When admitted, at eight o'clock P.M., she was complaining of severe pain in the umbilical region, and had been vomiting occasionally for some time previously. Much thirst; pulse small. The taxis was attempted, but without success. An enema of warm water was then administered, two grains of opium given, rags wetted with sulphuric ether applied to the tumour, and the attempts at reduction renewed, but still without success.

I was now sent for, at about half-past eleven P.M., and, finding that the attempts at reduction by taxis were ineffectual, I immediately operated. A T-formed incision was made over the tumour, and the margins of the ring readily exposed, closely embracing its neck. These were divided by means of the probe-pointed bistoury to such an extent as was believed quite sufficient to relieve the constriction, and attempts made to reduce the bowel. These, I at once saw, would be ineffectual; inasmuch as, although the tumour itself could be made to recede somewhat, no impression was made upon its contents by such a slight degree of force as I believed alone to be admissible. I now thought that I had to do with one of those cases in which the stricture is seated in the neck of the sac, and that its incision would be necessary. Before doing so, however, I examined the neck more carefully, and found that the constriction was caused by a very narrow band of filamentous tissue, external to the neck. The edge of the bistoury was applied to this; and, as soon as it was divided, the parts were liberated, and the bowel was readily returned. On the morning of the 14th, the day after the operation, a dose of castor oil was administered, followed by a lavement, which had the effect of freely moving the bowels. On the 16th, some erythematous inflammation made its appearance around the edges of the wound, attended with slight constitutional disturbance. This, however, gradually subsided, and the patient made a good recovery. I may mention that, during the operation, it was discovered that this patient was affected with disease of the right ovary, the tumour being about the size of the two closed fists, and somewhat tender to the touch.

CASE IV.—*Oblique Inguinal Hernia—Strangulation for twelve hours—Operation—Cure.*

Christopher Lowrie, æt. thirty-eight, admitted Sept. 19, 1847. Had been affected with hernia for some years, which, until the day of his admission, had always been reducible. On the morning of that day, he had been unable to return it as usual, and applied for assistance. The taxis having proved ineffectual, he was sent to the hospital to be under my care. Before I saw him, attempts to reduce the bowel had been repeatedly made, but without avail. I was now sent for at about eight or nine P.M., and, finding that I could make no impression on the tumour, immediately proceeded to operate. An incision of about three inches in length was made over the neck of the tumour, exposing the tendon of the external oblique, which was found to be the seat of the stricture. This was readily

divided by means of the probe-pointed bistoury, and the bowel returned with ease. The patient made a most rapid recovery; indeed, so little inconvenience did he feel from the operation, that he got out of bed next morning, and it was with some difficulty that he could be brought to submit to the necessary restraint until a truss could be procured.

CASE V.—*Femoral Hernia—Strangulation for thirty hours—Operation—Cure.*

This case I visited with my friend, Dr Williamson of Leith, in November. The patient, W. P., was an unhealthy-looking subject, and had apparently suffered severely from secondary syphilis. He had been affected with femoral hernia for some years, which he had always, until the present occasion, been able to reduce. It had descended two days previously to my seeing him, and symptoms of strangulation had existed for about thirty hours. Various attempts to reduce it had failed; and, when I visited him, he was suffering severely. My own attempts to reduce it, which were not long continued, having likewise proved unsuccessful, I immediately, with Dr W.'s concurrence, proceeded to operate. A T-formed incision was made over the neck of the tumour, and the ring exposed. Matters were found in the same state as in the case of the woman Mainger, and the stricture divided in the same manner, without interfering with the sac. The operation was followed by immediate relief, the vomiting ceased, and the bowels were moved by medicine during the course of the following day. The man made a good recovery, although the wound was some time of cicatrizing.

CASE VI.—*Femoral Hernia—Strangulation for forty-eight Hours—Operation—Cure.*

This patient, the last on whom I have operated in this manner, was a corpulent woman, Mrs L., aged sixty-three, affected with femoral hernia, whom I visited, in company with Dr Alexander, on the evening of the 20th November. She had been labouring under symptoms of strangulated hernia from the afternoon of the 18th, and there had been stercoraceous vomitings from the morning of the 19th. She complained, when I saw her, of twisting pain at the umbilicus, and general pain of abdomen; but there was little or no tenderness. The pulse was 90, and of moderate strength. The tumour was of moderate size, and evidently contained a portion of intestine, with omentum. The taxis was tried, but ineffectually. I accordingly immediately operated, assisted by Mr Walker and Dr Alexander. As the patient was corpulent, a pretty free T-formed incision was made over the neck of the tumour, exposing the ring, which was divided to such an extent as I believed sufficient to relieve the stricture. The bowel not being reduced by such a degree of pressure as I thought admissible, I was led to make a further examination, and found that the neck of the sac was embraced by a narrow band, similar to what I have described as having been met with in two of the preceding cases. This was divided, and first the bowel, and then the omentum, reduced with facility. The chloroform was used in this case, with the effect of producing complete muscular relaxation, and greatly facilitating the steps of the operation. The patient vomited largely of stercoraceous matter during the operation. She was left under the effects of the chloroform. On the 21st, I found her free from pain, there had been no more vomiting, and the bowels had been freely moved. When asked whether she had felt pain during the operation, she answered, that I might have cut her to pieces without her being aware of it. She made a good recovery.

Within the time in which these operations were performed, I have operated upon two other cases of hernia, without following the same practice. In the first, the patient was a very old woman, in whom the strangulation had existed for such a length of time, and the feeling of the parts was such, as to lead me to fear that gangrene was established, and, in consequence, to decide upon opening the

sac. On doing so, it appeared that the bowel had receded, probably during the operation, and that a portion of omentum only remained. From what followed, however, it would appear that gangrene of the bowel had in reality existed. The patient continued to progress very favourably for five days after the operation, but, on the sixth, a fecal fistula formed, and continued to discharge for some time; but ultimately the patient recovered.

In the second case, the patient was affected with entero-epiplocele of long standing, and the sac was opened by the first incision, in consequence, apparently, of a prolongation of the sac, containing omentum only, having been raised with the fold of skin, and trans-fixed in incising the integuments. The patient made a good recovery.

I have already stated that I was strongly prejudiced against this mode of operating, and that it was only on finding the parts so favourably circumstanced, in the first case I have related, for dividing the stricture external to the sac, as very naturally to suggest to me the query—Why open the sac?—that I had recourse to it. The succeeding cases have strongly tended to impress upon my mind the truth of the remark of Richter when speaking of this operation,—"Why should the surgeon not be afraid of doing that by the taxis, which only a quarter of an hour afterwards he fears to do in the operation? Will a surgeon of sense be deterred by such reasons from making the attempt? And ought not such reasons, with equal justice, make him reject the taxis, the tobacco smoke, and all other means of reduction, because by all such measures the hernia is reduced without opening the sac?" I would by no means recommend the indiscriminate application of the operation. There are some cases in which its performance would be impossible, and others in which it would be inadmissible; but I believe it to be the proper practice, not only in those large herniæ in which it is recommended by Sir A. Cooper, but likewise in the great majority of recent cases, small as well as large.

Several objections have been urged against this practice—some of them I shall briefly notice. The first I shall advert to, is the alleged difficulty of the operation. This, I believe, has been much exaggerated. In none of the preceding cases was the operation attended with any great degree of difficulty; and in the first only, in which I have mentioned the stricture as having been formed by the sheath of the vessels, had I any hesitation as to the procedure it would be necessary to adopt, inasmuch as in that case I believed at first I had to do with stricture in the neck of the sac itself. In the last case, in which the stricture was similarly situated, in which a considerable degree of corpulence existed, and the parts lay at a great depth, as my friend Mr Walker can testify, the operation was certainly somewhat difficult in consequence, perhaps more so than it would have been had the sac been opened in the usual way, and it required, it may be, somewhat greater caution in its performance.

I cannot help thinking, that some of the cases in which surgeons have failed in completing the operation without opening the sac, must have been similar to these two, and that the difficulty would have been overcome by a little further perseverance. In some of the cases the stricture was reached and divided with the greatest ease, so much so as to strike those who were present, as well as myself, with the simplicity of the operation. If I may judge from my own very limited experience, I should think that, in the great majority of cases, the division of the stricture in this manner would be unattended with any great degree of difficulty; and, even if it were somewhat greater, it could scarcely be urged as any very weighty argument against it, if it could be proved to be attended with additional safety.

Another objection stated is, that there is ground to believe, that the neck of the sac is in general much concerned in causing the strangulating constriction, so as to require division no less than the fascial stricture itself; and that there is a risk, under these circumstances, of returning the sac along with the viscera, in which case strangulation may still be maintained by its narrow neck. It cannot be denied, that cases do occur in which the stricture is seated in the sac itself; but this is just one class of cases in which the operation is inadmissible, or in which, I should rather say, it is impossible to perform it. Its existence there, however, cannot be ascertained until we have proceeded so far with the operation; and, when we do find it so seated, we must just open the sac and proceed in the ordinary way; but then the patient is none the worse of the attempt. I believe, moreover, that cases of this kind are comparatively rare; and that, possibly, some of those in which this state of matters was supposed to have been met with, may have been similar to those I have related. The danger of returning the sac along with the viscera under these circumstances, in which case the strangulation would still be maintained, is not I believe great. This has happened, I am aware, in cases in which the surgeon had returned the whole tumour, under the belief that he had exposed the intestine, when in reality the sac had not been opened at all. But then it would not have happened had he set out with the view of dividing the stricture without opening the sac. Indeed, I believe, that there is less danger of this accident occurring when the sac is exposed, than when reduction is attempted by the taxis, provided due precautions be used, the pressure be applied in the proper direction, and no undue force employed. We have the sac fully exposed, and the pressure is to be applied in such a manner, by grasping laterally, as to act upon its contents without the risk of returning the whole tumour; an accident of which there can be little fear if but a very moderate degree of force is employed, no greater being admissible, according to Mr Key, than is used in reducing a hernia when strangulation does not exist. Besides, should this accident unfor-

tunately happen, it would be immediately recognised, and the proper steps taken.

The most serious objection undoubtedly is, the danger of returning the bowel or omentum in a state of gangrene. Wherever there is any suspicion, either from the length of time which the strangulation has existed, or from the symptoms, that the intestine is in this state—then it is the clear duty of the surgeon to proceed in the ordinary way to open the sac, and to give exit to the contents of the bowel. In some cases, the existence of gangrene is pretty unequivocally pointed out by the symptoms; but there are others in which it may exist, and there may be nothing to indicate with certainty the actual state of matters; still there is generally sufficient to give rise at least to strong suspicions, and of course to induce the surgeon to proceed in the usual manner. The return of a portion of bowel in such a state, would be attended with considerable risk of fecal extravasation when the slough separated, and there is no doubt that the resistance of the undivided sac would act injuriously; but perhaps less so, as has been argued, than when sloughing takes place after reduction by the taxis,—the only barrier in addition to the recent adhesion of the integuments, as in the operation when the sac is divided, being a single layer of serous membrane, which had already been partially detached from neighbouring tissues, and would readily slough under the first burst of inflammation excited by fecal matter in immediate contact with it. In some cases we find, when the sac is opened, that the bowel, though not actually in a state of gangrene, presents such appearances as to indicate an approach to it; and in others we find, though the bowel appears at the time to be in a healthy state, that in a few days a discharge of feces takes place by the wound, indicating either perforation by sloughing or ulceration. The risk of this occurring, Mr Key considers, and I believe with justice, to be much diminished by this operation; the preserving the sac entire diminishing the chance of inflammation, which, in the weakened condition of the bowel, is the cause of the subsequent sloughing of the coats. This objection does not, of course, apply to cases in the early stages of strangulation; and fortunately cases of gangrene are comparatively rarely met with, surgeons generally being now strongly impressed with the importance of early operative interference, and more alive to the danger of long-continued attempts at reduction by the taxis and its adjuvants.

From the above remarks, it will be seen that I would by no means recommend the indiscriminate application of the operation. I am fully convinced, however, that it ought to be attempted in all those cases which we have an opportunity of seeing in their earlier stages, and that it would add materially to the success of the operation for hernia. I have no doubt that, were the practice more frequently tried, its efficacy would be established. There are many cases, as I have stated, in which the attempt to perform the opera-

tion in this manner would be improper, and others in which its completion would be impossible; but in many, I believe in the great majority, it ought to be attempted, and I believe might be readily accomplished, I would say, in most cases in which the attempt at reduction by the taxis is allowable. If we succeed, there is this great advantage, that, instead of a wound implicating important structures, we have simply an incision of the soft parts external to the sac, and attended with little more risk than if the protrusion had been returned by the taxis. If we fail, little or no harm is done. A prominent character of the operation, and one that raises it above many of the objections that have been urged against it, being, as has been well said by Mr Key, that should the attempt to execute it fail, either from want of dexterity on the part of the operator, or from any particular difficulty in the case, the operation can be completed in the ordinary way by laying the sac open.

ARTICLE II.—*On the Characters presented by Urine containing a deposit of Oxalate of Lime.* BY JAMES W. BEGBIE, M.D., President of the Royal Medical Society.

THE chief characters presented by this urine have been very differently stated and described by authors. As I happen, during the last two years, to have examined a considerable number of specimens of this urine, I am led to believe that a short account of these, as verifying some and throwing doubt on other received characters, may not be uninteresting.

First, As to the colour of the urine. I have almost invariably found it of an amber colour, darker than in health. Dr Prout describes it as generally of a pale citron-yellow or greenish hue. In more than one instance I have seen a colour similar to this; but I apprehend that it cannot be considered as in any degree characteristic of the secretion. The amber colour, on the other hand, is likely to lead to a suspicion of the presence of the crystals, more especially when associated with other characters to be immediately described.

Second, The average specific gravity of those specimens I have examined was 1.028, in three or four only was it below 1.015, in not a few above 1.030. In one specimen the specific gravity was 1.034, and in another as high as 1.040; in the two last, the urea was present in such excess, that an immediate crystallisation was caused by the addition of a few drops of nitric acid.

The determination of the average density of oxalic urine is of interest in connexion with its pathological origin, more especially since Dr Prout has asserted its similarity with, and convertibility into, saccharine or diabetic urine. Dr Prout has attributed the low-

ness in density of some specimens, to the increase in the quantity of urine secreted. Dr Bird states, that he has always found the heaviest specimens to contain the most of the oxalate. The pale urine I have in one or two instances (contrary to expectation) found equal in density to the darker variety, and in such the urea was small in quantity, but the crystals were present in very large numbers. In these, the density must doubtless be owing to the presence of some other ingredient.

A difference, as Dr Bird¹ has pointed out, not unfrequently exists in the specific gravity of the urine passed in the morning and evening; and on this account it is desirable, that in all cases a specimen of both should be submitted to examination. Of fifteen specimens I have examined lately, the specific gravity was identical in eight. In six the morning urine was the heavier, and in only one the evening.

An important fact in connexion with these discrepancies in the specific gravity is, that in all the evening specimens which were examined, the density of which was lower than those of the morning, the urea, as far as I could judge, appeared to be present in equal if not in greater proportion than in the morning specimens; further, in the latter I am convinced that the oxalate existed in greater quantity, judging from the non-diminution in the specific gravity, as also from the appearance presented by the deposit under the microscope.

Third, The odour exhaled by oxalic urine is aromatic, occasionally approaching to that of the sweetbrier, noticed in urine containing the cystic oxide. I do not think that this peculiar odour is ever entirely absent; by gently heating the urine it was always greatly increased. In the pale urine, and especially in that possessing the greenish-yellow hue, it was always strongest. In the darker variety it was masked by the pungent urinous smell, denoting the existence of urea in excess.

Fourth, Urine containing oxalate of lime, I have found, when placed in favourable circumstances, less prone to decomposition than any other. I mean, of course, oxalic urine in which there existed no association of lithate of ammonia, no great excess of urea, and little phosphatic complication.

Fifth, As regards the quantity of urine secreted, there exists a difference of opinion. Dr Prout² has frequently, if not generally, seen it above the healthy standard. Dr Bird observes, that from actual measurement he has been unable to detect any decided increase.

In the few specimens of oxalic urine I have had an opportunity of examining, in which the urea was present in small quantity, the spe-

¹ Bird's Urinary Deposits, p. 129.

² Prout's Stomach and Renal Diseases, p. 64.

cific gravity low, and the urine of pale colour, there was a manifest and decided increase in the quantity secreted. As a general rule, the following conclusions in regard to the quantity of urine secreted, will I think be found correct:—1st, That the higher the specific gravity the greater will be the amount of urea present; and that, when this is great, the urine will be in natural quantity, or, at least, not greatly exceeding it. And, 2nd, That the lower the specific gravity is, the less will be the amount of urea present, and the secretion of urine will be considerably increased. In neither will there be found any material difference in the oxalic deposit.

Sixth, In all of the specimens, with the exceptions now to be noticed, which I have examined, the reaction was decidedly acid, frequently powerfully so. In a few there existed a tendency to neutrality. In all of these the oxalate was associated with phosphates; for a copious turbidity ensued on the addition of a few drops of nitric acid.

Seventh, An abundant deposit of epithelium almost invariably accompanies the oxalate of lime, varying considerably, however, in different cases, and at different periods in the same case. I have examined specimens of oxalic urine, almost, if not altogether, free from this admixture, which, in a few days thereafter, contained a large amount of the cells. In such instances, the number of the crystals was simultaneously increased. So common has Dr Bird found the presence of epithelium, that he has stated that its absence is the exception to the general rule; and the circumstance of epithelium existing in considerable quantity in the urine, has, he adds, frequently induced him to examine specimens of urine specially for the oxalate.

Eighth, The oxalate of lime has been described as in general diffused through the urine, and only falling to the bottom of the containing vessel when associated with a quantity of mucus, or a crystalline deposit of some other salt, as, for instance, of uric acid.

I believe, however, that even in specimens of urine in which, upon examination, crystals of no other salt can be detected, a deposit of the oxalate may be seen, and an almost positive opinion with regard to its nature be entertained. The peculiar transparency of oxalic urine, together with the colour alluded to, when seen once or twice, seldom fails to be again recognised.

I have repeatedly verified the truth of this observation. In cases where the oxalate does fall to the bottom of the jar, it is seen to form a small narrow filiform stratum; this, when shaken up, and the urine then exposed to a brilliant light, entirely disappears, and numberless minute shining points are seen scattered throughout.

Ninth, Such are the chief characters presented by this urine; but though, as has been previously observed, their occurring well marked is likely to lead to a suspicion of the presence of the crystals, that can only be properly determined by microscopic examination.

In doing so, I have invariably followed the method proposed by Dr Bird; that operation is undoubtedly by far the most satisfactory:

but the process may be rendered more simple, by merely placing a drop or two of the lowermost stratum of the urine under the microscope.

The crystals of oxalate of lime appear as beautifully formed transparent octahedra. Dr H. Bence Jones¹ informs us that Mons. Vigla was the first who described the octahedral crystals. Dr Jones has himself met with oxalate of lime in the form of cubes, and on one occasion found dodecahedral crystals; the former I have seen repeatedly, the latter never.

Other forms of the oxalate have been described. Dr Bird mentions that of dumb-bells, or rather of two kidneys with their concavities opposed. Of this variety I have seen three specimens from the first examination, associated with, and gradually entirely giving place to, the octahedral crystals.

Lately, an opportunity was afforded me of examining a specimen of urine in which the crystals assumed the form of exact single kidneys. At first I was unable to come to any conclusion in regard to their nature, and I should not finally have determined upon their being crystals of the salt in question, had I not, in rotating the glass in which the deposit was placed, brought into the field of the microscope, one, and afterwards three or four, of the dumb-bell crystals. The single kidneys presented no fractured edge or irregularities, as if they were formed by the mere disunion of the dumb-bells. There existed in the urine, besides the oxalate of lime, some amorphous urate of ammonia, a few crystals of uric acid, and a large deposit of epithelium.

The solubility of the single kidney crystals in nitric, and their insolubility in acetic acids, was determined. I have, since this examination, on several occasions again examined the urine of this patient, but have never seen the single kidneys, their place having been assumed by the more common octahedral variety. The dumb-bell crystals, Dr Bird believes to owe their form to a prolific arrangement of minute acicular crystals; this I have verified myself: and, for the single kidney-shaped crystals, I would claim a similarity of production, though I have allowed to escape the only certain method of determining this point.

The following Table, illustrating the colour, specific gravity, reaction, &c., of fifteen specimens of oxalic urine I have examined lately, exhibits also the complications observed.

Before concluding, I shall append an additional note on some other ingredients I have found in oxalic urine.

In three specimens of albuminous urine depending on the progress of the granular degeneration of the kidney, I have detected the crystals. In two of these they existed in large numbers. In one, the

¹ Medico-Chirurgical Transactions, Vol. XXVII. p. 146.

TABLE.

Age and Sex of Patient.	Colour of Urine.	Specific Gravity.	Reaction.	Amount of Urea.	Complications.
1. Male, æt. 43.	Dark amber.	1·028.	Acid.	In excess.	Phosphates deposited by heat, on first examination, afterwards urate of ammonia.
2. Male, æt. 40.	Dark amber.	1·028 to 1·040.	Powerfully acid.	In great excess.	Uric acid crystals.
3. Male, æt. 35.	Greenish yellow.	1·024.	Slightly acid.	In small quantity.	Mucus, and dense deposit of epithelium.
4. Male, æt. 32.	Amber.	1·030.	Acid.	Considerable.	Oxalate of lime crystals very large—a few of uric acid, epithelium.
5. Male, æt. 38.	Greenish yellow.	1·030.	Acid.	Large.	None.
6. Male, æt. 25.	Bright amber.	1·030.	Faintly acid.	Small.	Mucus, and a dense deposit of epithelium.
7. Male, æt. 38.	Greenish yellow.	1·030.	Acid.	Natural.	A few uric acid crystals.
8. Male, æt. 42.	Amber.	1·028.	Acid.	Natural.	Crystals of uric acid.
9. Female, æt. 38.	Greenish yellow.	1·030.	Acid.	In excess.	Pink urate of ammonia—a faint trace of albumen—a few blood corpuscles.
10. Male, æt. 35.	Amber.	1·028.	Acid.	Natural.	Crystals of uric acid.
11. Male, æt. 36.	Amber.	1·028.	Acid.	Natural.	Crystals of uric acid—deposit of epithelium.
12. Male, æt. 50.	Amber.	1·028.	Acid.	Increased.	Pink urate of ammonia, and crystals of uric acid.
13. Female, æt. 30.	Dark amber.	1·030.	Acid.	Natural.	Urate of ammonia—slightly phosphatic.
14. Female, æt. 22.	Natural.	1·022.	Acid.	Natural.	White urate of ammonia—a few uric acid crystals.
15. Male, æt. 19.	Amber.	1·032.	Powerfully acid.	Excess.	Urate of ammonia—epithelium.

albumen only manifested itself in the urine a short time previous to death, and when the crystals had greatly diminished both as regards number and size. The patient died of phthisis. In a second case, the crystals were in large numbers, and the albumen in large quantity, occurring in the urine of a man who also died of phthisis, complicated with degeneration of the kidneys and delirium tremens. This case and the preceding are interesting, as showing the occurrence of the oxalate of lime in phthysical patients; calculi of this salt Liebig has stated never occur in such persons. In the third and only other instance, the albumen was in small quantity, the crystals few but very large.

Spermatozoa and the spermatic globules have been detected in urine containing oxalate of lime. Professor Wolff of Bonn first pointed out this circumstance; and, according to him, oxalate of lime is a constant indication of their existence. This statement is quite opposed to the experience of Dr Bird, and, I believe, of all other observers.

With regard to the association of sugar and oxalate of lime, I may state the few following particulars.

I have only had an opportunity of examining three specimens of diabetic urine with the aid of the microscope, and in two of these oxalate of lime crystals were present. One of the specimens was obtained from a patient lately under Dr Christison's care in the clinical male ward of the Infirmary; and in his urine the crystals were, on first examination, found in considerable numbers. This man left the hospital with his disease somewhat relieved, and his general health improved. I examined his urine again shortly before his dismissal, and detected the crystals; but they were fewer in number, and of greatly reduced dimensions.

The other case, in the urine of which they occurred, was that of a girl, aged three years, of unhealthy appearance. The fact of the urine being passed in very large quantity, was one of the first symptoms which alarmed the friends, and the very first the knowledge of which they put me in possession of.

The urine I examined with great care. It was of a pale colour, of acid reaction; sp. gr. 1.030. Urea present, but in small quantity. By Moore's test the presence of sugar was detected, and, under the microscope, a few of the torulæ or fungoid vegetations so characteristic of diabetic urine, as also a few minute crystals of oxalate of lime. It is interesting, in connexion with this case, that the father of the child presented well-marked symptoms of the oxalic diathesis, verified by examination of his urine.

One word in regard to the presence of the crystals in the urine in special diseases.

I have found them in the urine in two cases of acute rheumatism, in both of which the heart subsequently became affected; in a case of bronchitis, and also in one of pneumonia, the urine of highly in-

flammatory type, scanty, dark in colour, high specific gravity, strongly acid reaction.

In one or two cases of typhus with intestinal complication; finally, in a case of scorbutus, and in two cases of phthisis—these last have been previously noticed.



ARTICLE III.—*Contributions to Acoustic Pathology.* By JAMES MERCER, M.D., F.R.C.S.E., Lecturer on Anatomy and Physiology, and on Diseases of the Ear, Edinburgh.

PART III.—*On the Pathological Sequences of Acute Inflammation of the Fibro-mucous structures of the Cavity of the Tympanum.*—
TYMPANITIS—MYRINGITIS.

THE inflammation that attacks the fibro-mucous structures of the cavity of the tympanum, may be either of a simple or isolated form, as when the membrana tympani is alone involved, or when it engages to a limited degree the membrane, and, more or less, the general investing membrane of the cavity; or it may be of an extremely complicated nature, more severe in its symptoms, and more fatal in its pathological sequences and results, in consequence of the numerous complications which this form of disease has always with all the deeper-seated parts of the organ.

The name of "*myringitis*" has recently been applied to this very complicated and dangerous form of disease of the middle ear, by Mr R. A. Wilde, an experienced and scientific practical aurist of Dublin. In a paper published in the November Number of 1847, of *The Dublin Quarterly Journal of Medicine*, this author has given the profession a most admirable and graphic description of the history, symptoms, and treatment of this formidable disease; and to this able production I would refer the readers of this paper, for the truthfulness of its description of the history, progress, symptoms, and treatment of it, both in the acute and chronic forms. It would be a mere repetition on my part, were I to attempt to add any thing to the above-mentioned production, as it is complete in itself; and, as I have stated at the commencement of this paper, I shall confine myself strictly to the pathological sequences resulting from this form of disease, myringitis. In this, I do not wish to detract any thing from the credit due to Mr Wilde; but, as a fellow-labourer in the same path in which he treads, I need only remind him, that "if we would treat of a science systematically and profitably, it is above all things necessary only to isolate it."—(*Feuchtersleben, Medical Psychology.*)

The pathological sequences which results from myringitis are very numerous, and by far the greater proportion of them are commonly fatal. I have endeavoured to arrange these, as simply and connectedly as the history, progress, and relative terminations of them

enabled me to do; and of these I would enumerate the following list, which I have been able to glean from the records of science, or have seen in my own experience.

SECTION I.—Caries of the Parietes of the Tympanum, producing Meningitis, without Destruction of the Petrous Portion of the Temporal Bone.

SECTION II.—Caries of the Parietes of the Tympanum, producing Meningitis or Cerebritis, in consequence of destruction of the Osseous Septum between its Cavity and that of the Cranium.

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I.—*Caries of the Parietes of the Tympanum, producing Meningitis, without Destruction of the Petrous Portion of the Temporal Bone.*

The pathological connexion between the existence of diseases of the middle ear, and those of the membranes and substance of the brain, was for a long period unnoticed, and little attention was therefore paid to them; and it was not until within the last thirty years that special attention was directed to them, and their essential importance distinctly pointed out. The merit of the first improvement in this department of medicine is undoubtedly due to the late Dr John Abercrombie, who, so early as 1821, directed the attention of the profession to the frequent occurrence of disease of the middle ear, as not only existing with, but generally preceding, inflammation of the dura mater, or the immediate investing membranes, or the substance of the brain. But even at this early period, Dr Abercrombie did not press this important fact so forcibly on the profession as his subsequent experience enabled him to do. For in his case of meningitis of the cerebellum (case 15th, Ed. 2d, 1829), that proved fatal in 1821, the patient had laboured under all those symptoms which usually attend, and are characteristic of acute myringitis, and had also a discharge of purulent matter from the left ear very early in the disease. On inspection of the brain, it was found all

healthy but the left lobe of the cerebellum. There, on its outer surface, was formed a uniform deposit of thick puriform matter, most abundant on the left side. The pia mater of the cerebellum was highly vascular; the dura mater was healthy; there was some purulent matter about the pituitary gland, and in the cavity of the middle ear, but *there was no appearance of disease of the bones connected with the ear, or of the dura mater covering them.*

This case we look upon as the most simple form—considering the pathological results—of the more extensive ravages which accompany, and are produced by acute myringitis. It is well known to all practical aurists who treat diseases of the ear on the principles of histological pathology, that in every case of acute myringitis considerable morbid changes always result to the parietes of the cavity of the middle ear, and no discharge of pus can take place from this cavity until the integrity of the membrana tympani becomes destroyed. It is unfortunate that no special account of the actual state of the parietes of the cavity of the middle ear has been recorded in the above quoted case, further than that “there was some purulent matter in the ear.” Had the parietes of the cavity been more carefully examined, a greater extent of disease might have been detected. It is also well known, that if a person has once suffered from acute myringitis, and that this has been more or less successively relieved, that so long as any purulent discharge takes place from the external ear, the disease still exists in a chronic form; but if the patient becomes exposed to the influence of those agencies capable of reproducing the disease, it usually returns with all the force of an original attack.

I am inclined, therefore, to view this case as one of myringitis, and it is also further interesting, in showing that disease of the membranes, or the substance of the brain, may result from diseases of the cavity of the middle ear, and without any destruction of the petrous portion of the temporal bone.

II.—*Caries of the Parietes of the Tympanum, producing Meningitis or Cerebritis, in consequence of destruction of the Osseous Septum between its Cavity and that of the Cranium.*

This is of more frequent occurrence than the former variety of cases, and is generally a very deceitful and insidious, but a most dangerous affection. It commences with all the symptoms of simple inflammation of the membrana tympani, or those of its more complicated form, myringitis; and many so affected consider it for a time as a trifling ear-ache. If a discharge of matter takes place from the ear, it is expected that the pain will be relieved; but, on the contrary, it becomes more and more violent. The general course of such cases is, that the patient becomes drowsy and oppressed, delirium supervenes, shiverings, singultus and subsultus tendinum, and ultimately complete coma.

It is, also, not uncommonly found to occur in cases where acrid lotions have been employed to check suddenly the purulent discharge from the cavity of the tympanum, without any other counter-irritation having been adopted to prevent the occurrence of inflammation of the brain. In these cases the patient, after complaining for a day or two of having had deep-seated and very acute pain, especially during the night, in the ear, and along the face or side of the neck, suddenly becomes restless and forgetful—lies rolling his head from side to side, or tossing about his arms, and in a short time sinks into coma.

In both of these forms, the petrous portion of the temporal bone will be found to be more or less destroyed; and, as an illustration of the general course and termination of this form of disease, I shall quote the following case from Dr Abercrombie:¹—

“A gentleman, æt. twenty, on the 20th January 1820, complained of violent toothach seated in a tooth in the right side of the upper jaw. On the 21st the pain extended into the ear, without any other symptom. On the 22d the pain continued in the ear, and extended towards the temple. He lay in bed part of the day, but got up afterwards. Leeches were applied, and he took some laxative medicine, which he vomited, and he had afterwards repeated vomiting. On the 23d the pain was more general over the head and across the forehead, with some vomitings, and at night shiverings. During the night he became incoherent and delirious; he was then seen by a surgeon, who found him very incoherent, but complaining of severe headach; the pulse 70, moderate in strength. Dr Abercrombie saw him on the 24th; his pulse was then 60; his face rather pale; the headach continued, and was chiefly referred to the forehead; his look was vacant; he answered questions distinctly when he was roused, but talked incoherently when his attention was not kept up. He was now treated by general bleeding, which he bore well; cold applications, blistering, and purging. On the evening of the 24th there was considerable shivering. On the 25th, there was less complaint, but more incoherence, and a tendency to stupor; pulse 60–70. On the 26th, pulse 100 to 120. On the 27th and 28th little change; answered questions when roused, but, when not spoken to, lay in an oppressed state, or talking incoherently; pulse 96 to 120; some slight, but fetid, discharge from the ear. On 29th, constant incoherent talking; pulse 96, of good strength; *‘the right eye was suffused, the ball of it appeared turgid and enlarged, and the cornea was covered with a yellow slough.’* *‘In the course of this day the mouth was, at times, observed to be drawn to the left side, especially when he was drinking.’* At night he began to sink, and died in the morning of the 30th.

“*Inspection of the Head.*—There was some effusion under the arachnoid on both hemispheres; much effusion into the ventricles, and extensive ramollissement of the septum lucidum, the fornix, and the cerebral matter bordering on the lateral ventricles. There was extensive caries of the right temporal bone; behind the ear, on the thin part of the bone, it was very dark-coloured; and the petrous portion of the bone was dark-coloured, very soft; and when cut into discharged matter from its cancelli, and from the cavity of the middle ear. The dura mater corresponding to the temporal bone was much thickened. The part of it which lay anterior to the petrous portion was in a state of recent inflammation; the part behind the petrous portion was much thickened and spongy; and, between it and the bone, there was a deposit of thick purulent matter. From this place the disease had spread along the tentorium cerebelli, and nearly over the whole surface of the cerebellum, on almost every part of which there was a

¹ Diseases of the Brain, Case VI. p. 34. Ed. 1829.

deposit of coagulable lymph, with thick, flocculent, purulent matter; this was most abundant on the tentorium, and the right side and posterior parts of the cerebellum, and it was traced into the fourth ventricle. Under the cerebellum there was a considerable quantity of pus, and in its substance there was a small abscess, in the posterior part, between the lobes."

Such are the extensive ravages of this truly frightful disease; and the two peculiar symptoms mentioned in the case, "twisting of the mouth," no notice was taken of it in the above report of the *post-mortem* examination. This will be more fully alluded to in a subsequent section of this paper, in reference to those cases of destruction of the entire petrous portion of the temporal bone, causing thereby destruction of the facial nerve in the aqueductus Fallopii; as also the "peculiar symptoms shown in the right eyeball," in connexion with the same destruction of bone, injuring the Gasserian ganglion, which lies upon its cranial surface.

III.—*Caries of the Parietes of the Tympanum, inducing Phlebitis of the Lateral Sinus and Internal Jugular Vein.*

This is another, and by no means an unfrequent, termination of complicated acute tympanitis, myringitis. In this class of cases, the osseous posterior septum of the mastoid cells gives way, and, immediately on its occurrence, the dura matter covering the point of diseased bone becomes diseased, presenting all the symptoms of meningitis. From the proximity of the sigmoid curve of the lateral sinus along the cranial surface of the mastoid cells, the lining membrane of the vein becomes speedily inflamed, and, extending rapidly along it to the heart, forms a fatal phlebitis of the internal jugular vein.

The following case, reported in the Reports of the Dublin Pathological Society, Vol. XIX., is one of the most interesting examples of this form of disease in the records of medicine.

"A boy, æt. sixteen years, entered the Hardwicke Hospital in Dublin, May 27, 1840, under the care of Mr R. W. Smith. He had been exposed to the greatest hardships and laborious exertions from his earliest youth. He had been ill for seven days previously to his entrance into the hospital; he complained of shiverings, and a cold, creeping sensation, succeeded by intense pain in the right ear and right side of the face. He had nausea and vomiting, with loss of appetite; he was constantly drowsy, and prevented from sleeping by a loud noise in his ear.

"After remaining under medical treatment for a short time, he left the hospital and resumed his work; but was soon obliged to discontinue it from the debility and occasional syncope with which he was overpowered. When he was again admitted he could not walk steadily; he had no spasmodic or irregular action of the muscles, but he staggered from vertigo; he was thin and pale, and had a vacant stare, with large and equally dilated pupils; his answers to questions were slowly but rationally given; he complained of severe shooting pains through the back part of his head into the right ear, from which flowed a greenish, fetid matter; his tongue was white and moist; his pulse 132, sharp and small; and his skin was hot.

"He grew rapidly worse after his admission; he slept but little, started frequently from his sleep, moaning from the acute pain in his right ear; whenever

he attempted to rise he supported his head with his hands, and was sensible of a noise in his head like the splashing of water; there was a sense of fluctuation and great tenderness over the mastoid process; a teaspoonful of fetid pus was given exit by incision, and the bone was found denuded of its periosteum; he had great epigastric tenderness and ardent thirst.

"Upon the 3d of June he had a jaundiced hue, and an attack of diarrhœa with tenesmus; he had also a distressing cough, and severe pain along the right side of the neck. Upon the 6th symptoms of arachnitis set in; violent, darting pain in the head; alternations of heats and chills; a rapid pulse; delirium; dilated and irregular pupils; vomiting; occasional singultus; he was restless; burning heat of scalp, and, cold extremities; he soon became comatose, ceased to answer questions rationally, and died June 11.

"*Examination of the Head.*—The brain was firm; the left hemisphere pale; the right highly vascular in the interior, and the membrane covering it was minutely injected with blood, especially along its inferior surface. Three small purulent deposits, surrounded by a vascular circle, and apparently encysted, were found at the inferior surface of the right lobe of the cerebellum, where it corresponded to the lateral sinus. The dura mater was separated by pus and lymph of a green colour from the anterior surface of the petrous portion of the temporal bone; but there was no perforation of the membrane. Over that portion of bone which constitutes the superior wall of the tympanum, it was elevated into a small tumour by a collection of fetid matter, and presented a sloughy aspect. The portion of bone corresponding to this abscess, of a circular form, from about one-fourth of an inch in diameter, was dead and of a dull white colour. The process of separation from the living bone was far advanced, and at one point of its origin the separation was complete, and the aperture thus formed communicated with the cavity of the tympanum; the remainder of the petrous portion was remarkable for its vascularity; the membrana tympani had disappeared completely, and the membranous walls of the right lateral sinus, throughout the whole of the mastoid portion of its course, were much thickened, and the lining membrane of the vessel presented a sloughy appearance, being covered with lymph of a greenish hue, and smeared with unhealthy purulent matter. This condition extended along the internal jugular vein and superior vena cava, to within a short distance of the entrance of the latter vessel with the right auricle. The lining membrane of the vena cava was of a dead tawny colour."

In connexion with this division of our subject, I will also quote the following case, as reported by Professor Syme in the March number of the *Monthly Journal of Medical Science*, 1841, p. 153, wherein the carotid artery was tied for hemorrhage from the external ear, and similar in its pathological cause to the above-mentioned case.

"In the spring of last year, Dr James Wood asked Mr Syme to see a young gentleman, eleven years of age, on account of an alarming hemorrhage from his ear. He was recovering from an attack of scarlatina, in consequence of which both ears had suppurated, when, upon the fifteenth day, a large quantity of blood was suddenly discharged from the right side. During the six succeeding days the bleeding returned three times, to the extent, by computation, of a pound on each occasion. It was deemed proper to place a ligature on the carotid artery, which was concluded to be the source of the hemorrhage. Bleeding recurred while the operation was being performed, and twice again to a small extent, not exceeding a few teaspoonfuls, in the course of the following evening and night.

"For several days afterwards, there was hardly any appearance of blood, and all the circumstances encouraged the entertainment of favourable hopes. Symptoms of cerebral excitement, however, then showed themselves, and terminated fatally on the eleventh day after the operation.

"On examination, it was found that the carotid artery was *not* concerned in the

disease, but that a small ulcerated aperture in the osseous septum, between the termination of the lateral sinus and the cavity of the ear, had permitted the blood to escape from this vessel.

"*Could this have been ascertained previously*, stuffing the ear would, of course, have suggested itself as the proper practice."

There is a foot-note in connexion with the above reported case, in which Professor Syme refers to "a case of bleeding from the ear, in which recovery followed this operation" (tying the carotid artery). I have consulted this case, and find that it has no pathological relation to the present section of cases. That case was, evidently, one of perforation of the internal carotid, before it had entered the canal in the petrous portion of the temporal bone, as the principal part of the blood that was discharged came from the back part of the pharynx. The *possible* symptom that was exhibited, *bleeding from the ear*, and leading to the supposition of perforation of the vessel, is distinctly shown from the results of the case to have been accidental. The blood that had been discharged into the pharynx would have been partly swallowed, and, during the primary effort at deglutition, the influence of the superior constrictor of the pharynx would carry the blood, also, into the pharyngeal opening of the Eustachian tube, and thence by it to the tympanum, where the membrana tympani, having been destroyed, it was discharged from the external ear.

It is unfortunate that, in the narration of the above case of Professor Syme's, no notice was taken of the *physical properties* of the blood discharged. In cases belonging to the present section, where the symptoms are so doubtful and so deceitful, every trifling circumstance should be taken into consideration before a positive diagnosis is formed or acted on.

IV.—*Caries of the Parietes of the Tympanum; Necrosis of the Petrous Portion of the Temporal Bone; Destruction of the Portio Dura in the Aqueductus Fallopii, producing Paralysis of the Muscles of the Face.*

This form of complication with myringitis is of comparative rarity, and with the exception of two cases, accidentally mentioned by Dr Abercrombie, one of which we referred to in the second section of the present paper, there is only another complete case on record, and reported by Dr R. Graves, in the Dublin Journal, Vol. XX. I have met with one case also in my own experience; but it was complicated with loss of sensation (anæsthesia) of the face, and which I will notice in the next section.

The case of Dr Graves is as follows:—

"A boy, about ten years of age, was admitted into the Meath Hospital labouring under general dropsy; he appeared of a scrofulous habit, and was much worn down by long-continued diarrhœa. Under appropriate treatment, his symptoms gradually, though slowly, disappeared, and he was restored to comparative health. We now observed that the right side of the face was paralysed, and on examina-

tion found that he had been subject to a discharge from the right ear for seven years previously. The paralysed cheek presented the phenomena usually observed in Bell's Paralysis. He was attacked soon after with acute pain in the ear, and in the left side of the head. A fortnight after, convulsions set in; the pain moved from the side to the back of the head, then to the back of the neck, and ultimately extended the whole way down the spine, and about this period the diarrhœa diminished. A few days before his death he was attacked with spasms resembling those of tetanus, and the surface of the body became exquisitely tender to the touch. He never had any loss of motion, and to the last his intellect was perfect. From the period when the pain set in to that of his death, the convulsions returned six times.

"*Post-Mortem.*—The portio dura was dissected on the face, and found healthy; the nerve was also healthy from its origin at the base of the brain to the entrance into the meatus auditorius internus. Immediately above this opening the dura mater was of a greenish colour, detached from the bone as if by fluid, and perforated by a round hole, large enough to admit one small crow-quill. On dividing this part of the membrane, the space between it and the bone was occupied by a thick, greenish offensive pus, and the opening in the dura mater was observed to be opposite to the foramen in the petrous portion of the temporal bones, called the *aqueductus vestibuli*. This opening was much enlarged, and the bone of it was in a carious condition.

"The nerves at the base of the brain were bathed in this thick green pus, but the organ itself was every where healthy, and free from excess of vascularity. The arachnoid was thickened and opaque, and the pia mater not more injected than natural. The ventricles were not distended. The theca vertebralis was much distended by the same kind of matter, which flowed abundantly from any accidental puncture of the membrane. The matter was contained in the sac of the arachnoid, which membrane was quite healthy, and presented its usual glistening appearance; no thickening or opacity in any part of its extent. The pia mater was also free from disease; all the attachments of the ligamentum dentatum remained unbroken. The spinal chord, on being slit up, presented no trace of disease. The roots of all the spinal nerves from the base of the brain were bathed in pus, the presence of which fluid on the surface of the brain and spinal chord, had no doubt irritated those organs, and occasioned the tetanic symptoms and the cutaneous tenderness. The portio dura was traced through the aqueductus Fallopii, about a quarter of an inch from its entrance; the nerve was completely cut through, and the petrous portion of the bone was extensively destroyed, and presented a mere shell. The membrana tympani, and all the internal ear, were completely destroyed."

It may be further mentioned here, that the spot where the portio dura was cut through, corresponds exactly to the point where the great petrosal, or vidian nerve, joins the portio dura, and forms the *intumescencia gangliiformis*.

I shall now proceed to consider the fifth section of cases, which, when they do occur in practice, are usually complicated with those of the fourth; viz. paralysis of sensation in one half of the face—*anæsthesia*.

V.—*Caries of the Parietes of the Tympanum; Necrosis of the Petrous Portion of the Temporal Bone; Destruction of the Gasserian Ganglion, producing Paralysis of Sensation in one half of the Face.*

When we find paralysis and distortion of the face, with loss of sensation of the parts, we have reason to suspect disease within the

head, even without the existence of any active morbid action in the cavity of the ear. These cases have been referred to by the late Dr Abercrombie in his section on diseases of the nerves; but he has not favoured us with any cases of anæsthesia of the face, produced by the previous existence of myringitis. His cases, however, are of great importance, and relate entirely to those of paralysis and anæsthesia consequent on some morbid state of the membranes surrounding the exit of the nerves from the cranial cavity in the substance of the brain, at their points of origin or emanation, or in some part of their course for distribution.

The symptoms of such cases are from those of the special case in connexion with myringitis, which I shall relate in every respect similar to those described by Dr Abercrombie. The case is as follows:—

“A young girl, seven years of age, and of a strumous habit of body, became affected with scarlatina anginosa in the summer of 1843. She was the daughter of a travelling gipsy, and resided in a wretched hovel in one of the filthiest alleys in the south side of the town. I was called to see her in the course of one of my dispensary visits. It was on the sixth day of attack when I first saw her. The cutaneous eruption, which had evidently been very dark, was almost gone; there was great difficulty in breathing, a hoarse voice, sneezing, cough without expectoration, and an occasional slight hemorrhage from the nose. The surface of the tongue, and insides of the cheeks, were covered with numerous aphthæ; the tonsils were much swelled, but there was no evidence of decided gangrene, though there was considerable superficial ulceration on both sides. The child was delirious, and had been so for twenty hours, screaming wildly, and instinctively putting her hands to her right ear, the right side of her face, and neck. When she was coherent, she complained to her mother of a severe pain coming on in these parts, and, when I attempted to examine her ear, she instinctively indicated severe agony, and tried to thrust away my hand. A discharge of matter had taken place from the right ear four hours before I saw her; but the symptoms showed no relief. On examining the mastoid process it was larger than usual, discoloured, and had a slight feeling of softening and pitting. An incision made into it gave exit to a full teaspoonful of very fetid pus; but none of the small bones, or any gritty particles, could then be found in that discharged matter, or in that coming from the outer ear. A large warm linseed meal poultice was applied to the right ear and side of the face; two grains of calomel, and three grains of Dover's powder, were ordered to be given every four hours, and, in the intervals, a teaspoonful of weak wine and water.

“On the morning of the second day there had been a decided increase of all the cerebral symptoms; the wine and water had been swallowed with difficulty, and part of it ejected again. A small enema of *Ol. Terebinth* and gruel, that had been exhibited the previous night, had operated well in emptying the bowels. The discharge still continued, both from the outer ear and the incision in the mastoid process, and, on examining the concha, I found the malleus and incus bones, with the stapes attached to the latter, there amongst the discharge. Several gritty pieces of bone were also picked out from that of the mastoid process; and I fully concluded that complete destruction of the ear bulb had taken place, and that necrosis of the petrous portion of the bone would follow. No palsy of the muscles of the face as yet; but difficulty in swallowing. The eyeball appeared larger than before, and had a dull look. A feather gently rubbed upon it still gave sensation, by a sluggish twinkling of the eyelids. Continued the medicines.

“At six P.M. that day, I again called, and found the cerebral symptoms the same. There was more incoherence, and extreme restlessness; she tossed about her hands and legs, and, whilst I was present, she had a short convulsion. There was now

distinct paralysis of the muscles of the face; greater difficulty in swallowing; the eyeball appeared still larger, and seemed to be starting from the orbit. It had become deeply congested, and was quite insensible to the irritation of the feather. The skin of the right side of the face might be pierced or pricked, but no sensation was evinced. The inside of the same cheek was in a similar state. I rubbed a little strong salt along the inside of the right cheek, and along the right side of the tongue, but no evidence of any sapid body being there was shown; and a similar result followed the giving of a little powdered colocynth. On the left side of the face, however, there was distinctive evidence of sensibility remaining both to pricking, salt, and colocynth; and the eyeball there was also fully sensitive, and apparently healthy. There was a slight fetid and bloody discharge from the right nostril. On examining the aperture of the mastoid, I found a spongy-looking mass of bone impacted in the incision there. This I carefully removed by a slight enlargement of the opening. (the mastoid bone was very soft and easily cut), and removed a great part of the mass of the petrous portion of the temporal bone. I bathed, then, the ear very gently with a sponge saturated with tepid water; gave her a little pure wine, and ordered a beef-tea enema. All the symptoms, as I left, were gradually increasing in severity.

"On washing carefully this necrosed portion of bone, I found it still to possess the conformation of the natural bone; its substance, however, was converted into a spongy mass, and the osseous labyrinth of the ear-bulb formed but a general part of the cancellated structure of it. Early on the third morning I found that, shortly after I had left, the convulsions came on with great frequency and violence; shiverings repeatedly; singultus, and ultimately coma, and death about four o'clock A.M. A dissection was granted.

"*Post-mortem appearances.*—To be careful in our examination, we succeeded in securing the entire head, stuffing up its place neatly, and leaving it apparently entire. On removing the calvarium and the dura mater corresponding to it, we found but a trifling sub-arachnoid effusion of opalescent lymph. No serum in the sac of the arachnoid there, but some congestion of the vessels of the pia mater on the upper surfaces of both hemispheres of the cerebrum. On slicing off these, there were a few bloody points here and there, similar to those found in cases of simple congestion of the veins of the cerebral substance. The lateral ventricles contained about two drachms of serum, and the septum lucidum and fornix were much softened. The choroid plexuses were much congested. On removing the entire nervous mass, we found the dura mater covering the upper surface of the petrous portion of the temporal bone very much diseased; it was elevated, soft, and spongy, of a dullish colour, and apparently on the point of becoming gangrenous. No distinct aperture was found in it, and it was raised up solely in consequence of the cavity from which the necrosed bone had been discharged, that cavity being completely filled with pus, and, floating on its surface, we found the Gasserian ganglion in a state of perfect destruction. The facial nerve was also found destroyed at its entrance into the aqueductus Fallopii, and was found so until the lower part of the stylo-mastoid canal. The whole of the osseous labyrinth had been destroyed and discharged; the osseous portion of the Eustachian tube that opens into the cavity of the tympanum was entire, but evidently diseased, and the internal carotid artery was not affected. Had the diseased action but continued for a few hours longer, the septum between this vessel and the tympanum would have been destroyed, and the vessel would have been opened. None of the tympanic muscles, vessels, or nerves, could be found; the osseous septum between the cavity and the sigmoid groove for the lateral sinus was entire, and no effects had been produced in the jugular vein.

"The inferior surface of the right middle lobe of the cerebrum, that lay upon the affected temporal bone, was highly inflamed, and much softened; there was a considerable effusion of lymph at the inner extremity of the right fissure of Sylvius, around the chiasm of the optic nerves, the tuber cinereum, the corpora albicantia, and the locus perforatus posterior, placed between the crura cerebri. The vascularity extended along the right crus cerebri to the mesocephalon, and

thence, by the right crus cerebelli, to its right hemisphere. To all these parts the lymphic effusion was chiefly confined, and there was also some fluid in the cerebellar fossæ, the greater part of which had escaped by the removal of the head. The eyeball had not gone on to complete disorganization; but every part of its interior structures showed distinctive evidence that it was far advanced in a state of gangrene. The vitreous body, and all within the iris, were converted into one confused mass.

"On dissecting the right nasal fossa and the pharynx, I found the Schneiderian membrane there in a state of extensive ulceration, not only in the general cavity, but also in all the facial cavities. The tonsils and side of the pharynx were also ulcerated; but the pharyngeal opening of the Eustachian tube, though also much ulcerated, was considerably entire. The left side was also much affected, but does not deserve a special description."

Such were the appearances seen in this interesting case; and I shall only add a few remarks in reference to its importance. It was remarked by Dr Abercrombie (p. 447), *loc. cit.*, "that a remarkable circumstance connected with the affections of the fifth nerve, is the tendency to inflammation and sloughing in parts which have lost their sensibility—particularly in the eye." Dr Abercrombie relates a case that occurred to Dr Alison, in which these results on the eyeball were very distinct; but the pathological cause did not belong to the present class of cases, as it was consequent on diseases between the Gasserian ganglion and the origin of the nerve at the mesocephalon.

VI.—*Caries of the Parietes of the Tympanum; Necrosis of the Petrous Portion of the Temporal Bone; Opening of the Internal Carotid Artery in its Canal of the Temporal Bone, either alone, or in conjunction with the Lateral Sinus, or the Destruction of the Gasserian Ganglion or the Facial Nerves.*

From the pathological sequences which I have shown as resulting from the ravages of complicated acute tympanitis, it will be easily understood that the above section of cases can easily form one of their number. The situation of the internal carotid in the canal of the petrous portion of the temporal bone, is not so secure in the nature of its position, or in the thickness of its osseous defences, as not to warn us that, some time or other, it will share alike in its destruction, as a sequence of myringitis, similar to what has so frequently occurred to the lateral sinus, and to the fifth and seventh pairs of nerves. There are several vulnerable points in the course of the artery in the canal of the bone, and the wonder is, that not one single case of its destruction has been put on record, so far as I can find; but that it is just as liable to destruction as any of the others are, is our decided conviction.

As I cannot present a single complete case to the profession in reference to this section, I must now conclude my remarks on this subject, by trusting that some more favoured observer will yet meet with such a case, and thus complete more fully the melancholy list of sequences that may follow acute tympanitis.

ARTICLE IV.—*Contributions to Pathology and Rational Medicine.*

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No. XIII.—CLINICAL AND HISTOLOGICAL RESEARCHES ON CANCEROUS AND CANCROID GROWTHS.—(*Continued.*)

OBSERVATION XXI.—*Cancroid Tumour attached to the Tentorium pressing upon the Cerebellum—Inflammatory softening of Cerebellum—Chronic Tubercular Cavern in left lung, with Vegetations—Death.*

MARGARET BOYNE, æt. forty, admitted into the Royal Infirmary, March 3, 1847, under Dr Andrew. She has never enjoyed good health, on account of stomachic derangement more or less severe. Latterly she has become much emaciated, and been subject to fits, said to be hysterical. On admission, there is extreme emaciation, and severe headach occurring at irregular intervals, but more acute towards evening and during the night. She frequently utters loud screams, and, on awakening from sleep, appears quite bewildered. The memory is much impaired. There is occasional vomiting, with pains in the stomach. The bowels are constipated. The menstrual discharge has been absent for some time. No fever; pulse 100.—*March 12*, She was observed to-day to squint with both eyes, and says that she sees double. The pain in the head is much increased, and the memory greatly impaired.—*March 13*, The articulation is defective.—*March 14*, Cannot be induced to speak; moans constantly. The strabismus continues, and she now passes urine involuntarily. There is no paralysis.—*March 15*, Comatose, sunk gradually, and expired in the evening.

Sectio Cadaveris, March 17, 1847. Body greatly emaciated.

Head.—The membranes of the brain were unusually dry, but otherwise healthy. Substance of the cerebral lobes healthy. From the two lateral ventricles nine drachms of clear serum were collected. On removing the base of the brain, it was discovered that the left lobe of the cerebellum was unusually soft, and pressed upon by a tumour firmly adherent to the inferior surface of the tentorium. The tumour was externally soft, the size of a hen's egg, and imbedded in the left lobe of the cerebellum. The substance of the cerebellum immediately in contact with it, and to the depth of a quarter of an inch, was softened, and of a light yellow fawn colour. This lesion gradually terminated in the healthy substance of the cerebellum. Leaving the tumour within the cranium, the cerebellum was now removed: on being subjected to a stream of water, it presented a deep cup-shaped cavity, corresponding to the size of the tumour, and a portion of its softened substance which had been washed away. The tumour was externally soft and pulpy, but on section was found to increase in density towards the centre. The nucleus, which was about the size of a walnut, was exceedingly tough under the knife, and its section displayed a smooth white surface, slightly tinged of a pale yellow. It was firmly adherent to the tentorium by a broad base.

Chest.—The pleuræ on the left side were strongly adherent over two superior thirds of the chest. It was found impossible to remove the lung without breaking into a chronic cavity in its apex the size of a hen's egg. The whole of the superior lobe was much condensed, indurated to the feel, and contained numerous cavities, varying in size from a hazel-nut to the large one in the apex just mentioned. Their walls were hard and rigid, of a mahogany brown colour, lined by a distinct membrane, free from pus or recent tubercle, and studded here and there with calcareous concretions. The large cavern at the apex contained a brown, corrugated, inspissated matter, of cheesy consistence and friable, covered all over with a white mould. The inferior lobe of this lung was healthy. The

pleuræ on the right side were also strongly adherent at the apex, over a space the size of a five-shilling piece. Here also was a chronic cavern the size of a nutmeg—its walls containing several calcareous concretions, and its cavity a quantity of friable cretaceous matter. It was surrounded by considerable puckering, and numerous obliterated bronchi composed of fibrous tissue arranged in a stellate manner around it.

The abdominal organs were healthy, with the exception of a few small tubercular deposits in the glands in front of the ileo-cæcal valve.

Microscopic Examination.—On examining a thin section removed from the centre of the tubercle, it presented a dense amorphous mass. On adding water and breaking it up somewhat, it was found to consist of numerous tubercle corpuscles, innumerable molecules and granules, a few fusiform corpuscles, and globules with double lines, closely aggregated together (Fig. 56). The softening in the cerebellum was composed of fragments of cylindrical and varicose tubes, mingled with numerous molecules, granules, and compound granular corpuscles and masses (Fig. 57).

The mould in the pulmonary cavern consisted of beautiful cryptogamic formations, the description of which in this place is unnecessary.

Fig. 56.

Fig. 57.

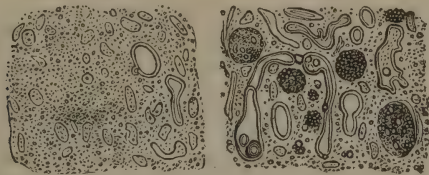


Fig. 56. Structure of the centre of tumour described Obs. XXI. composed of molecules, granules, and tubercle corpuscles, associated with a few fragments of nerve tubes. Fig. 57. External and more softened portion of the tumour, containing a larger number of fragments of the nerve tubes, with numerous compound granular corpuscles and masses.

Remarks.—This singular case admits of many observations having reference to the brain and lungs—which, however, are not in any way connected with the present inquiry. The position of the tumour, attached to the under surface of the tentorium, and exerting its pressure and disorganizing powers directly downwards on the lobe of the cerebellum, instead of the pons varolii, explains the absence of paralysis in the extremities. On the other hand, the strabismus may have been induced by the collection of fluid in the ventricles, which might also, by means of pressure on the cerebral lobes, be connected with the loss of memory and other cerebral derangements. On this subject, however, I need not dwell. The lesions of the lungs exhibit an unequivocal proof of tubercular caverns in progress of cure.

The tumour contained none of the cells we have seen so constant in cancer, but was composed of an agglomeration of imperfectly formed bodies, usually denominated tubercle corpuscles. From this circumstance, as much as from the occurrence of old tubercular caverns in the lungs, we can have little hesitation in considering its nature to be tuberculous instead of cancerous, as it was first supposed to be by all who saw it. Many such growths in the brain have doubtless been recorded as cancer of that organ.

OBSERVATION XXII.—*Cancroid Tumour attached to the Tentorium—Inflammatory softening of the Right Corpus Striatum and Optic Thalamus—Death.*¹

Mrs M., æt. at the period of her death seventy-eight, had always enjoyed good health until the 1st of October 1845, when she was found lying on the floor comatose, although perfectly sensible and well a few minutes previously. Complete stupor continued for many hours, with stertorous breathing, and when consciousness returned the left side of the body was found to be paralysed. In the course of three weeks the paralytic arm began to stiffen, and this went on gradually increasing; so that, in the course of a few months, the arm was strongly contracted, and the fingers bent and fixed like a bird's claw. During this period intelligence partially returned, and she was able to recognise friends, and answer a few simple and direct questions. She talked much, however, incoherently, and laboured under numerous hallucinations. Her manner was now very childish. She had frequent fits of crying and irritability, and now and then fell into a stupor, marked by lividity of countenance and a quick pulse. This condition passed off in a few hours, after a stimulant was exhibited. The left leg underwent the same gradual progress of stiffening as the upper extremity, but not to so great an extent, although it was the seat of some pain, and she was constantly entreating that it should be rubbed. About six months prior to death, she became worse in every respect, had very short intervals of partial consciousness, was extremely irritable, uttered piercing cries, had involuntary evacuations, and became dropsical, with sloughing over the sacrum. She died on the 11th of March 1847.

Sectio Cadaveris, March 13, 1847. The head only was examined.

The membranes and external surface of the brain were healthy. On exposing the right lateral ventricle, the whole of the corpus striatum and anterior half of the optic thalamus were found to be softened, of pulpy consistence, and in one place diffuent, and of an ochry yellow colour. On cutting through the softened part transversely, the lesion was found to extend internally into the substance of the hemisphere over a space the size of a large walnut, the colour becoming lighter and lighter as it terminated in the healthy substance of the brain. A tumour was firmly attached to the under surface of the tentorium, and pressing on the substance of the left lobe of the cerebellum superiorly. It was the size of a small walnut, surrounded by a distinct cyst, and, when cut into, the substance presented a soft granular and slightly fibrous consistence, and pale yellow colour. The other parts of the brain were healthy.

Microscopic Examination.—The softening of the corpus striatum and optic thalamus contained numerous compound granular corpuscles, masses, and granules, mingled with broken down fragments of the nerve tubes, as in all cases of inflammatory softening.

On examining a minute portion of the substance of the tumour, it was found to consist of very peculiar bodies, imbedded in what at first appeared amorphous and granular matter (Fig. 58). The addition of water enabled me to separate the constituents of the tumour, composed, first, of the bodies alluded to, and, secondly, of the substance in which they were imbedded (Fig. 59).

1. These bodies were round or oval in shape, and varied in size from the 1-100th to the 1-20th of a millimetre in diameter. They were solid, transparent, with dark abrupt edges, shadowed gradually toward the centre. Each was inclosed in a sheath of filamentous tissue, varying in thickness from the 1-120th to the 1-80th of a millimetre in diameter. Direct pressure caused them to crack in various ways, but generally in a radiated manner, from the centre to the circumference, with two or several segments (Fig. 58, 59, *b, c, d*). An excess of ether, and even boiling in that fluid, produced no change in them. Acetic acid made the sheath more transparent, and rendered visible several oval and elongated

¹ This case occurred in the practice of Dr Peddie, who kindly invited me to witness the examination.

nuclei of the filamentous tissue (Fig. 60). Alcohol and caustic ammonia produced no change. Friction caused them to assume a cylindrical form. (c.) On the addition of diluted nitric acid, they gradually became more transparent, and exhibited a concentric fibrous arrangement around a circular or oval space. This space, which now resembled a nucleus, contained one or two distinct nucleoli, which in its turn contained or was composed of one or more granules (Fig. 61).

From this it became evident that these were organic bodies impregnated with mineral matter, the nature of which my friend, Dr Douglas MacLagan, was so good as to determine. On incinerating a portion of the tumour in a platina crucible, it was reduced to a white ash, in which the globular bodies could still be detected by a microscopic examination. The investing membrane had been destroyed, but the bodies themselves were unchanged, with the exception of a crenated margin, and the presence of distinct concentric rings. Dr MacLagan then determined the earthy matter to consist principally of phosphates.

2. The substance in which these bodies were imbedded, when diluted with water, was seen to consist of nucleated fibro-plastic corpuscles (Lebert), oval, elongated, and passing into fusiform corpuscles, of which there were great numbers. These were mingled with a considerable quantity of molecules and granules (Fig. 59).

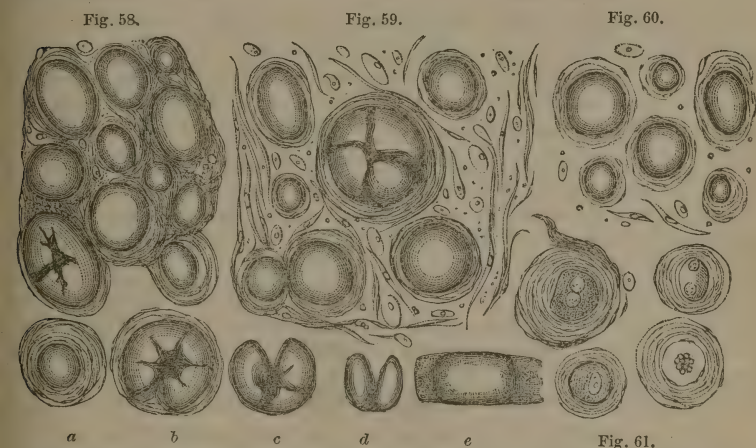


Fig. 58. Mineral bodies imbedded in a seemingly amorphous matter, described Obs. XXII. Fig. 59. The same after dilution with water. Fig. 60. The same after the addition of acetic acid. Fig. 61. The same after the addition of nitric acid. a. One of these bodies with a thick investing capsule. b to d. Others exhibiting various methods in which they crack on pressure. e. Cylindrical form produced by friction.

Remarks.—The structure of this tumour appeared to me to be so peculiar, that I requested Dr Richard Quain to exhibit it to the Pathological Society of London. This he kindly did at its meeting of the 5th of April 1847. At the following meeting, it is reported that Dr Garrod had also made a chemical examination of these bodies, and found them composed chiefly of phosphate of lime and animal matter; that when incinerated, they were insoluble in acetic acid and alkalis, but soluble in hydrochloric and nitric acids, and precipitated from its solutions by ammonia.

Mr Nathaniel Ward remarked, that the bodies under consideration appeared analogous to those described by Valentin as occurring

in the choroid plexus and pineal gland; and which, he states, appear under the microscope at first sight something like air bubbles, but on closer examination are found surrounded by a clear transparent membrane, so as to appear to be contained in a cell formation. On being pressed, they break in a radiated manner, transversely or irregularly. After treatment with acids, the organic skeleton remains, and the concentric structure is still visible. They are with difficulty converted into an ash under the blow-pipe, and even then preserve their laminated arrangement. They are composed of carbonate of lime, with the basic phosphate, mixed up with a little phosphate of magnesia and ammonia.

On referring to the plate and description of Valentin alluded to, there can, I think, be little doubt that the bodies he has described are analogous to, if not identical, with those occurring in the preceding Observation. Their true nature is involved in some obscurity, although it seems certain that nucleated cells have been formed, which afterwards become impregnated with mineral matter, and so constitute a peculiar form of mineral concretion. I have seen cartilage cells in an enchondromatous tumour similarly affected (*Monthly Journal*, August 1847, Fig. 1, p. 88). Lebert has figured similar mineral bodies, presenting concentric lamina, found in a cyst, with walls apparently ossified, from the ovary (*Physiologie Pathologique*, Plate XI. Fig. 10). He has also given three representations of cancer cells with several concentric circles, apparently owing to the regular growth of cell within cell (Plate XVIII. Fig. 9; Plate XXI. Figs. 4 and 9). In one of these latter (Fig. 4), there were both single and double nuclei, as I have figured the bodies in Obs. XXII., after the addition of nitric acid (see Fig. 61). Gluge also, in the Sixteenth Part of his Atlas of Pathological Anatomy, figures similar bodies (Plate II.) removed from an encysted tumour in the dura mater at the base of the brain. He describes them as dividing into four segments on pressure, and as being entirely soluble in mineral acids. Nothing is said of their possessing an organic basis.

From all these facts, we must conclude that the peculiar bodies described (Obs. XXII.), are nucleated cells, which afterwards become impregnated with mineral matters by endosmosis, exactly in the same manner that a similar process is accomplished in plants. Further, it is by no means unlikely that the cells, in the Observation I have recorded, were originally cancer cells, although further researches are requisite fully to establish that conclusion.

With respect to the case itself, there can be little doubt that all the symptoms were produced by the softening of the corpus striatum, optic thalamus, and cerebral hemisphere. The tumour had caused no corresponding depression in the cerebellum, or induced organic lesion in it, and had probably in no way contributed to the fatal event.

ARTICLE V.—*Case of Mortification of the Lower Extremity, from Spontaneous Obliteration of its Arteries, in a young subject—Amputation Twice—Ossific Transformation of the Femoral Artery—Recovery.* By ALEXANDER FIDDES, late Surgeon to the Kingston Dispensary, Jamaica.

ALEXIS SEQUEIRA, aged twenty-three years, came first under my care two years ago, complaining of his left foot. It was painful, had a livid colour, and felt colder than its fellow. The small toe was black, dry, and insensible. Over the course of the tendo Achilles there was the cicatrix of an ulcer, which had proved very difficult to heal; and partly from this, partly from a contracted state of the muscles of the calf, there was a permanent elevation of the heel, so that, in walking, he touched the ground only with the anterior part of the sole. Under the use of poultices, the mortified toe separated, the wound cicatrized, and, by rest and other sedative measures, he felt altogether so much better, that I took my leave, and saw nothing more of him till the middle of August 1847, when I was a second time requested to see him. He then informed me, that though the limb had always felt more or less stiff and painful since my former attendance, yet that it had not prevented his walking abroad until lately, when a black spot made its appearance where the toe had been, and the pain, at the same time, became so aggravated, and the whole limb so stiff and contracted, that he was obliged to keep his bed. The foot felt cold and clammy, and was purple coloured. The cicatrix above the heel had ulcerated, and all the muscles of the limb were rigid and painful on being pressed. On examining the course of the arteries with the fingers and stethoscope, no pulsation could be detected in that side from the foot up to the aorta's bifurcation. There was nothing morbid in the heart's action, or in the circulation of the opposite limb. During my subsequent attendance, extending to a period of two months, matters became daily worse. The temperature of the foot and lower part of the leg was always below the standard heat of the body. The muscular contraction increased until the leg was bent at a right angle with the thigh, and the thigh drawn up upon the pelvis. Gangrene seized all the toes in succession, and spread progressively along the foot. The ulcer above the heel showed a proneness to slough. The pain became almost unsupportable, prevented sleep, and was hardly allayed even by liberal doses of opium. The mouth became covered with aphthous ulceration, and hectic irritation set in.

On the 19th of October, the gangrene had extended close to the ankle joint without showing any attempt at a line of demarcation, and his powers had become so depressed, as to render it obvious that he would soon sink unless relieved of the cause of irritation.

Impelled by the urgency of his condition, but without sanguine expectations of ultimate success, I amputated the limb that day close under the knee, with the concurrence and assistance of Dr Charles Campbell and Dr James Scott. The skin, fascia, and muscles constituting the flaps, looked sound; but there was no bleeding beyond slight oozing, nor could any artery be recognised on the cut surfaces. The integuments were stitched together, and a roller loosely applied. On dissecting the removed limb, the arteries were found to have lost all trace of their tubular formation, having degenerated into tough, yellow-coloured ligamentous bands. The veins were unobstructed, but diminished in calibre, thickened in their coats, and morbidly adherent to the surrounding parts. They consequently did not collapse when cut across, but remained open, like an artery.

October 21.—On removing the dressings this morning, the whole anterior flap was gangrenous; some febrile disturbance; stitches removed—hot water dressings.

November 10.—All the mortified parts have separated, exposing the tibia and fibula denuded of periosteum. There has been no sloughing in the posterior flap,

which is now granulating. Sleeps and eats well. General health greatly better. Muscles of the thigh have lost their spasmodic rigidity, and are not painful on being pressed. There is consequently greater freedom in the movements of the hip.

December 11.—Progressive improvement in general health. The exposed condition of the bones rendering the stump unfit for any useful purpose, I amputated this day in the middle of the thigh, by antero-posterior flaps. The cut surfaces oozed freely, and two arteries required to be tied; one was a muscular twig; the other, a considerable branch, ran in the centre of the great sciatic nerve, and required to be carefully pulled out to keep the nerve clear of the knot.

December 14.—Stump dressed; no uneasiness or discharge. Seems well united.

December 16.—Stitches removed; perfect adhesion of the integuments, except a small aperture through which the ligatures hung; adhesive straps applied.

January 5, 1848.—He called at my house this morning, walking well with his wooden leg. Being in excellent health, he is anxious to resume his avocation. The stump shows no sign of imperfect circulation.

When the amputated portion of limb was dissected, the femoral artery, as low as the knee, was found to have undergone an osseous transformation; but, unlike the calcareous degeneration of the aged, it consisted of a chain, or series of pieces of bone, white, spiculated, and compact, having physical properties similar, in all appearance, to natural osseous tissue. These were deposited in, and linked together by, a yellow fibrous substance, similar to that which occupied the room of the arteries below the knee. This was evidently the matrix in which they were generated and developed. Some of these ossific bodies are an inch long, and nearly half an inch broad. They resemble the bony deposits sometimes found in the *falx major*, and other processes of the *dura mater*.¹ The femoral vein presented similar appearances to the veins described above.

Remarks.—The transformation of the femoral artery observed in the present case, may be considered, I think, as a disease *sui generis*; for it does not appear analogous to the senile degeneration, but distinct from it in structure and mode of growth. Though lower in the scale of organization than the tissue which it has supplanted, it is, nevertheless, capable of carrying on its own nutrition; and it seems probable, if a collateral circulation could be established in cases of this kind, that the arterial trunk thus transformed would remain throughout life without causing disturbance, or falling under the operation of that law by which foreign and injurious substances are expelled from the body. On the other hand, the calcareous degeneration of

¹ I am satisfied, from my own observation and that of others who have had ample opportunities for *post-mortem* investigation, that ossific formations within the cranium are much more common here than in Britain. If this depend on solar heat, as I believe, the fact must hold good universally throughout the tropics, though I am not aware of its having been noticed. In some cases of this kind, I have been unable to connect the presence of these deposits with the existence of any morbid symptoms during life. In others, I have seen insanity, epilepsy, and some obscure forms of headach, associated with them.

In the case of an elderly lady lately attended by me, who was attacked suddenly with loss of speech and jaundice, without any lesion of sensation or motion at first, but which ended fatally by coma, a large, irregular, coral-like osseous body was found in the *falx cerebri*, which had pressed upon and indented the contiguous cerebral substance. No further lesion could be detected in the brain by the closest inspection, although it may have eluded the sight, as the parts were not examined microscopically.

the old man has no title to the rank of a vascular organized structure, being merely a deposit of earthy matter between the tunics of the artery, retained mechanically as an incrustation, and which, sooner or later, operates destructively, as a foreign body, upon the vessel. Both these morbid alterations have, of course, an obstructive effect on the circulation, and produce a liability to chronic gangrene,—in the one, the arterial canal must always be obliterated; in the other, the vessel, though inelastic, may be still pervious, and capable of transmitting the stream of blood. In old persons the calcareous degeneration takes place without any apparent inflammatory action, as a natural consequence of age, or from a pathological state of the fluids, similar to that which produces the gouty and urinary deposits, as has been ingeniously supposed by Andral; but the ossiform transformation appears to be the result of an antecedent arteritis. The first step in the morbid process towards its formation being occlusion of the arterial canal by coagulable lymph, conversion into a dense fibrous structure, then, by a continuance of the inflammatory action, ossification—for it has been fully ascertained by observation of disease and by experiments, that chronic inflammation in fibrous tissue induces its ossification. This hypothesis of the osseous transformation, then, has the support of analogy, although, as Andral observes, we perhaps express as much as we know of the origin of accidental osseous formations, when we say, that they are produced by a perversion of nutrition. Although amputation was performed in opposition to the established principle which forbids such a procedure in idiopathic mortification, so long as there is no line of separation between the dead and living parts—yet I am convinced that this young man's life was saved by the departure from that rule of surgery.

It would probably have been more judicious to have amputated in the first instance above the knee, as the flaps there, from their thick and extensive attachments, and proximity to the centre of circulation, would have had a better arterial supply than the flaps below the knee had in the first operation. This is made probable by the fact, that while the thin integumentary flaps on the anterior surface of the leg perished, the thick and muscular one taken from the calf retained its vitality completely.

The iliac trunk being obstructed, the circulation must be carried on chiefly through anastomoses between the lumbar arteries and ramifications of the gluteal and ischiatic, that had escaped obliteration.

It was remarkable and curious, that the spasmodic state of the muscles ceased as soon as the mortified foot was got rid of. This may be accounted for by supposing that the non-sentient nervous filaments (incident) of the foot were kept in a state of constant irritation by the *inflammatio debilis* which preceded the mortification, and that they conveyed the irritation upwards to the spinal cord,

whence it was reflected by the involuntary motor nerves to the several muscles of the extremity, which were in this way kept in a state of perpetual excitement and contraction.

Such would be the explanation according to the doctrines of Marshall Hall, who, by his researches, has shed a flood of light on the pathology of spasmodic diseases.

ARTICLE VI.—*Case of Mechanical Injury of the Kidneys, followed by Coma; suppression of the Secretion of Urea by the Kidneys, and Absorption of Urea into the Blood—Recovery.*—By E. J. SHEARMAN, M.D., of Rotherham.

IN consequence of death generally so speedily following the suspension of the excretion of urea by the kidneys, from its consequent quick absorption into the blood, and poisonous influence on the brain and nervous system, it rarely happens that time is given for a practitioner to determine decidedly, both chemically and pathologically, that the comatose symptoms depend altogether on the non-elimination of urea by the kidneys. The following case, from the attending circumstances, elucidates this point so simply and fully, that I cannot refrain putting it on record.

On the 23d of last September, Master Edward Chimes, aged eight years, in perfect health, while at play, was run over across the loins by a heavy truck. In two hours after the accident I saw him. He was then in a state of collapse; and my impression was, that some internal hemorrhage was then going on, for he was blanched, cold, and pulseless. He complained of acute pain in the left lumbar region, which was very tender to the touch, spreading to both inguinal and the pubic region. I gave him stimulants, and kept him warm; by which means, in the course of thirty-six hours, he gradually improved, and then he passed a large quantity of blood with his urine, not having passed any urine since the accident. This was repeated several times during the next twenty-four hours.

I examined this urine and blood most carefully; but failed to detect the least particle of urea or urates in it.

My little patient became more restless; fever set in, with a pulse at 130; and the pain in the region of the kidneys increased, notwithstanding the application of leeches, &c. &c. But these symptoms, in the course of two days, were succeeded by coma. He could not be kept awake.

I now bled him in the arm, and re-applied leeches to the tender part.

On examining this blood, urea was most distinctly detected in it, and in considerable quantity. The urine, at the same time, contained not a particle of urea, urates, uric acid, or albumen, and its specific gravity was only 1.005.

I got him under the influence of mercury as quickly as possible. As soon as its specific effect was apparent, urea gradually reappeared in the urine, and its specific gravity increased. By degrees, the comatose symptoms subsided, and in the course of five weeks his usual health was re-established. He continues quite well.

The mode of detecting urea in the blood which I adopted, was the one recommended by Dr G. O. Rees (On the Analysis of Blood and Urine in Health and Disease, 2d Ed., page 40); and which I will

describe shortly, as some readers may not have that useful work in their possession.

The first quantity of serum analyzed, was 400 grains by weight, which was evaporated to dryness over an open steam bath. I broke up the dry extract, added 2 oz. of distilled water, and digested it over a steam bath for an hour, occasionally supplying the loss of water; filtered the digested fluid; washing the residue on the filter with distilled water, which I added to the mother liquor. I then evaporated the whole over an open steam bath, and digested the residue with eight times its bulk of absolute alcohol at a gentle heat for half an hour; taking care not to diminish materially the bulk of the fluid. It was then filtered a second time; evaporated to dryness; and dissolved in a little lukewarm distilled water; and again evaporated to the consistence of a thin syrup. I now added a few drops of nitric acid, and set it aside to crystallize.

Previously to adding the nitric acid, a very strong odour of urea was perceived. On examining the fluid under the microscope to which the nitric acid had been added, tabular crystals of nitrate of urea were easily perceived, commencing in appearance as transverse lines across the watch-glass.

On examining, afterwards, a larger quantity of serum (600 grains), a more considerable quantity of the nitrate of urea was observed.

Although the quantities of serum analyzed, were, in both cases, small, undeniable proof of the existence in them of urea, in considerable quantity, presented themselves: there must then have been a large quantity of urea in the blood.

It was my intention to have ascertained the amount of urea in a certain portion of serum; but was obliged, from existing circumstances, to suspend my examinations at this point.

It is well known that in youth the quantity of urea in the urine is much larger than in the adult, owing to the more rapid disintegration of the tissues. In this case, a *considerable portion* of that excretion must have been circulating through the system.

In the absence of an actual examination of the organs affected, it appears to me, that the ramifications of the renal arteries, which form the external vascular portion of the kidneys, were ruptured by the accident, which would be followed by congestion and inflammation of the Malpighian bodies and Tubuli uriniferi; thus preventing all real secretion; and merely allowing the watery part of the blood to percolate through the tubular portion of the organs.

Part Second.

REVIEWS.

Die Krankheiten der Milz. Eine Pathologisch-Therapeutische Abhandlung. Von Dr C. R. HEINRICH. Leipzig: 1847.

The Diseases of the Spleen; a Therapeutico-Pathological Treatise. By Dr C. R. HEINRICH. Leipsic: 1847. Pp. 450.

THE obscurity in which the physiology of the spleen has been so long enveloped, extends also to its pathology. Little is known either of its inflammatory affections, or the changes of structure to which it is subject. No doubt, many isolated and valuable contributions to this department of pathology have appeared from time to time in various periodicals; but, so far as we are aware, notwithstanding the immense advances our science has made within the last five-and-twenty years, no separate treatise has, as yet, been directed to a consideration of the nature, causes, and treatment, of diseases of the spleen. This desideratum our author has undertaken to supply, and, as the path is somewhat new, he modestly informs us that the work is not to be regarded in the light "of a complete and highly polished fabric, but as a wall, a fragment, on whose foundations future workmen may extend their operations." The work is arranged in two divisions; the first being devoted to a consideration of diseases of the spleen in general; the second to an exposition of its more special maladies. As a natural introduction to these, four chapters are devoted to a history of the development of the spleen throughout the whole series of animals, to its position and structure in man, to its functions, and to a history of its diseases.

As the subject is interesting, and but little known, we shall endeavour to present our readers with such an abstract of the work as may not only prove useful, but prompt to further inquiry; and, in the mean time, confine ourselves to the introductory portion.

The spleen lies deep in the left hypochondrium, and, in the healthy state, its presence and limits can only be accurately ascertained by means of percussion. It must have attained some size ere it projects beyond the ribs, and is appreciable to the touch. In our examinations of this organ, therefore, our principal means of determining its size and position are the pleximeter. But it must not be forgotten that there is no organ in the body which exhibits so many changes, both of size and weight, as the spleen. And this is true of it not only in the diseased, but also in the healthy state. From the measurements of Krause, it appears that the length of the spleen is from five to five and a half inches, whilst its breadth is from three

to four. Schlemm, on the other hand, states its normal length to be from four to five inches. In the child, it is more than a half less than in the adult. The most accurate tables of its weight are those given by Dr J. Reid, in this Journal, some years ago.

Much light has, of late years, been thrown on the structure of the spleen by means of the microscope, and more especially in the works of Müller, Henle, Vogel, Bardeleben, and Evans. From the researches of these authors, it appears that the spleen is made up of the following parts:—first, of a covering membrane; second, of the usual apparatus of arteries, veins, lymphatics, and nerves; third, of a peculiar stroma, or fibrous tissue; fourth, of spleen vesicles, which contain a pulpy substance, and fill up the meshes of the stroma.

The coats of the spleen consist of two very different but closely connected membranes, an external and an internal, proper to the spleen. The former is a direct process from the internal surface of the peritoneum, and, like other serous membranes, is constantly moist and smooth. The latter, and peculiar coat, is a whitish, tolerably firm membrane, and generally regarded as fibrous; but, according to the researches of the author, it is composed of cellular and elastic tissue, and, in proof of this, its great elasticity may be cited. It is quite peculiar to the spleen, gives the organ its form, and serves as a support to the soft parenchyma. Whilst the external surface, with the exception of the hilus, is entirely enveloped in a peritoneal covering, the inner surface closely adheres to that of the spleen. The proper tissue is not penetrated by the vessels as they enter the hilus; but it accompanies and surrounds them, penetrates with them into the parenchyma, and forms, by the films it sends off, a firm basis—the stroma, or fibrous tissue of the spleen.

On examining the peculiar substance of the spleen, it is found to consist of a firm, rigid, fibrous-looking base, and of a red, pulpy mass, filling the spaces of the former. The best mode of examining this stroma, or base, our author informs us, is to macerate a fresh human spleen for some days in acidulated water, cut it into thin slices, free it well from blood and similar matters, by means of a blunt knife, and frequent washing in water and ammonia. On placing a portion thus prepared under an object glass, of even moderately magnifying powers, it appears like a bundle of parallel running fibres, which interlace and cross each other in such a way as to resemble the trabeculæ cordis, having very much the appearance of muscular fibres. The soft, loose, and red pulp contained within the meshes of the stroma, and which becomes brighter on exposure to the air, may be best seen in a slice of fresh healthy spleen. It requires no further preparation than cleaning it of any blood corpuscles which may obstruct the view. On placing it under a magnifier of from 200 to 300, there will then be perceived a more or less close aggregate of generally round, rarely oval, corpuscles, of from 1-300 to 1-350 of a line long, and 1-400 to 1-500 of a line broad, p. 13. Vogel asserts that these are hol-

lowed out in a cup-like manner, and Henle that they contain no nucleus; with neither of these opinions can our author coincide, because, while some of them here and there appear to contain a dark point resembling a nucleus, yet, on moving them about, it is evident that it is merely a granule attached to the surface. Furthermore, each vesicle contains within its smooth transparent membrane an amorphous finely granulated substance, which, on the addition of acetic acid, is evidently recognised as something peculiar. These corpuscles either lie or float singly in the surrounding fluid, or they are grouped irregularly together like a cluster of grapes. These bodies, of such various form and size in one and the same organ, our author can only regard as cell formations in various degrees of development and transformation, which are peculiar to the spleen and its cognate sanguineous glands. He agrees with Evans in thinking, that the secretion of their contents is the peculiar function of the parenchyma of the spleen.

Each spleen vesicle has, besides its artery and vein, an accompanying lymphatic. When the stomach is empty these vessels are scarcely visible; they become distinctly perceptible in the bodies of those dying suddenly, and who have partaken of either liquid or solid ingesta a short time previously; they are then turgid, and appear like white points. They are also perceptible in certain pathological states, as after typhus, the exanthemata, &c. They are more readily seen in the ruminating animals than in man; and the more distinctly the points appear, the more do the vesicles, and indeed the entire parenchyma, of the organ become enlarged. In such cases the vesicle may attain the size of a line in diameter, lying like a round corpuscle in the pulp of the organ, from which it may be readily detached. On puncturing one of these swollen vesicles it collapses, and evacuates a fluid muddy substance, rich in corpuscles, which for size may be compared to the blood globules, though not so regular in their form, and colourless; subsequently they more resemble the chyle or lymph globules. The spleen vesicle has now become a *Malpighian* corpuscle. The vesicles or cells of the spleen, and the spleen corpuscles, so named after their discoverer Malpighi, are, according to our author, essentially the same cell formations, and identical, differing only according to the degree of their development and condition.

The researches of C. H. Schultz and Simon have thrown much light on the state of the blood in the portal system generally; but our present knowledge of the physiological condition of that contained within the veins of the spleen, is, our author conceives, still very defective. He believes it to be different from that contained in the other veins of the body. The researches on this subject have, as yet, been rather scanty, and somewhat contradictory. The following summary comprises all that is known on the subject. According to Soemmering, the blood contained within the veins of the spleen is of a deeper dirtier colour than that of other veins. It is

probable that its specific gravity, like that of the vena portæ and vena cava, is also somewhat lighter. (Thackrah.)

Saunders, Tiedeman, and Gmelin believe that, as regards coagulability, there is no difference between splenic and other blood. Bardeleben concurs in this, and also states, that even under the microscope its blood corpuscles appeared to be the same as that of other blood. Soemmering and Heusinger, on the other hand, affirm that it coagulates more slowly, and that in the dead body the portal blood is always found in a fluid state. Thackrah, Heusinger, and Schultz agree in stating, that the clot of blood obtained both from the vena portæ, and the splenic vein of horses and dogs, is always very soft, and very soon dissolves. Home and Heusinger have shown that the serum is large in proportion to the clot. Heusinger found that, in dogs killed immediately after being fed, the proportion of serum was greater than after a ten hours' fast. Rolof and Soemmering have stated in a general way, that the blood contained in the veins of the spleen is more watery than other blood. Heusinger found the serum of a red colour in dogs. In like manner, Thackrah and Schultz found the serum of the portal blood more or less reddened from dissolved colouring matter, and the former of a much higher specific gravity than that of other veins. According to Heusinger, the serum contains less albumen and gelatine. And, according to Thackrah, the serum of portal blood coagulates neither so completely nor so rapidly as that of other venous blood. According to Soemmering, the blood of the splenic veins contains a large quantity of alkaline salts. On this circumstance, Frerichs has lately founded an hypothesis, in which he supposes one part of the function of the spleen to be, to furnish to the liver by means of the veins a richly alkalized blood, and another, to give off by the lymphatics a poorly alkalized lymph. Lastly, Soemmering affirms, that this blood is poor in oil, and Heusinger states that he has never found fat globules in it. The contrary, however, was long ago affirmed by Dumas, and probably correctly, at least the portal blood has been found by Schultz and Simon, when compared with other blood, to be very rich in fat. It has also been observed microscopically by Simon. The want of a proper quantitative analysis of the portal blood, and more especially of that of the spleen, is still a sensible gap in our physiological knowledge. When furnished with that, it is to be hoped such a step will be gained, as to lead to clearer and more comprehensive views of the true nature of the spleen.

Having disposed of the structure of the spleen, our author next proceeds to consider its physiology. In treating of this subject, said John Bell many years ago, we must be indulged in some speculation; indeed, it is privileged ground; and truly the privilege has not been neglected. No organ of the body has given rise to more strange and wild physiological speculations than the spleen. By some it has been regarded as a mere useless appendage, filling

a vacant space in the body, or, at most, acting as a counterpoise to the weight of the liver; others, mounting a step higher, considered it the seat of the soul—the cause of the venereal appetite; while Sir Anthony Carlisle could give it no higher a function than that of a mere oven to heat the great end of the stomach, and thus assist in the process of fermentation. Leaving such absurdities, we pass to more rational views regarding it. Our author has no hesitation in placing the spleen in the chylopoetic system, and considers its structure so far determined by recent histological researches, as to permit no doubt of its being a lymphatic gland. Comparative anatomy likewise supports this view, as the spleen is wanting in that class of animals in whom no lymphatic system can be anatomically demonstrated; and further, its size bears proportion to the development of this system. We shall give the author's own words as to the part the spleen plays in the assimilating process. After describing the contents of the thoracic duct and their appearance, he proceeds:—"There is almost constantly mixed with the white contents of the thoracic duct a tolerable number of ready-prepared blood globules. Their number varies under different circumstances, but is very much the same under similar conditions of the organism. This fluid is of a much darker colour than that seen in other lymphatics; it is sometimes of a grey colour, or greyish red, sometimes of a rosy hue, and at others of a blood red. This red colour is most striking in the herbivora, increases considerably towards the end of the duct, and is still more marked when its contents are coagulated under the influence of atmospheric air, and when the serum begins to escape in consequence of the contraction of the clot. On inquiring into the cause of this colour, which has not yet been satisfactorily explained, it must evidently be referred to a double origin. A chief part of it is evidently owing to the access of the oxygen of air, a process which takes place most completely during respiration in the lungs; and thus, partly by a physical, partly by a chemical process, is completed the transformation of the tolerably advanced and matured lymph globule into the red blood corpuscle. This chief and concluding act of hæmatisation, must, as a necessary preparation, have been preceded by another of a purely chemical nature, by means of which the red colouring matter is separated, to constitute a covering to the contents of the compound chyle globules, made up of fat and albumen, and by means of which the fatty and watery contents of the portal blood undergo a considerable metamorphosis. The liver and the lungs are the organs in which takes place the chemical transformation of fat, which is thrown off by the former in the shape of bile, in the latter as oxidized carbon."—P. 23. It appears to him, as already suspected by Hewson, that it specially belongs to the spleen, though also to the mesenteric glands, *to separate the blood pigment, and thus co-operate in regulating the watery contents of the new accession (aufnahme) of chyle.* The influence of the spleen on the formation of pigment is, he thinks, further strengthened by

the fact that red-blooded animals only possess a spleen; and that, in animals in which the spleen has been extirpated, the gall-bladder has been found to contain only a small quantity of colourless bile.

The views of Oesterlen are quite opposed to those of all other authors. According to him, the spleen, like all other glands, is made up of two principal parts; first, a crowded heap of peculiar corpuscles, or so named cytoblasts; second, of fat cells. In connexion herewith he regards the action of the sanguineous glands, on the composition of the blood, as a *chemical transformation of the fatty matter of the serum*; the blood is in them partly freed of this fatty matter, but retains by the solution of the cytoblasts, which are always the last to be added to the fluids, a larger quantity of protein compounds. Our author holds this theory to be untenable; both on anatomical grounds, and also that in dogs and rabbits killed by frequent and large doses of olive oil, the lungs, liver, and kidneys, were regularly found to have undergone fatty degeneration; whilst in the parenchyma of the spleen, though subjected to microscopic observation, not a drop of oil could be discovered. Whence he argues, that were the spleen a fat-transforming organ like the liver, which undoubtedly throws off fat in the form of bile, or like the lungs, in which the oil undergoes the process of combustion, then deposits of fatty oil should be found in its cells as well as in those of these organs. The spleen he believes to be as important a preparatory organ for the liver, as the latter for the lungs. Many physiologists have indeed considered it as a bile-secreting organ, without, however, being able to give any satisfactory reason for their so doing. Heinrich lately put forth another view. Looking only at the relation of the spleen to the lungs, he regards the spleen as the lungs of the abdomen, a water lung. The true relation of this organ to the other blood-preparing organs, may be said to be a combination of the above two. Without the spleen there is no normal activity of the liver. Without the spleen no normal respiration. The course of the thoracic duct, from its origin in the superficial net of lymphatics in the spleen and the neighbouring mesenteric glands, to its opening into the venous circulation, is the peculiar workshop of the blood globules, which first become visible here in consequence of the union of the splenic lymph with the chyle globules. Tiedeman and Gmelin found no fibrine in the chyme, but discovered it in small quantity in the thoracic duct. Rees found the specific gravity of the fluid contained in this duct in man, to be not more than 10·24, whilst that of the serum of the blood in general, was at the least 10·52. Let the product of the abdominal lymphatic glands be ever so highly organized, yet hæmotosis is only fully completed in the necessary process of respiration. It is during the process of circulation through the lungs that the new blood first gets rid, by combustion, of its superfluous quantity of fat, whereby its normal quantity of fibrine, which is a higher organisation of albumen, is retained, and the

blood corpuscles are first observed in their proper form and number. The author does not enter upon the morphological part of the inquiry regarding the development of blood corpuscles; but expresses his surprise that, while modern physiology has made this the subject of its special attention, the connexion of the spleen therewith has been most strikingly neglected. He then refers to the researches of Kölliker, and quotes the following as his results regarding the mode in which the blood corpuscles are developed in the full grown mammalia.

First, The blood corpuscles are formed from the colourless cells of the lymph and chyle.

Second, These cells originate in vessels of the smaller and medium diameter, in consequence of free nuclei becoming surrounded with granules, which melt together to form a membrane.

Third, In the vessels of medium diameter, along with this formation of lymph corpuscles, there is also another similar one, which originates in the growth of those which have been previously formed.

Fourth, There are observed in the thoracic duct two kinds of lymph corpuscles,—a larger and a smaller. The first, it is likely, become dissolved in the blood; the latter, it is highly probable, are transformed into blood corpuscles, in consequence of their nuclei disappearing, and their cells becoming filled with colorising matter.

Fifth, Hence, in full grown mammalia, the completely developed blood corpuscles are colourless non-nucleated cells.

In addition to the active part the spleen takes in the preparation of the blood, our author also attributes to it what he calls a *passive* action, or that it acts as a diverticulum to relieve other organs from the too great quantity of blood which in various states of the body may be thrown upon them. This is a very old view, and was long ago supported most ingeniously by Rush and others. Some have thought that it acted as a diverticulum for particular organs, Chaussier for the stomach, Tiedeman and Gmelin for the liver. According to their researches, this organ becomes enlarged from three to four hours after a meal, in consequence of the great quantity of blood sent to it through the vena portæ; but the organ being incapable of dilating, the superfluity goes to the spleen through the splenic vein. Dobson, on the other hand, regarded it as a diverticulum for the whole body. According to him the spleen attains its maximum size from four to five hours after a meal, and this he attributes to the general plethora the system acquires from the food; and, as our vessels cannot contain more blood at one time than another, the superfluity goes to the spleen until it is carried off by the urine, &c.

Broussais, who generalised his notions, regarded diverticuli as of two kinds—temporary and permanent; among the former, he placed the supra-renal capsules, thymus and thyroid glands; among the latter, the spleen. From all we know of the functions of the spleen, then, we think its functions may be reduced to the three

following heads ; 1st, it may be regarded as a diverticulum ; 2d, as an elaborating organ ; and, 3d, that its lymphatics in some way modify the chyle in the thoracic duct. One great argument against either of the latter views is, that the spleen has been, and may be removed with apparent impunity. We must confess, however, we have no great faith in any such experiments. At most, they merely prove that the spleen is not *indispensably* necessary for the various changes effected upon the blood and chyle ; but they by no means prove that it does not concur in these. The results of some experiments, indeed, rather tend more strongly to confirm the latter. Appearances, such as are observed in certain diseases of the spleen, and denoting a want of hæmatin, have been witnessed by Bardeleben, Assolant, Schmidt, and others, after extirpation of the organ.—(*To be continued.*)

PARKES, MILROY, AND GIACOMINI, *on Asiatic Cholera.*—(*Continued from page 608.*)

A Disquisition on Pestilential Cholera ; being an Attempt to Explain its Phenomena, Nature, Cause, Prevention, and Treatment, by reference to an Extrinsic Fungous Origin. By CHARLES COWDELL, M.B., M.R.C.S. London : 1848. 8vo. Pp. 210.

IN our last Number we entered at considerable length into the history of the past and present epidemics of cholera ; into its symptoms, morbid anatomy, and pathology. We have now to direct the attention of our readers to what is known of its mode of transmission, and of its treatment.

MODE OF TRANSMISSION OF CHOLERA.

When a disease of so fatal a nature as cholera attacks a town or district, destroying numbers of persons, the vulgar are very apt to consider that it is contagious, or infectious, and that an avoidance of those affected is a necessary precaution against danger. It almost invariably happens, under such circumstances, that many well authenticated examples of the disorder, apparently arising in this way, are at once found, which are stated with such confidence as to make it a matter of some difficulty to resist their influence. They are also often accompanied with a minuteness of detail and circumstance, that for a time bear down all opposition. The force of arguments so obtained, is always great in proportion to the rarity of the disease, and the panic of the people. Such arguments are, in all cases, nearly the same, and are derived, first, from the coincidence between the irruption of the disorder in previously unaffected places, with the progress of armies, the arrival of ships, of caravans, of fugi-

tives, pilgrims, or individuals; second, the coincidence between the occurrence of cholera in individuals, and their contact with others actually labouring under the disease; and, third, the immunity afforded by seclusion in the midst of an unhealthy district. No doubt, most remarkable examples of these coincidences and immunities may be obtained, and this in considerable number, from the history of the last epidemic. We willingly confess that a study of them is very likely to convince any one of the contagious nature of the disease, whose mind is not fully alive to the inherent fallacy of all such arguments, and to the difficulty there is, in such inquiries, of rigidly determining the relation between cause and effect.

On the other hand, all those who have witnessed the disease on a large scale, and especially Indian practitioners, are opposed to the idea of contagion. The principal arguments which they have brought forward are, first, that the medical men and attendants on the sick have not generally been attacked in undue proportion; second, that the disease, on many occasions, does not spread under circumstances of free intercourse; third, that quarantine regulations have failed in preventing its advance; and, fourth, that its duration, in many places, is very short. We do not consider it necessary to enter into details on each of these heads; we have only to assure our readers that numerous incontrovertible facts fully establish their correctness. One that has always seemed to us the most forcible, is the particular course which cholera often follows. For instance, the north-west passage of the epidemic of 1817—from Calcutta to this country; and, in more limited epidemics, its northward or southward progress, although the same intercourse in every direction has taken place between the inhabitants. Thus, Dr Parkes, in describing the epidemic which occurred at Moulmein in 1842-43, observes:—

During this progress from the north towards the south, cholera, as already stated, attacked chiefly or exclusively the towns and villages situated in low marshy places, on the banks of rivers, or on the shores of the sea. It did not extend inland; and the Burmans were accustomed to escape it by leaving their houses and travelling into the jungle. Directly the first death occurred in any village, the men deserted their fishing, or their paddy-fields, and, betaking themselves to their endless forests, preferred the chances of famine and the dangers of the jungle, to the risks of exposure to the attacks of cholera. They universally stated, that, though they were left without food by this flight, and were exposed to the burning noonday rays, and to the heavy tropical dews at night, yet cholera invariably left them after the second or third day's march inland. The Burmese did not believe the disease to be contagious: they attributed it to the malice of "*nats*," or demons, and they treated it chiefly with the cold affusion, in which they placed some confidence, and with vegetable stimulants and aromatics, of which they possess a very great variety.—P. 160.

Of late years, the different facts and arguments opposed to the contagious nature of cholera, have undoubtedly gained much in force and extension; and governments, in consequence, have begun to see the impolicy and inutility of quarantine regulations as preventive

measures. Dr Milroy sums up his argument on this subject with the following *resumé* of the facts in its support:—

When it is remembered that nine-tenths, I might rather say ninety-nine out of every hundred of the medical men in India, entirely reject the idea of the disease being propagated by infection,—that it has over and over again broken out in places remote from, and having no direct communication with, those where it chiefly prevailed,—that the attendants upon the sick are not a whit more liable to be attacked than others, a fact quite as true in Europe and America as in the East Indies,—that the pestilence every now and then unexpectedly bursts out in some district previously healthy with amazing fury, sweeps off its thousands, and then, in the course of a week or so, ceases altogether, sometimes after a thunder-storm, at other times without any appreciable cause,—that, in its migratory course, it has frequently appeared in numerous points of a large and scattered city at the very same time, while, in other instances, the distance of a few hundred yards has made all the difference between a region of almost inevitable death and one of complete exemption, and even of health, notwithstanding that uninterrupted communication existed all the while between the two; and when, too, we call to mind the indisputable fact that, upon no one solitary occasion have quarantine and other preventive measures of a like nature, however stringently and perseveringly employed, ever yet succeeded in keeping out the disease from any country;—that the Russian government, in 1831, having found their utter inefficacy, speedily abandoned all attempts of the sort;—that the Austrian Emperor formally declared that “he had committed an error in adopting the vexatious and worse than useless quarantine and cordon regulations against cholera,” frankly admitting that he did so before the nature of the disease was properly understood,—that Prussia, too, having in vain had recourse to the same expedients, was forced to give them up,—that, in our own country, the government intimated, in the speech delivered from the throne, if not their positive disbelief, at least their emphatic incredulity as to the importation of the disease from the continent by shipping or otherwise,—that one of the latest acts of the Central Board of Health in London was to announce that cholera patients should be as freely admitted into our public hospitals as any other sick,—that the Board of Health in Ireland candidly admitted that “they were not able to trace the disease to any communication by which it might have been introduced into the neighbourhood of Dublin,”—that the leading physicians and surgeons in Paris drew up a formal memorial, declaring their disbelief in its infectiousness, and that the French Academy of Medicine adopted and confirmed this opinion,—that the government of the United States, too, at first tried the effects of quarantine protection, but quickly abandoned it, the chief medical men in New York, Philadelphia, and other leading cities of the Union having pronounced against it;—when, besides these numerous and forcible reasons, we think of the singular exemption of some countries in Europe from the disease, for one, two, and even four years after the general visitation in 1831-2, and even after the pestilence had crossed the Atlantic, and made its power to be felt over nearly the entire extent of the New World; and all this, too, certainly not from any unusual stringency in the quarantine laws of those countries, but from some hidden cause quite beyond our ken,—can any one, after impartially thinking upon all these things, reasonably entertain a doubt as to the utter inadequacy of personal infection to account for the career of cholera, or hold to the folly and wickedness of ever again attempting to arrest its march by measures which have been proved to be wholly valueless? As well might we ascribe the blasting of our crops to direct transmission of the morbid cause from plant to plant—although unquestionably some forms of blight are capable of being propagated by immediate contact of the healthy with the diseased—and seek to protect them from the unseen foe by building a lofty wall around the threatened fields, as hope to keep out a disease like the epidemic cholera or influenza, by sanitary cordons or quarantine restrictions.—Pp. 33-35.

What, then, it may be asked, is the true mode in which cholera is transmitted? Dr Milroy, having got rid of contagion, says that it is propagated in the same mysterious manner as the influenza, or the blight which attacks vegetation. Dr Parkes, on the other hand, assumes the existence of a certain morbid agent, or virus, the nature and origin of which is unknown, but which is propagated under certain conditions. These conditions he considers to be, first, the comparative independence of the virus of the influence of temperature. He says comparative, because hot climates are acknowledged to be the most common foci of the poison ;—second, the extraordinary influence exerted by the soil upon the virus, which sometimes seems to be contained only in the lowest strata of air ; at others, possesses greater volatility, and passes with rapidity over large tracts of country ;—third, the affinity of the poisons for moisture, although there seems to be a point of saturation beyond which moisture checks the development ;—fourth, the remarkable predilection of cholera for all places where human beings are thickly crowded together, and where the effluvia from the excretions are consequently abundant.

It finds its conditions of development, not in any unusual atmospheric vicissitudes, but in the ordinary local and customary conditions. At Moulmein it prevails in one part of the town for months before it invades another ; the only reason for this preference seems to be, that this part is nearest the river, and is the lowest, dampest, and most thickly populated quarter. It does not attack the residents on the high grounds, and when it spreads from its original point of seizure, the conditions which allow of such spread are to be recognised in the occurrence of meteorological phenomena previously wanting. So also at Madras ; the disease is heard of at a station ninety miles off ; a few days afterwards it appears in Madras itself ; the atmospheric phenomena are not very different from those of years unmarked by its prevalence ; but there is a certain degree of moisture in the atmosphere, and a wind blows directly from the station in which the disease had shortly before been prevalent. It is very fatal in the Black Town, crowded with Hindoos, and in the quarter of the Musselmauns collected round the palace of their Nuwaub, both places dense with a dirty and offensive population ; it is less severe in the fort, quartered by English soldiers and tolerably clean ; it is not seen at all in the houses of the English residents, scattered for miles along the chief roads and the shores of the sea. In both cases, we have the usual evidence of a poison exhaled from unknown sources, and existent in the atmosphere, entering a town, and propagating itself more or less rapidly according as it meets with moisture, animal and vegetable effluvia, and perhaps other terrestrial exhalations, which form the conditions which it demands for its development and increase.—Pp. 173-4.

Whether, then, we regard the hypothetical virus of Dr Parkes as an existing entity or not, the circumstances which he has pointed out as favouring its operation on the human economy are not the less deserving attention.

We had written thus far, when the work of Dr Cowdell fell into our hands. He considers the morbid cause to which we have been alluding, to be in its nature fungous, and endeavours to show that it is developed in the same manner, and produces the same effects as the fungi generally. In successive chapters he discusses the argument under the following heads. 1st, The aptitude of fungi for the

habitat assigned to them as agents in pestilential diseases. *2d*, Such of the most remarkable among the known effects, produced by organized beings of the order protophyta (Fungi, Algæ) as are illustrative of the argument. *3d*, The capability of fungi to produce the phenomena of pestilential disease, as exhibited in pestilential cholera. *4th*, The circumstances which probably concur to produce from the germs of fungi the effects of pestilential disease,—with facts to prove that a variety of nidus causes a variety of fungus. *5th*, That the prevention or destruction of fungous germination, and the consequent catalytic action, is effected by the very medicinal agents, recommended, on the highest authority, in the prophylaxis and early curative treatment of pestilential cholera.

Dr Cowdell has exercised considerable ingenuity in the construction of his hypothesis, and might have strengthened it much more by a reference to the original authors who had written upon the subject, instead of merely consulting abstracts of their works in the pages of the British and Foreign Medical Review, Braithwaite's Retrospect, &c. We must, however, inform him that the idea is not new, and that he will find a masterly exposition of it, as applied to contagious and epidemic diseases, by Professor Henle, in his *Pathologische Untersuchungen*, published in 1840. But, notwithstanding all the talent and research which have been lavished on this speculation, Dr Cowdell has failed to exhibit greater proofs of its correctness now, than when Henle wrote, eight years ago. It is true that the blight among plants, the muscardine among silkworms, and *tinea favosa*, *aphthæ*, and several other diseases in man, may spread by the propagation and development of fungous germs. In all these cases the microscope furnishes us with ocular proof of their existence. Such has not yet been the case in cholera, typhus, and similar disorders; and consequently, with whatever ingenuity analysis may be drawn between them, this view of their origin can only, in the present state of science, be considered as pure hypothesis.

RATIONAL TREATMENT OF CHOLERA.

It is unnecessary, perhaps, for us to remark, that every possible kind of remedy, and plan of treatment, even the most opposite in their nature, have been proposed and employed in Asiatic cholera. This at once betrays the absence of rational indications based upon a knowledge of the pathology of the disease. It is not, then, our intention to draw up a catalogue of what the numerous individual experiences and ideas of practitioners have led them to recommend, but rather to place before our readers the views of our authors on the subject, and ascertain, if possible, how far they may reasonably be considered consistent with the known phenomena the disease presents.

According to Dr Parkes, cholera runs a certain course. When the algid symptoms have once shown themselves, a case cannot be cut short. Even in the mildest forms, warmth does not return altogether

for a long time ; but, when the disease has reached its acme, the patient is invariably seen to remain for some hours in a peculiar state, during which time nature seems to be gradually repairing the injury which has been done. If respiration could be maintained—not the mere mechanical act of breathing in and out, but the chemical process in sufficient integrity to allow the blood to circulate through the capillaries of the lungs—nature would gradually bring about the cure. This is the great problem which medicine has to accomplish, and which, next to the discovery of some actual antidote to the poison itself, appears to be the most ready method of accomplishing the cure of cholera.—(*Parkes*, p. 204.)

According to *Giacomini*, the rational treatment consists in overcoming the phlebitis (venous congestion), causing suspension of the circulation. For this purpose various hyposthenics are indicated ; but he says they are often useless, because there is a complete absence of assimilation in cholera patients.

The instantaneous dryness of the cellular tissue by the operation of the morbid matter absorbed, and the filled state of the veins are such, that, at a certain epoch of the disease, there are no means of causing any remedy whatever to pass by assimilation into the blood. The skin is as dead, and does not absorb, and what is introduced into the stomach only washes or encumbers the absorbing passages. Consequently the best indicated remedies are not digested or absorbed, the digestive organs and skin not lending themselves to this office, and so the most powerful resources of art are rendered of no effect, in fault of a proper channel whereby they may be introduced into the blood.—*Annales*, p. 333.

Dr *Parkes* makes exactly the same statement, saying—

The great difficulty in the treatment of cholera, and the cause of the contradictory and opposing statements which have been made respecting the value of particular medicines, is to be found in the peculiar action of the choleraic poison. This action, by arresting the circulation, and thereby rendering absorption difficult, opposing itself to the common method of administering remedies. After a certain period of the disease, medicines remain in the stomach, and do not pass into the circulation, or do so with great difficulty and slowness. At least this is to be inferred, both from the circumstance that in the advanced stage, calomel, acetate of lead, creosote, opium, turpentine, &c., have been found in the stomach hours after they have been taken, and that fluids taken to appease thirst, remain in and distend the stomach, if they are not vomited, and also from the evident languor and delay of the circulation,—states which are considered unfavourable for absorption.—P. 200.

As medicines, therefore, cannot with any good effect be given internally, other means must be adopted for overcoming the venous congestion. Of these the most powerful seem to be bleeding, cold to the surface, and injections into the veins. Let us examine what our authors tell us regarding each of these means of cure.

Bleeding.—Dr *Parkes* states that the benefit resulting from bleeding was generally more marked according as the disease was in its earliest stage, and according as it tended towards the several varieties of pseudo cholera.

In these latter cases the employment of blood-letting was sometimes followed by very striking results, particularly in those cases attended by a full

pulse, and severe general spasms. For example, I saw a stout European soldier one hour after admission into hospital: he was violently purged and vomited, and was labouring under the most severe and frightful spasms. They were general and quite tetanic in character; the pulse was hard and sharp; the skin warm. He had been treated with calomel and opium without benefit. I immediately opened a vein, and took away forty ounces of blood before the spasms ceased. I then gave him Tinct. Opii, 3j. and repeated it in an hour. The pulse immediately after the bleeding became fuller and less resisting, the vomiting, purging, and spasms ceased, a gentle perspiration appeared on the skin, and he recovered without another symptom of any kind. It was the most striking instance I ever saw of pseudo cholera being cut short.—P. 207.

In the advanced stage he does not think it so useful, although if it do no good, it seems not to be injurious, and occasionally relieves the painful dyspnœa and oppression at the heart. It is, however, very difficult to get blood at this period; it flows from the arm in drops, and warm fomentations are often necessary even to procure these. According to Giacomini—

Blood-letting, as a rule, ought to be practised largely, with a view of preventing the phlebitis, the dilatation of the veins, their engorgement and their immobility. We say, as a rule—for if we wait until dilatation be effected and permanent, the blood only flows drop by drop, and all that is obtained only serves to empty the inferior part of the vein opened, without producing any advantage to the patient—we say, the blood-letting ought to be large; for if the quantity extracted does not correspond to what is indicated, the bleeding is of no effect, and, as the disease makes progress, inexperienced persons attribute to the bleeding the exasperation of the malady.—(*Annales*, p. 333).

Bleeding, when it can be practised, therefore, seems not only theoretically valuable in order to remove the venous congestion, but, when employed judiciously, has been found practically beneficial.

Cold to the surface.—Empirical practitioners, amongst whom we must place Dr Milroy, naturally conceive, that when so much coldness of the surface exists, heat is directly indicated. He says—

The first thing to be done is to have the patient at once stripped and enveloped in warm blankets. The application of bottles of hot water, bags of hot salt or bran to the feet, between the legs, and along the course of the spine, will always be useful in increasing the warmth of the general surface. This is a point of great importance; as the cutaneous circulation is all but arrested, and the blood is consequently accumulated in the internal viscera. The sympathy between the skin and the alimentary canal is known to every one by experience. Cold feet will often cause severe pain in the stomach and bowels; and, on the other hand, indigestion and diarrhœa are almost invariably attended with a chilly state of the surface. The removal of the exciting cause in either case will speedily relieve, or altogether dissipate, the superinduced symptoms. How important, then, it must be to act upon this therapeutic principle in a disease like cholera, in which the whole body is marbly cold, and the gastro-intestinal canal is so strangely and violently perturbed!—P. 43.

Yet all this seems perfectly hypothetical, and constitutes an admirable commentary on the inutility of empiric practice generally, which, instead of seeking to remove the pathological cause of the disease, loses time in vainly endeavouring to alleviate the individual symptoms presented. How opposite are the statements of Parkes and Giacomini. For instance, Dr Parkes says—

Warm-baths, vapour-baths, and warmth applied in any way to the surface, never appeared to me to be of the slightest service in true cholera. The spasms were sometimes relieved, but the algide symptoms were almost invariably increased. The depressing effects of the warm-bath were sometimes marked and unmistakable. I have seen a man walk firmly to the bath, with a pulse of tolerable volume, and a cool but not cold surface, and in five or ten minutes have seen the same man carried from the bath, with a pulse almost imperceptible, and a cold and clammy skin. I cannot find in my notes a single case in which the warm-bath appeared beneficial. It is, indeed, unlikely that the attempt to restore warmth by these trifling means, when the grand source of animal heat is so fatally disordered, can ever be successful. Several writers have also recorded their belief in the inutility of this measure.

Cold to the surface was a measure much more grateful to the patients than warmth. This might have been anticipated also from the way in which the bed-clothes are thrown off, so as to expose the surface freely to the air. The cold affusion, even in the last stage, two or three hours before death, sometimes caused the pulse to become again perceptible. Perhaps the application of cold to the surface may affect the respiration in some way; the gasping inspiration which the shock of the falling water generally induces, may influence the circulation in the lungs, like the first impression of the cold air on the body of the newly-born infant. But, unfortunately, after a short time, the reviving effects of the cold affusion disappear, and the case resumes its former course. The use of large fans and punkahs, causing a blast of air upon the body, seemed to me to be occasionally useful, and to be generally agreeable to the patient.—(*Parkes*, p. 209-211).

Again Giacomini observes—

What seems wonderful and incredible is, that the cold bath during the algide period should be immediately followed by heat of skin, elevation of the pulse, cessation of the cramps, and freedom of respiration, as I have observed this very morning (7th July 1836). In my opinion, therapeutics up to this time does not possess a more efficacious and prompt remedy wherewith to combat the cholera than the cold bath, the passage in the vessels being obstructed by vascular hyposthenics. It is important, however, that in this mode of treatment the cold applications should not be alternated with warm, as the employment of these last may become very hurtful.—(*Annales*, p. 333.

Cold baths, repeated morning and evening, have done prodigies; not only the heat and pulse have reappeared by this treatment; but bleeding, impracticable until then, became possible, and the disease was overcome in the majority of cases the most grave.—(*Ibid.* p. 334).

We leave, then, our readers to judge whether an empirical or rational practice should be followed in the application of heat and cold.

Injections into the Veins.—We have already seen that medicines will not pass into the blood when taken by the mouth; their direct introduction therefore into the circulation by means of injection, although a bold practice, seems to be perfectly warrantable. The immediate effects produced by the saline injections of Dr Latta of Leith, and Dr Mackintosh of Edinburgh, under the idea that the salts in the blood were defective, have been described by all who saw them to be most extraordinary. They dissolved ʒss of muriate of soda, and ʒiv of sesqui-carbonate of soda in ten pints of water, at a temperature varying from 106° to 120° Fah., which were injected slowly, half an hour being consumed in the process. After the injection of a few ounces, the pulse, which had ceased to be felt at the wrist, be-

came perceptible, and the heat of the body returned. By the time three or four pints had been injected, the pulse became good; the cramps ceased; the body that could not be heated was rendered warm, and all the other symptoms were alleviated. These magical effects, however, were not lasting. The discharges continued, and the evacuations became even more profuse; the patient now relapsed into his former condition, from which he could again be temporarily roused by a repetition of the injection; the amendment, however, was more transient, and death followed.

Dr Parkes says, that in some cases which he witnessed in India, he did not even see the temporary vivifying effects which generally followed the employment of these injections in Edinburgh; he therefore determined on trying some other agent.

I still thought that alkalies and salines seemed indicated by the evident occasional escape of the water and salts of the blood, and I fancied some benefit might result from an attempt to supply to the system a proteine compound; although, of course, I could form no conception of the probable mode in which such an addition could prove useful. I determined, therefore, to inject into the veins an alkaline solution of albumen. I shall now detail, as briefly as the subject will permit, the few cases in which these injections were used. In all these cases I believed the patients to be doomed to an inevitable death. I did not consider myself authorized to try an experiment of so serious a nature, on any man, while there yet remained a chance of his rallying under the ordinary treatment. Consequently, I must premise the plan was tried under the most unfavourable circumstances.—P. 219.

The solution injected was composed of sesqui-carbonate of soda ʒiv ; chloride of sodium, ʒij ; the albumen of one egg; and four pints of water, at a temperature of 98° Fah. The flakes of coagulated albumen were separated by filtration, and the fluid was slowly injected in five very desperate cases. We regret that our space will not allow us to give the details of each. Suffice it to say, that there was, as in the case of the purely saline injections, a marked temporary improvement, but they all ultimately died. Dr Parkes observes—

My own impression is still somewhat in favour of this practice. All these five cases were of the worst kind; there did not appear to be a chance of recovery for any of them under the ordinary treatment, and yet one was certainly carried through the cold stage, and, if differently treated during the consecutive fever, might have been completely cured. I think also that the alkaline solution might have been made weaker with advantage, and other ingredients might perhaps have been added.

The operation should, I think, be again tried; it is very simple; it gives no pain, for in this stage of cholera the skin is almost insensible; and it can do no harm to the patients, who are, in fact, doomed to almost certain death. The possible supervention of phlebitis in an after stage should, I think, be disregarded; this disease is less formidable than cholera. The injection is not to cure cholera, but to restore and to sustain the circulation for some few hours, until the healing force of nature may repair the lesions of the blood and restore to the vitiated fluid its normal composition.—P. 237-9.

On the whole, we agree with Dr Parkes in thinking, that injections into the veins, perhaps somewhat modified, deserve a more extensive trial.

For other points connected with the treatment of cholera, we must refer to the different works published on the subject. The opposite opinions maintained with respect to the value of different drugs, are undoubtedly owing to practitioners having mistaken ordinary cases of diarrhœa, or the slighter forms of the disease, for the true algide cholera. To this subject we may return at some future period, in connexion with a little work we have just received from Dr Spencer Thomson on British Cholera.

Observations on some parts of Surgical Practice, to which is prefixed an Inquiry into the Claims that Surgery may be supposed to have for being classed as a Science. By JOHN P. VINCENT, late Senior Surgeon to St Bartholomew's Hospital. London: 1847. 8vo.

MR VINCENT'S work consists of two distinct parts, 1st, An introduction devoted to an inquiry into the claims of surgery to be classed as a science; 2d, Practical observations. In both departments there is a want of systematic arrangement, and a peculiarity in the author's style, a sudden transition from one subject to another, which renders it not a little difficult to follow him throughout his reasoning to its results; and in the practical part, which contains much that is worthy of attention, this peculiarity of style interferes considerably with its usefulness. Mr Vincent commences his inquiry into the claims of surgery to be classed as a science, by commenting on the vague ideas generally entertained as to what really is science; if we understand him aright, "science is the comprehension of truth in any department of knowledge over which the mind has the power of giving certainty to the results of its investigations," in contradistinction "to the knowledge of things derived from the senses and not from the intellect;" such knowledge, according to Mr V., being "neither fixed or certain, because it is not capable of being demonstrated;" and his opinion seems to be, that it is wrong in surgeons to assume, that the understanding cannot attain as great a degree of scientific perfection in their branch of natural knowledge, as it can in mathematics and arithmetic. Now we think that surgeons are in this instance right; there is evidently a great difference between fixed and absolute sciences, such as mathematics or arithmetic, and natural sciences, such as medicine and surgery. The numbers in arithmetic never vary in their relative value; and hence, in dealing with them, we can calculate positively on certain fixed and absolute results. No analogy, therefore, can be drawn between this and a science dealing with organized living bodies, "the vital actions and functions of which vary not only in different individuals, but even in the same individual under different circumstances." As regards the slight value which our author seems to attach to the knowledge derived

from the senses for advancing surgical science, we must bear in mind that this, after all, is the only source from which the surgeon in his department can gather the facts to be afterwards elaborated, by the intellectual process of reasoning, into great general principles. Again, though we agree with him, that surgeons are but too ready to rest on the prevailing opinions of the day, often without due inquiry as to the grounds on which these rest, we are of opinion, that great caution is necessary in rejecting, as mere matter of opinion, the information derived from others. Human life is too short, and the opportunities of individuals too limited, to admit of any one man constructing a complete system for himself; he must (exercising due discrimination) depend to a great extent for information upon the labours of others who have had greater experience than himself. In the words of a modern writer, "Any thing which I am told by a credible witness is information, and so is any thing which comes to my knowledge through any of my own five senses, and so is any knowledge that I gain directly by attending to the processes and feelings of my own mind; but any notion which does not come from one or other of these three legitimate sources, sensation, consciousness, or competent testimony, is good for nothing. It may be an ingenious hypothesis, or a plausible opinion, but it is not matter of fact, it is not information; till it assume a positive form it is not knowledge, and I have no security for its eventual truth."

. . . . "Those things which I have observed for myself, and those which others have told me, make up a solid basis of truth—a *terra firma* of fact; if I am dissatisfied with its narrow limits, I may fling myself over into the abyss of speculation, and, finding in every deep a deeper still, perish at last in total scepticism; or, I may try to soar upwards into a transcendental region, and, after fruitless efforts to be wise beyond my nature's capacity, be content to fold my weary pinions at last on the homely landing-place of common sense and tangible truth." In a practical natural science like surgery, we must look to facts, the knowledge of which we have derived from the evidence of our own senses, or from the positive information conveyed to us by others, as the materials on which our intellectual powers are to work,—the basis on which the superstructure of scientific principles is to be raised; but to trust for its advancement to a process of pure intellection unaided by the senses, is a long since exploded absurdity.

We believe, however, that our author's practical conclusions would be nearly the same in spite of his "pure intellection" theory; for his native good sense bursts through the mists of his philosophy in the following passage, which is strangely at variance with some of his previous reasoning:—

The improvement of surgery upon scientific principles, must commence and proceed by investigating the more common instances of disease, by which there is afforded a larger field for making observations, [what! without appealing to the senses?] a wider latitude for determining the relations, and a

greater facility for obtaining the points of bearing the facts have with each other, than the consideration of rare specimens of disease can afford. In this way only is the greatest knowledge of disease to be acquired. The simple and elementary principles are to be first understood in learning any science. Surgeons, by attaching so much importance to rare cases of disease, rarely do more for the benefit of surgery than giving currency to ill-formed opinions.

Shortly after emitting this ray of light he again becomes misty, dreamy, and philosophical, and enters into speculations we consider it unnecessary to notice; and accordingly we turn to his practical observations as more congenial to our taste.

The practical department of the work embraces observations on a very considerable number of surgical diseases and cases; but they are not arranged under separate heads, nor in such a manner as to render them as useful as they might have been made for reference and consultation. The author's plan seems to be, to lay down certain general propositions regarding what he considers as the laws of vital action in health and disease, and then to apply these laws or principles to the elucidation of certain points in special diseases, which, he believes, exhibit their action. To be fairly understood, the book requires to be read through as one connected whole, and with a constant reference to the preliminary propositions with which it sets out. The first subject treated of is muscular action, considered in reference to fractures and dislocations. Mr Vincent very justly reflects on the want of attention paid to this subject, and remarks on the necessity of studying the functions of muscles in their associated actions, and also the connexion and sympathy between the actions of what are termed the antagonist muscles of a limb; some of his propositions on this subject are sufficiently startling and novel. Thus, at p. 10, he says:—

There is yet another law of muscular action that is to be constantly attended to in practical surgery. It is, that the relaxation of muscles is to be effected by attending to their position, when they are required to throw out their strongest exertions; and not, as usually is supposed, by approximating their attachments. It is a fact we might almost expect, as the result of the powerful influence of association under which they act, and which, whilst this combination exists, regulates the exercises of their forces. Thus the powerful gastrocnemius muscle exerts its greatest strength, when, in progression, it is acting to advance the body forward, by throwing its weight upon the toes or metatarsal part of the foot. To do this the foot is extended, which is the same thing as approximating the attachments of the muscle. Now, that case which is called dislocation of the foot backwards, and in which the tibia is presenting in front of the astragalus, offers an exemplification of this position. Of course the gastrocnemius has its lever of action increased in power as the foot lies extended, by the heel projecting so much behind, which advantage, joined to that of its habitual exertion when in this direction, forms very great opposition to the foot being brought to a flexed position; but yet this is not difficult, and the surgeon will sensibly feel the cessation of its action, the moment it is brought to a right angle with the axis of the tibia. This is owing to its being then in a state of least action, in the usual exercise of its powers.

The example here given does not, when fully considered, bear out the author in his conclusions. It is perfectly true, that, to reduce the luxation in question, the foot requires to be brought to

a right angle with the axis of the tibia; but what position of the leg most facilitates this being done? certainly not the extended position, when the fibres of the gastrocnemius are on the stretch (or at what Mr V. would call their minimum condition of action); but, on the contrary, the bent position, when the leg is flexed fully upon the thigh, so as to approximate the points of attachment of that muscle and relax its fibres. In our own experience, at least, both in the form of dislocation alluded to, and also in oblique fracture of the lower part of the tibia, this is the method by which we have found reduction most readily accomplished. As regards the second case given in illustration of this assumed law—namely, fracture of the humerus below the insertion of the deltoid—we think there is a very obvious cause for the fact he mentions; we mean, the power exerted by the pectoralis major, latissimus dorsi, and teres major muscles, tending to draw the upper portion of the humerus towards the chest, so as to antagonize in a great measure the action of the deltoid. The same peculiar views regarding muscular action mark his observations on dislocations; but he gives many useful practical hints as to the reduction of particular luxations, and as to the distinction between the circumstances in recent and old standing dislocations. Another proposition, which he attempts to lay down as a general law, is, that in the animal economy there exists “a power which can set aside the ordinary laws of matter.” His meaning appears to be, that nature, having adapted certain parts for special functions, there is a natural tendency in these parts under all circumstances to carry on their natural functions; as examples of this law, he adduces cases of openings in the perineum communicating with the urethra, and the urine nevertheless continuing to pass by the urethra instead of the more dependent opening; and especially refers to the example of this after the operation of lithotomy, the water passing by the natural passage before the perineal incision has healed. As to the former of these examples, we readily admit, that in many cases of fistula in perino, only a small quantity of water comparatively speaking passes by the openings in the perineum; but is this in opposition to the natural laws of hydraulics and gravitation? is it not easily explained, when we consider the narrow and tortuous character of the fistulous channels as compared with the canal of the urethra? As regards the urine passing by the urethra before the wound is healed after lithotomy, we must recollect, that the incision of the prostate heals before the external incision; in the earlier stages, it generally depends upon swelling of the lips of the prostatic incision or of the parts around it, and when, in the first days after the operation, the urine does not come away freely by the wound, it is by no means a very favourable symptom: so that we think that, in this proposition also, our author’s examples scarcely justify his conclusion. In speaking of the powers of the constitution to bear up under severe injuries, Mr Vincent strongly recommends abstaining from operative procedure in the first in-

stance, merely watching the patient and throwing in stimuli, if the vital powers seem to flag, or if they are required for the purpose of arresting the progress of gangrene.

I may safely say, he says, that for the many years I have followed up these principles, I have never had a case of traumatic gangrene arising from severe injuries that has not stopped. Of all the subjects that come before the surgeon for prompt and decisive conduct, this is, I am convinced, one of the most important. It is an opportunity for science to step in and save life; it is a demonstration of the ability to decide the agitated question of immediate amputation or not. It is the exercise of a convincing practice much beyond any proof derived from arguments. I am convinced, that by adopting this course of watching the expressions which the constitution shows during the first three or four days of the first stage of very serious injuries, and by throwing in brandy when the indications demand it, that there can be no occasion to amputate on account of traumatic gangrene.

He then animadverts on Baron Larrey's advocacy of immediate amputation, which he condemns as unjustifiable. We believe that Mr Vincent's experience as to the unfavourable results of primary amputations is the same as that of most hospital surgeons, and that his views are very much founded upon this unfavourable experience; but in reviewing this much agitated point, it would be well if surgeons, before condemning the advocates of either side of the question simply from their own experience, would duly consider the different circumstances under which the cases have been treated. Whilst the statistics of civil hospitals in large towns show a very great fatality after primary amputations, the statistics of military surgery, on the other hand, as exhibited in the works of Larrey and Guthrie, show a very great proportion of success in primary, over secondary amputations. We therefore think, that if surgeons were to employ themselves in examining into the causes of this difference of result, they would render a much greater benefit to surgical science, than by ranging themselves as uncompromising adherents of either party.

Mr Vincent makes some very useful observations on varix and varicose ulcers; in regard to the operations for obliterating the veins in varix, he observes—

I have always been at a loss to understand the reasoning which has led to the adoption of the practice of obliterating a vein in a varicose limb. Varix is not a mere disease of a part of a vein, which the tying of the tube, or the removal of the part, can get rid of. It is the derangement of a system of tubes designed for conveying the blood to the centre of circulation; and one condition for properly carrying this on is, that the space in which the fluid is to move should be duly capacious. The breaking up of one channel must throw more fluid into those that are left open; and as the relief of all tubes under disease is just in proportion to the undisturbed course which the contents of the tube are allowed to take, so I apprehend this practice is really calculated to keep the varicose state of the limb. I have seen cases where varicose veins of the leg have been tied, or divided, and where portions have been removed; but as soon as the patient got about afterwards, I have observed that the leg has been embellished with fully as many diseased vessels as before these several operations.

Mr Vincent might have added, that by throwing the returning current of blood upon the collateral venous branches, we not merely distend them, but, that distension gradually effaces their valvular structure, and induces a permanently varicose condition of those veins. The pathology of burns is next considered, and the nature of the different kinds of burns, with reference to the agents causing them, is very carefully and judiciously treated of. Though our author treats at great length on scrofula, we regret to say we have derived no information; and he seems to be quite unacquainted with the more recent researches on this subject. The principal methods of treatment he recommends are, sea air, attention to the cleanliness of the skin, the use of the flesh brush, and in certain affections of the eye, mild mercurials, not a word of cod liver oil, or iodine and its preparations; whilst as regards the local affections in this disease, his remarks prove him to be quite unacquainted with the true pathological condition of the fungoid testicle, and of the recent improvements in its treatment proposed by Mr Syme. Again, whilst he treats us to a long disquisition on the comparative advantages of peas and beans over glass beads for keeping issues open, he does not favour us with a single remark as to his experience in reference to excision of diseased articular surfaces in scrofulous patients. In his treatment of venereal diseases, Mr Vincent is one of the old school, a decided advocate for mercury; but we shall make no remarks on this subject, as our limits forbid our entering on such discussion. Mr Vincent has long enjoyed a very wide field for practical observation, and he appears to have reasoned much on the cases presented to him in his practice; but his work does not evince a knowledge of what has been doing in surgical pathology and operative surgery by others. He seems also to have observed many things through the distorting medium of peculiar views, and hence, as we have more than once noticed, his facts do not always bear out his conclusions; and, on the whole, though the book contains much valuable practical information scattered throughout its pages, we confess ourselves disappointed in it, as coming from a surgeon of Mr Vincent's acknowledged talent and experience.

A System of Surgery. By J. M. CHELIUS. *Translated from the German, and accompanied with additional Notes and Observations.* By JOHN T. SOUTH, Surgeon to St Thomas' Hospital, &c. 2 vols. London: 1847. 8vo.

WE have not yet been able to find room for a lengthened review of this admirable work; but can no longer delay bringing it under the consideration of our readers, and strongly recommending it to their notice. The original text-book of Chelius is well known to be one of the best of its class ever published; and the

valuable and extensive notes of Mr South, in which he has largely embodied the results of his own experience, have undoubtedly rendered his translation of it one of the most complete treatises on surgery in the English language. It seems to us to be alike essential both to the student and practitioner.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVII.

MEETING IV.—*Wednesday, January 5, 1848.*—Dr GAIRDNER in the Chair.

1. *Mr Spence* read two cases in surgery, which are published at length in our last Number.

2. *Professor Miller* read a case of encysted calculus removed by lithotomy. This is also published in our last Number.

Dr Newbigging related the history of a case of encysted stone in the bladder, which had occurred, many years ago, in the practice of Sir William Newbigging. A man, thirty-eight years of age, underwent the operation of lithotomy. On seizing the stone with the forceps, the resistance to its extraction was such as to induce the belief that the opening in the prostate was too small, and this was accordingly dilated so far as was deemed safe. The resistance, however, still continuing, the finger of the left hand was introduced along with the forceps, when it was ascertained that part of the stone was imbedded in a sac, occasioned by the contraction of a portion of the bladder forming a ring around the calculus, which retained and prevented the stone being removed. While the forceps were made to maintain their hold, the ring, which had formed a deep fissure in the stone, was gradually, although somewhat forcibly, dilated by the finger; and, so soon as this was accomplished, the final extraction of the calculus was effected. Notwithstanding the manipulation, and the large dimensions of the stone, the patient recovered in the usual time, without one unfavourable symptom. The calculus is in the collection of the Royal College of Surgeons of Edinburgh.

A discussion now took place on Chloroform, in the course of which Dr Lowe remarked that he had found it useful in preventing certain insane persons from voluntarily rejecting food after it had been administered by the esophagus tube; and Professor Miller had found it, in one case, to relax a spasmodic stricture, so that a large bougie could be passed without difficulty or suffering.

3. *Dr Beunett* exhibited two kidneys studded throughout with small abscesses, and demonstrated, under the microscope, that the pus corpuscles they contained were the nuclei of cells.

MEETING V.—*Wednesday, February 2, 1848.*—Dr ALISON, V.P., in the Chair.

4. *Dr Sellar* read a paper, entitled "On the Signification of Fact in Medicine, and on the Hurtful Effects of the Incautious Use of such Modern Sources of Fact as the Microscope, the Stethoscope, Chemical Analysis, Statistics, &c."

The object of this paper was not to discourage the employment of such sources of medical facts as the microscope, the stethoscope, chemical analysis, statistics, and the like, but to enforce certain precautions against errors, and misapplications of their results, with a view to the greater efficiency of these powerful means of research. In the outset, the author stated that the representations which science affords of the facts, in particular of organic nature, though termed facts by courtesy, may, and often do, involve no inconsiderable amount of error. The admixture of error with the so-called facts, is great in

first observations, particularly when these are made not directly, but by the aid of instruments, chemical analysis, or numerical investigations. Fallacies of this kind are to be overcome only by time, and a careful study of the peculiar tendencies to error in each of these modes of research; and the attempt to force practical medicine, at every step, into a conformity with observations which may prove very wide of the truth, is, in every point of view, extremely injurious.

The discoveries, or supposed discoveries, made from time to time by each method of research referred to, are apt to be magnified into an undue importance at the expense of the older views of the same subjects; and much confusion and false reasoning are introduced by the use of old terms, materially altered from their original signification by new aspects of the objects concerned. The over-ardent partisans of these new methods delight to dwell on their mere conception of the future state of medicine, ages hence, perhaps, as if already present, and thus overrate its past defects. They are apt to fancy that a purely rational medicine is already within our reach, deducible from such researches. Here they confound with rational medicine the mere application of scientific methods to improve the observation of diseases. The microscope, the stethoscope, chemical analysis, and statistical researches, afford incalculable resources for the augmentation and refinement of the history and pathology of diseases. But the progress so made, though founded on science, is but the advance of empirical medicine—the application of newly-discovered aids to observation. So long as the history of a disease, however much improved, or the pathology of a disease, however much improved, falls short of suggesting at once the appropriate treatment, there is no purely rational medicine. The province of rational medicine has been hitherto very limited. And the attempt to found a seeming rational treatment on the successive new discoveries made by the several methods of research under consideration, is what must be resisted as the besetting error of the times. The actual treatment of diseases still rests, and must for ages rest, almost exclusively on the empirical method—the method of observation. Medicine is not so much an empirico-rationalism as a rational empiricism—that is, an empiricism aided by scientific methods. Hardly ever has this been departed from without the introduction of the most hurtful errors in practice. The great lesson which the history of medicine teaches, is to beware of a supposed rational treatment.

As the cultivation of common morbid anatomy, and the attempt to found a rational treatment on it, without taking into account past experience and the other aids of pathology, led into obvious errors of practice, so does the too exclusive cultivation of the refined morbid anatomy, laid open by the microscope, threaten at present to carry us into a like course of fallacy. The stethoscope, by detecting slighter derangements in the chest, corresponding, not in degree, but in kind, to the most dangerous diseases of the old pathology, and though curable spontaneously or under trivial means, yet assuming the same names, has introduced a perverted mode of reasoning as to the treatment necessary in those dangerous forms of disease. Chemical analysis threatens to prove one of the most prolific sources of error in practical medicine, merely from a spirit of impatience urging its less cautious partisans to overstep the bounds within which it promises so much efficient aid to pathology, and rush into the field of rational medicine, where, owing to the very rudimental state of organic chemistry, its day of usefulness is still far distant. Statistical inquiries are in their very nature so fruitful in errors, that, for a long time to come, more detriment than benefit to medicine must be anticipated from this quarter. And the only hope of rendering such enquiries finally subservient to the advancement of medicine, lies in keeping examples of the kinds of fallacy to which they are liable, sedulously before the eyes of those who engage in this mode of research. The deduction of facts from previously established general principles or laws, however plausible, almost uniformly fails in medicine, owing to the invariable dissimilarity, in some one or more respects, between things bearing the same names; and all conclusions obtained in this way, though of much value when

not misapplied, should be regarded as mere suggestions, to be tried by the all-efficient test of experience.

A lengthy discussion followed the reading of this paper, for which, unfortunately, we have no room.

MEETING VI.—*Wednesday, February 16, 1848.*—Mr COCKBURN in the Chair.

1. *Dr Duncan* read several cases of hernia, treated by dividing the stricture without opening the sac. This paper is published in the present Number.

EDINBURGH OBSTETRIC SOCIETY.

SESSION VII.

MEETING I.—*Wednesday, December 22, 1847.*—Dr SIMPSON, President, in the Chair.

CASE OF TWINS—EXPULSION OF THE PLACENTA OF THE SECOND CHILD AFTER THE BIRTH OF THE FIRST.

Dr Woodhead met lately with the following abnormal expulsion of the placenta in a case of twins. After the birth of the first child, the second presented naturally. When the membranes ruptured, a considerable quantity of blood escaped along with the water. On making an immediate examination, *Dr Woodhead* found a placenta lying in the passages, on removing which he found it to be that of the infant still within the uterus. As the bleeding did not continue, he left the infant to be expelled by the uterine contractions. It was not born till twenty minutes after the placenta had come away; but, although still-born, it revived on the ordinary means being used, and it has continued to do well. The placenta of the first infant came away afterwards in the usual manner.

QUESTION OF HEMORRHAGE IN TWIN CASES FROM DETACHMENT OF THE PLACENTÆ, ONE CHILD REMAINING IN UTERO.

Dr Simpson observed, that cases like Mr Woodhead's were strongly illustrative of the curious pathological fact, that *complete* detachment of the placenta was accompanied by little or no hemorrhage; whilst the *partial* detachment of the placenta almost inevitably gave rise to severe and dangerous flooding. He cited different instances of complications in twin-births illustrative of this obstetric law, as for example the following,—

1. When, after the birth of the first child in twins, the placenta of that child is accidentally expelled and removed, the other retained infant and placenta generally remain in utero without flooding. Mauriceau, Denman, Cazeaux, &c., had stated, that under such circumstances flooding must necessarily occur, but the fact was—it did not, its non-occurrence being in obedience to the general law, that when the detachment of the placenta is complete, there is little or no tendency whatever to any attendant hemorrhage. *Dr Simpson* cited cases from *Dr Collins'* treatise illustrative of this, and mentioned others of the same kind which had been communicated to him by *Dr Dawson*, *Dr Dickson*, *Dr Campbell*, &c. "Thus," writes *Dr Dawson* of Newcastle, "I have seen two cases of twins, where the placenta of the first came away before the birth of the second child. There was no hemorrhage in either case. Mr Lang (*Mr Dawson* adds) has met with two cases of this description, and also without hemorrhage;" But,

2. When, after the birth of the first child in twins, the placenta of the second child is accidentally expelled, and an infant and placenta yet remain in utero, still there is generally no attendant flooding. Mr Woodhead's case was an illustration of this. A case of the same kind had been related to *Dr Simpson* by *Dr Andrews*, lecturer on midwifery at the Westminster School, London. One of *Dr Andrews'* pupils was in attendance. A placenta was

expelled after the birth of the first child. Little or no hemorrhage occurred till the second infant was born. Some time afterwards, it was found that the previously expelled placenta belonged to it, and the still retained placenta belonged to the child that had been first born. Here, in a case of twins, there was a child and a placenta remaining in utero; but the remaining placenta was not the placenta of that child. And the placenta which was previously detached and separated, had necessarily torn asunder the bag of membranes, leaving the portion of uterine surface, to which it was itself affixed, exposed, without that exposure leading to any marked degree of hemorrhage.

3. In twins, between the birth of the first and of the second child, the two separate or double placentaë belonging to both are occasionally expelled, and no hemorrhage follows, although there is one child still retained in utero. Dr Dewar of Dunfermline had reported two cases of this kind to Dr Simpson. In one "the placentaë were firmly united, and were discharged after the birth of the first child. There was *no* unnatural discharge of blood. The mother did well." In the second case, "the mother had borne several children. The first child (which was at the full time) presented by the feet, and immediately after its birth, the two placentaë, connected by a membranous but not vascular band, were expelled. Labour followed briskly, and, in little more than five minutes, the second child, which presented naturally, was born. *The hemorrhage* (Dr Dewar adds) *was very slight*, and not greater than what occurs after some ordinary labours. The mother did well." It may be considered that, in these two cases, the interval between the expulsion of the placenta and the birth of the child, was too brief to allow accurate observations to be made upon the degree of existing hemorrhage. This objection is answered by a third case of the same kind, mentioned to Dr Simpson by Dr Protheroe Smith of London. "I was (he writes) called to a patient some time since, who had given birth to one child, immediately after which a large double placenta followed without hemorrhage, leaving a second child in utero, which was expelled, dead of course, *three or four hours afterwards, without further discharge of blood.*" Dr Tyler published, two years ago, a similar case in *The American Journal of the Medical Sciences* for October 1843. After the birth of the first child, a double placenta was discharged. Two hours afterwards, the shoulder of the second child was found presenting, and the uterus in a state of "hour-glass contraction." Many unsuccessful efforts to turn the foetus were made. At last its thorax was evacuated, and the breech brought down. The mother made a good recovery. "Here (observes Dr Tyler) we have a case not merely of simple placenta prævia, but a double surface exposed, there having been two placentaë; the fibres of the womb, first, in a state of rigid contraction; then the irritation consequent upon the performance of the embryotomy; and lastly, a state of excessive relaxation, *and still not a drop of hemorrhage*, and the female entirely recovered."

THE APPEARANCE OF THE AREOLA AS A SIGN OF PREGNANCY.

Dr Simpson showed to the Society a woman seven months gone with child whose breasts gave no indication whatever of her pregnant state. The case was a peculiar one. There was no doubt of her being pregnant, as the foetal heart could be distinctly heard; but, at an earlier stage, great doubt had been thrown on the nature of the case, from the presence of several large fibrous tumours in the walls of the uterus. These had even given rise to the suspicion, on the part of the physician she had consulted, that the foetus was extra-uterine. She had been four years married, and was now pregnant for the first time. On examining both breasts, there is no appearance whatever of a single enlarged gland on the areola; besides, the areola is not at all tumid, and is scarcely darker than the surrounding skin. Dr Simpson had caused a drawing to be made of this breast, which he contrasted with another of the areola of a lady who had never been pregnant, but was suffering from great uterine irritation. In this last instance the areola was turgid, and of a dark brown colour, the papillæ were numerous and much enlarged, and the superficial veins very

large and prominent. He had observed the same appearances as in the last case in other non-pregnant women. In one the woman could never be pregnant, as the uterus was malformed, and not more than one inch and a half in length. In another case the marks were so distinct, that the late Dr Hamilton, trusting mainly to them as undoubted indications of pregnancy, two or three years before pronounced the lady to be pregnant, when the uterus was only enlarged by a mass of fibrous tumours.

Dr Kerr had under his care a lady who is now four months gone in her third pregnancy. The areola is not at all discoloured; but the papillæ are much enlarged and prominent.

ACCIDENTAL HEMORRHAGE.

Dr Moir had recently met with a case of uterine hemorrhage, following the escape of an unusually large quantity of liquor amnii during labour. He believed that the hemorrhage in this case arose from separation of the placenta, owing to the great contraction of the uterus consequent on the discharge of the water from its cavity. The cause producing the hemorrhage in this case, was the very measure recommended by many to arrest accidental hemorrhage—viz. rupture of the membranes.

INDUCTION OF PREMATURE LABOUR.

Dr Moir stated, that, in inducing premature labour, in a large number of cases he had followed, the plan recommended by the late Dr Hamilton. He began by opening up the os uteri with the finger, and separating the membranes. This he repeated every day, or every second day, separating the membranes farther and farther, first with the finger, and afterwards with a round or bent wire. Under this treatment, labour generally comes on from the fourth to the tenth day. Dr Hamilton recommended, as a general rule, that the period for the induction of labour should be at the middle of the eighth month. Dr Moir considered one very great advantage of this plan to be, that the membranes did not, in general, rupture till the usual period in an ordinary labour, after the os uteri was well dilated—a circumstance of very great importance for the safety of the infant. In two cases in which he had brought on labour by the introduction of sponge tents, the membranes were ruptured before the commencement of labour.

When the labour did not offer to come on after a week from the time he had begun to separate the membranes, Dr Hamilton was in the habit of passing up a male catheter with a stilet by the side of the membranes, for a few inches as far as they were separated, and piercing them there with the stilet, and allowing a small quantity of the liquor amnii to escape. Dr Moir stated that he had still in his possession the instrument Dr Hamilton used for this purpose.

Dr Simpson had latterly used conical-shaped sponge tents for opening up the os uteri, in all the cases where he had had occasion to bring on premature labour. These tents were made of sponge dipped in solution of gum arabic, and dried under very strong compression. It is necessary generally to use several tents, and the last one must sometimes be very large. It is not by any means requisite to use the speculum in introducing these tents, as has been recommended and practised in Germany; they are very easily introduced with the finger alone, or by means of a simple bent wire handle, the point of which is passed up into their larger extremity. They are easily applied—cause the patient little or no pain or mischief, and they have generally the os uteri well opened before the labour begins. He had never, in any case, seen the membranes ruptured by them before the commencement of labour. And he believed that the preservation of the membranes was a matter of the first moment in regard to securing the preservation of the child. Dr Hamilton had preserved the membranes, and saved fifty out of fifty-seven children; Dr Meriman had ruptured the membranes, and saved twenty-three out of forty-one children. Dr Simpson had noticed lately a proposal to induce labour by injecting water between the membranes and the uterus, in order to separate them.

He considered this would be a dangerous practice, as the placenta might be separated as well as the membranes, and hemorrhage consequently ensue.

ARTIFICIAL REMOVAL OF A VERY LARGE FIBROUS TUMOUR IMBEDDED IN THE POSTERIOR WALL OF THE UTERUS.

Dr Simpson explained to the Society the mode in which he had lately removed a large fibrous tumour from the posterior wall of the uterus. The patient was forty-six years of age; she had borne ten children, the youngest being six years old, naturally strong and healthy. During the last two years she had been subject to severe floodings, which recurred every fortnight, and had reduced her excessively. Her general health had become quite broken up, and for several months she had suffered much from sore throat, and general irritation of the mucous membranes. *Dr Simpson* found, on examination, a large fibrous tumour imbedded in the back wall of the uterus, and which protruded downwards upon the top of the vagina in a rounded form, the os uteri and uterine cavity lying in front of it—as ascertained by the uterine sound.

Dr Simpson (accompanied by *Drs Malcolm, Zeigler, Weir, and Keith*) made an opening, by means of caustic potass, into the most prominent part of the tumour, about one inch behind the os uteri. It extended through the layer of uterine substance, and into the mass of the tumour itself. On passing up the finger into the hole thus made, it was found that it could be passed easily between the substance of the tumour and the uterine wall. The tumour, in fact, seemed to be very loosely connected with its uterine envelope, and could be separated from it with great ease as far as the finger could pass between them. Two days after the caustic was used, *Dr Simpson* found the artificial opening enlarging like the os uteri in labour, and the tumour beginning to protrude through it. It opened up gradually, the patient taking some ergot, and on the fifth day a large piece of the tumour was found pushed low down into the vagina, while the edge of uterine wall could barely be felt, encircling it like the rim of the os uteri when fully dilated. The abdominal tumour had subsided considerably. On the eighth day, *Dr Simpson* attempted to pass a ligature round the mass, but found it could not possibly be made to include but a very small portion. He separated, however, and brought away a small fragment, not without giving a good deal of pain. The tumour now gradually and more completely filled the vagina. The uterus, however, seemed unable to throw it off entirely, and the patient was getting exhausted from the quantity of the discharge, which was very fetid and offensive. On the 12th day, *Dr Simpson*, while she was completely under the influence of chloroform, passed up his hand by the side of the tumour, completed the separation of the remaining adhesion (like an adherent placenta), and brought away the tumour in one mass. This was done in a very few minutes. The patient awoke up quite quietly, said she had felt no pain whatever; nor did she complain at all of pain in the region of the uterus subsequently. She proceeded very well for three days, her pulse not above eighty; when, in consequence of her nurse taken unwarrantable liberties with her in making her get out of bed, washing, &c., she was seized with rigors, followed by severe sore throat and irritative fever. This completely exhausted her remaining strength, and she died six days after the tumour was removed. The body was examined by *Dr Bennett, Dr Scott, Dr Simpson, and Dr Keith*. The peritoneal surface of the uterus was perfectly healthy, and showed no trace whatever of inflammatory action. The cavity in the back wall, in which the tumour had been imbedded, was much reduced in size, and appeared to be tending to a healthy cicatrization.

The tumour weighed three pounds eight ounces when removed. It must have been somewhat heavier, as the great discharge for several days was no doubt from partial softening and decomposition of the tumour. Various French surgeons had cut down upon such tumours, and enucleated them by instruments, or by the hand. In the above case, *Dr S.* had merely formed an artificial opening into the uterine cyst (as it were) containing the tumour, and then allowed the

uterine walls, which are hypertrophied around such masses (exactly like the uterus in pregnancy), to push down and expel the foreign body. It was, he believed, the first time this new operation had been performed, and the largest fibrous tumour ever yet enucleated. The tumour, on division, was shown to be the common fibrous tumour of the uterus.

PUBLIC DINNER TO MR SYME.

On Friday, February 11, one of the most gratifying spectacles we ever witnessed connected with the profession, took place in the Waterloo Rooms. It consisted of the spontaneous gathering together of one hundred and six medical men, in order to entertain Mr Syme at dinner previous to his departure for London. So large an assemblage, embracing nearly all the talent and professional respectability of Edinburgh, together with several friends who came from a distance, sufficiently attests the high estimation in which that gentleman was held in the northern part of the kingdom. It is not our intention to enter into the particulars; because all public dinners are pretty much alike, and the proceedings, therefore, may be generally guessed at. We cannot, however, resist recording the fact, that only one sentiment of respect and esteem for the distinguished guest seemed to animate this large party—mingled with regret at his departure, and perfect confidence in his future successful career. That the Edinburgh School of Medicine should again be called upon to furnish a first class Surgeon for the University of London, might be expected to create a little jealousy; but we must confess we were scarcely prepared for such an ignoble manifestation of it, as the Dublin Quarterly Journal exhibited in its last Number. We sincerely regret that such petty feelings have ever been permitted to influence any portion of the medical press of this country.

BOOKS RECEIVED.

1. Report of the Proceedings of the Pathological Society of London—First Session, 1846-7. London. 8vo.

2. Adventures of a Medical Student. By Robert Douglas, Surgeon, R.N. With a Memoir of the Life of the Author. 3 vols. 8vo. 1848.

3. Remarks on the Alleged Fatal Case of Chloroform Inhalation. By J. Y. Simpson, M.D., &c. 8vo.

4. British Cholera, its Nature and Causes considered in connexion with Sanitary Improvement, and in comparison with Asiatic Cholera. By Spencer Thomson, M.D., &c. London. 8vo. 1848.

5. A Disquisition on Pestilential Cholera; being an Attempt to explain its Phenomena, Nature, Cause, Prevention, and Treatment, by reference to an Extrinsic Fungous Origin. By Charles Cowdell, M.B., &c. London. 8vo. 1848.

6. Elements of Natural Philosophy; being an Experimental Introduction to the Study of the Physical Sciences. By Golding Bird, A.M., M.D., &c. Third Edition. London. 12mo. 1848.

7. Dysphonia Clericorum; or, Clergyman's Sore Throat—its Pathology, Treatment, and Prevention. By James Mackness, M.D. London. 8vo. 1848.

8. On the Aims and Philosophic Method of Pathological Research; an Inaugural Address delivered at St Thomas's Hospital, December 15th, 1847. By John Simon, F.R.S., &c. London. 8vo.

9. Hassall's Microscopic Anatomy. Part 12.

10. Outlines of Medical Proof. By Thomas Mayo, M.D., F.R.S., &c. London. 8vo. 1848.

11. Hospital Elections and Medical Reform, addressed to the Governors of St George's Hospital, and to the Profession. By Edwin Lee, &c. &c. London. 8vo. 1848.

12. Sanitary Reform and Agricultural Improvements; or, How to promote Health and Abundance. In Three Letters. By Charles F. Ellerman, Esq., &c. Letter I. London. 8vo. 1848.

13. Neue Methode, die Blutmenge im Theirischen Organismus zu bestimmen.

TO CORRESPONDENTS.

Communications have been received from Dr Henry Goode, Dr Reid, Dr Toogood, Dr Weisz, William MacLean, and William Kerr, Esqrs.

Professor Gregory's letter in our next.

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APRIL, 1848.

No. 22. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Etherization in Surgery*: PART III.—*Does Etherization increase or decrease the Mortality attendant upon Surgical Operations?* By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh.—[Continued from p. 333 of November No. of the Journal.]

“Why dost thou whet thy knife so earnestly?
Shylock must be merciful.
On what compulsion *must* I? Tell me that.”

Shakspeare's Merchant of Venice.

STATISTICAL INQUIRY INTO THE RESULTS OF ETHERIZATION IN
AMPUTATIONS.

IN two preceding papers on Etherization in Surgery, published in the Numbers of this Journal for September and November last, I took occasion to discuss various points connected with the subject, and more particularly dwelt upon the necessity of having recourse to the evidence of a large collection of statistics as the only proper and legitimate method of determining the fact, whether the previous superinduction of artificial anæsthesia increased, decreased, or altered in any way the mortality attendant upon surgical operations. During the interval, various circumstances and engagements have intervened to delay the publication of the following inquiry, the results of which were laid at length before the Medico-Chirurgical Society of Edinburgh in July last.—(See *Monthly Journal* for October 1847, p. 302.) From that time up to November, I continued to receive additional returns, all of which have been embodied in the Tables, pp. 700, 701, and 705.

Shortly after etherization began to be employed in surgery, its alleged beneficial or baneful effects were keenly discussed among the members of the profession; and principally, or entirely, upon the results of individual or isolated cases. Some eagerly and stoutly doubted, *in toto*, the possibility of making operations painless; and many who admitted its possibility, denied altogether its propriety, on the alleged ground of its increasing the general subsequent dangers of the patient, inducing a variety of alleged morbid states and lesions, and adding, on the whole, to the fatality of operative surgery.

Amidst the many conflicting and contradictory assertions that were uttered on these points, I became convinced that there was only one method of arriving at the truth, viz., by instituting a statistical investigation upon as large a scale as possible into the results of the practice, and thus ascertaining whether, out of an extensive series of operations performed with and without etherization, the mortality was greater or was less when the patients were operated on in a narcotized and anæsthetic state, than when they were operated on in a waking and æsthetic state.

The first difficulty to be encountered in such an inquiry was the difficulty of obtaining a proper field and standard for the proposed comparison. But *first* of all it was evident that the comparison, whatever it might be, could only be properly instituted between patients operated on in public hospitals, with and without etherization. For we had nowhere published, nor did it seem possible to obtain, any adequate comparative returns of the results of operations from the surgical practice of private practitioners. Besides, hospital returns were preferable in this respect, that there existed on the whole, every where, undoubtedly a far greater uniformity between the hygienic and other collateral circumstances of patients operated on in hospital than in private practice. *Secondly*, however, it was further evident, that in seeking and fixing upon a criterion by which we could compare the statistical results of surgical operations formerly performed without ether, with those now performed upon etherized patients, it was improper and impossible to institute the comparison between *all* operations and reports of operations in hospitals; for the severity and danger of the operations performed in, and reported from, different hospitals, differed immensely in their nature, and consequently in their results. In order, therefore, to obtain the primary requisite for a correct statistical inquiry—of having data of a *similar* kind and character for the proposed testing and comparison—it was necessary to select and contrast the results of some *one* operation without ether, with the results of the same *one* operation with ether. With this view I selected the larger amputations of the limbs as the fittest field on which to conduct the proposed investigation; and I restricted myself to hospital amputations of the thigh, leg, arm, and fore-arm, on account of their being every where performed in almost the same manner, for the same causes, under the same circumstances, and on the same class of subjects;

and because there already existed extensive published researches, by Phillips, Lawrie, and Malgaigne, into their absolute mortality, when performed under ordinary circumstances and without anæsthesia, to aid us in satisfactorily determining the nature of the results of the new practice of operating upon patients in an anæsthetic state.

Having thus fixed upon the mode of inquiry, I proceeded to apply for returns from all the surgical hospitals of Great Britain and Ireland that I could hear of, as likely to have employed etherization in amputations. And I feel it quite impossible to return thanks in any adequate terms for the very great politeness and kindness with which my inquiries were answered on all hands.¹ In some hospitals

¹ In my letter of application I stated, that "the effects, whether favourable or unfavourable, of etherization upon the ultimate recoveries of patients from surgical operations, is still a matter of much doubt and uncertainty. We have as yet had no proper collection of data to ascertain whether the mortality of operations has been increased or not by patients being placed under the influence of ether at the period of their performance. In order to determine as far as possible this important point, I have been induced to undertake the statistical investigation of the results of the larger amputations in cases where ether inhalation was employed at the time of operation. Amputations have been selected for this purpose in preference to other operations, because they are, under all common circumstances, nearly and every where alike, and because the general average mortality accompanying most of the greater amputations is already known from the inquiries of Phillips, Lawrie, and others, and thus a ready standard of comparison afforded us. You would, therefore, oblige me by filling up the following table with any results, however few in number, of amputations in which ether was used in your hospital. I especially wish to know *all* the deaths as well as all the recoveries in these operations; and by thus collating, on the whole, a large body of statistical data, I hope to be able to arrive at some interesting general results."

Copy of Form of Table sent.—"Results of Amputations performed upon Patients in an Etherized State in the ——— Hospital."

Seat of Amputation.	Primary or for Injury.		Secondary or for Disease.	
	Total No. of Cases.	Total No. of Deaths.	Total No. of Cases.	Total No. of Deaths.
Amputation of Thigh				
Amputation of Leg				
Amputation of Arm.....				
Amputation of Fore-Arm...				
Total.....				

It may be proper to remark, that in answer to the returns I had the results of twenty-four amputations of the fore-arm sent me, which are not included in the subsequent remarks in the text. Out of these twenty-four amputations ten were primary, with one death, and fourteen secondary, with two deaths. I have omitted them in the text in consequence of finding that Mr Phillips, in his standard of amputations, confines his returns to those of the thigh, leg, and arm, and does not include those of the fore-arm.

ether had not been tried, and I was consequently furnished with no data; in others in which it was used, my correspondents were quite at issue about its propriety; many were doubtful; some expressed themselves strongly against it; others strongly for it. But I was principally anxious to obtain the total results, believing that *they* would decide the question far more certainly than any individual experience or individual opinion could. In Table No. I (see p. 701 and 702), these results are given in a detailed form, with the names of the gentlemen who kindly reported each return to me.¹

GENERAL MORTALITY OF AMPUTATIONS OF THE THIGH, LEG, AND ARM, WITHOUT ETHERIZATION.

Before attempting to determine whether the results in these ether amputations (Table, No. I.) are, or are not favourable to the adoption of Anæsthesia in Surgery, let me in the first place state the results of the previous investigations that have been published by Phillips, Lawrie, and Malgaigne, relating to the mortality of these same amputations, when the same amputations were performed without ether. In the year 1837, Mr Benjamin Phillips brought before the Royal Medico-Chirurgical Society of London, a communication² on the results of amputation of the thigh, leg, and arm, in different countries. From the collection of cases which he laid before the society, Mr Phillips concluded that the general mortality of these larger amputations amounted to 23 deaths in the 100 operations. The correctness, however, of his conclusions was called in question by the publishing committee of the society, on the idea that the alleged mortality was too great, and he was recommended to investigate the subject more fully before proceeding to publish his observations. Further inquiry served only to satisfy him that his previous results were understated rather than overstated.

Subsequently, in 1844, Mr Phillips published a table of a still more extensive series of cases.³ This collection, however, includes the results of private as well as of hospital practice. "They are (says Mr Phillips) the whole, so far as I know, of the cases of amputation recorded in the periodical literature of this and other countries during the present century." The table (No. II. p. 703), extracted from Mr Phillips' second paper, shows in a summary way the results which he obtained from these sources.

¹ In No. 49 of the Table the name of the hospital is not mentioned, as my correspondent unfortunately omitted to date his return. The Paris hospital returns of twenty-two cases (No. 40) are distributed according to the standard of Malgaigne; Dr Burguières, in a note to me, having said that he was unable to state the exact number of these amputations which were respectively primary and secondary.

² Observations on the Results of Amputations in different Countries. Medical Gazette, Vol. XXII. 1837-8.—P. 457.

³ Medical Gazette, Vol. XXXIII. 1843-44.—P. 804.

No. I.—TABLE SHOWING, IN DETAIL, THE NUMBER OF INDIVIDUAL AMPUTATIONS AND THEIR RESULTS, IN DIFFERENT HOSPITALS, UPON 302 PATIENTS UNDER ETHERIZATION.

No.	Name of Hospital.	Name of Reporter.	Amputation of Thigh.				Amputation of Leg.				Amputation of Arm.				TOTAL CASES.			
			Primary.		Secondary.		Cases.	Primary.		Deaths.	Cases.	Primary.		Deaths.	Cases.	Deaths.	Cases.	Deaths.
			Cases.	Deaths.	Cases.	Deaths.		Cases.	Deaths.			Cases.	Deaths.					
1	Aberdeen Royal Infirmary,	Dr Keith and	—	—	2	1	—	—	—	0	5	—	—	—	7	1	—	—
2	Bedford General Infirmary,	Dr Macintosh,	—	—	3	0	—	—	—	—	—	—	—	—	4	0	—	—
3	Birmingham General Infirmary,	Mr Hurst,	—	—	1	0	2	0	1	0	1	0	0	—	4	0	—	—
4	Birmingham Queen's Hospital,	Mr Amphlett,	—	—	1	0	1	0	0	0	3	0	0	—	4	0	—	—
5	Bristol, General Hospital,	Dr Wright,	—	—	3	0	—	—	—	0	3	—	—	—	6	0	—	—
6	Bristol Infirmary,	Dr Lansdowne and	—	—	1	1	—	—	—	—	—	—	—	—	1	—	—	—
7	Belfast Hospital,	Mr Mason,	1	1	1	1	—	—	—	0	1	0	—	—	1	1	—	—
8	Cumberland Infirmary,	Mr Moore,	—	—	2	1	—	—	—	0	1	0	—	—	3	1	—	—
9	Chester Infirmary,	Mr Page,	—	—	—	—	—	—	—	0	1	—	—	—	4	1	—	—
10	Cork South Infirmary,	Mr Harrison,	—	—	—	—	—	—	—	0	1	—	—	—	1	0	—	—
11	Dundee Infirmary,	Dr Tanner,	—	—	3	0	1	0	1	1	2	—	—	—	5	1	—	—
12	Dunfries Infirmary,	Dr Monroe,	1	1	—	—	—	—	—	0	1	—	—	—	1	0	—	—
13	Derbyshire General Infirmary,	Mr Borthwick,	—	—	1	0	—	—	—	—	—	—	—	—	—	—	—	—
14	Devon and Exeter Hospital,	Mr Fox,	—	—	2	0	1	—	—	0	1	—	—	—	1	0	—	—
15	Dublin Mercer Hospital,	Mr James,	—	—	1	1	—	—	—	—	—	—	—	—	3	0	—	—
16	Dublin Richmond Surgical Hospital,	Dr Jamieson,	—	—	3	1	—	—	—	—	—	—	—	—	1	1	—	—
17	Edinburgh Infirmary,	Dr Hamilton,	1	1	2	0	—	—	—	1	1	—	—	—	4	2	—	—
18	Edinburgh Infirmary,	Mr Miller,	2	2	4	1	1	0	0	0	3	—	—	—	3	1	—	—
19	Elgin Infirmary,	Dr Duncan,	—	—	—	—	1	—	—	—	—	—	—	—	1	1	—	—
20	Glasgow Hospital,	Dr Paul, and	—	—	8	4	5	1	—	2	5	0	—	—	7	1	9	—
21	Hereford Infirmary,	Dr Steele,	—	—	3	0	—	—	—	—	—	—	—	—	17	—	—	—
22	Hull General Infirmary,	Mr Wandby,	1	0	1	1	3	1	—	0	2	0	—	—	3	0	—	—
23	Hants, Royal South Infirmary,	Mr Craven,	—	—	—	—	—	—	—	0	1	—	—	—	4	1	—	—
24	Ipswich Hospital,	Dr Bullar,	—	—	3	0	—	—	—	—	1	—	—	—	3	1	—	—
	Carried forward,	Dr Durrant,	6	5	45	12	14	2	33	4	7	0	12	5	27	7	90	21

TABLE SHOWING, IN DETAIL, THE NUMBER OF INDIVIDUAL AMPUTATIONS AND THEIR RESULTS, IN DIFFERENT HOSPITALS, UPON 302 PATIENTS UNDER ETHERIZATION, *continued*.

No.	Name of Hospital.	Name of Reporter.	Amputation of Thigh.				Amputation of Leg.				Amputation of Arm.				TOTAL CASES.			
			Primary.		Secondary.		Cases.	Primary.		Deaths.	Cases.	Secondary.		Deaths.	Cases.	Primary.		Deaths.
			Cases.	Deaths.	Cases.	Deaths.		Cases.	Deaths.			Cases.	Deaths.			Cases.	Deaths.	
25	Brought forward,	Mr Hey,	6	5	45	12	14	2	0	4	33	12	0	5	27	7	0	21
26	Leeds Infirmary,	Mr Curling,	1	0	4	0	1	0	0	0	1	—	—	—	1	0	0	0
27	London Hospital,	Mr Haig,	2	1	6	3	3	2	1	1	4	1	1	0	6	2	3	4
28	London, St Bartholomew's Hospital,	Mr Liston & Mr Cadge,	2	1	5	0	—	—	—	1	5	1	—	1	2	1	1	2
29	London, University College Hospital,	Mr Ferguson,	—	—	1	0	—	—	—	0	7	—	—	0	2	—	2	0
30	London, King's College Hospital,	Dr Snow,	—	—	12	2	—	—	—	1	1	1	—	0	—	—	2	2
31	London, Middlesex Hospital,	Mr Shaw,	—	—	5	0	1	0	—	1	1	—	—	0	—	—	7	1
32	London, Charing-Cross Hospital,	Mr Avery,	1	0	2	1	—	—	—	—	1	—	—	0	1	0	1	1
33	London, Westminster Hospital,	Dr Bird,	—	—	4	0	—	—	—	—	—	—	—	0	—	—	6	0
34	Leicester Infirmary,	Mr Paget,	2	0	—	—	—	—	—	—	1	—	—	0	2	0	1	0
35	Liverpool Northern Hospital,	Dr Bainbrigge,	—	—	3	0	1	0	—	3	4	2	1	1	1	0	0	0
36	Liverpool Southern Hospital,	Mr Morris,	—	—	1	2	1	0	—	—	1	2	0	0	2	0	2	6
37	Manchester Royal Infirmary,	Dr Reid,	—	—	2	1	—	—	—	1	2	1	0	1	—	—	4	0
38	Newcastle Infirmary,	Mr Greenhow,	—	—	—	—	2	2	0	1	2	—	—	—	3	2	—	—
39	Nottingham General Hospital,	Mr Wright,	—	—	3	0	1	0	—	0	1	—	—	—	2	1	4	5
40	Paris Hospitals,	Dr Burguières,	3	2	7	2	3	1	1	2	5	2	1	1	8	4	4	0
41	Perth Infirmary,	Dr McFarlane,	4	2	1	0	—	—	—	—	—	—	—	—	4	2	1	0
42	Sussex County Hospital,	Mr Parson,	1	0	3	0	3	1	0	0	1	—	0	—	5	1	4	0
43	Staffordshire, North Infirmary,	Mr Turner,	—	—	3	0	—	—	—	0	2	—	—	0	4	—	0	0
44	Sheffield General Infirmary,	Mr Jackson,	—	—	2	0	—	—	—	0	2	2	—	—	7	—	2	2
45	Salisbury Infirmary,	Mr Young,	—	—	4	2	—	—	—	0	3	—	—	—	2	—	5	0
46	Stockport Infirmary,	Mr Rayner,	—	—	3	0	—	—	—	0	2	—	—	0	—	—	0	0
47	Winchester County Hospital,	Mr Wickham,	—	—	4	0	—	—	—	0	2	—	—	—	7	—	8	0
48	Worcester Infirmary,	Mr Sheppard,	1	1	4	0	—	—	—	0	4	2	1	—	3	2	0	0
49	Mr Stocker,	Mr Stocker,	1	0	1	0	1	0	—	—	—	—	—	—	2	0	—	0
	Total,		24	12	121	25	32	9	9	13	81	27	4	8	73	25	229	46

No. II.—*Table of the Mortality of 1369 cases of Amputation of the Thigh, Leg and Arm.*

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No of Cases.	No. of Deaths.	Per Centage of Deaths.	No. of Cases.	No. of Deaths.	Per Centage of Deaths.
Thigh,	245	176	72	415	87	21
Leg,	204	88	43	231	61	27
Arm,	164	49	29	110	26	24
TOTAL,	613	313	51	756	174	23

In the year 1840, Dr Lawrie of Glasgow published an excellent paper¹ on the results of amputations, with tables showing the rate of mortality from amputation in the Glasgow hospital, from the period of its foundation in 1794 down to 1839. Dr Lawrie's inquiries yielded an average mortality greater than that of Mr Phillips, being as high as 36 per cent. The following table, made from data in Mr Lawrie's paper, contains the results of amputation of the thigh, leg, and arm in the Glasgow hospital.

No. III.—*Table of the Mortality of 242 Amputations of the Thigh, Leg, and Arm in the Glasgow Hospital from 1794 to 1839.*

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Per Centage of Deaths.	No. of Cases.	No. of Deaths.	Per Centage of Deaths.
Thigh,	35	27	77	92	19	20
Leg,	27	18	66	35	12	34
Arm,	36	18	50	17	3	17
TOTAL,	98	63	64	144	34	23

In 1842, a valuable series of papers on the statistics of amputations was published by Professor Malgaigne in the *Archives G n rales de M decine*, his data being derived from the reports of the Parisian hospitals. In these papers, Malgaigne enters largely upon the subject of the mortality of amputations. The following table,

¹ On the Results of Amputations. Medical Gazette, Vol. XXVII. 1841, p. 394.

compiled from data in his returns,¹ exhibits a mortality still higher than that of the Glasgow hospital.

No. IV.—*Table of the Mortality of 484 Amputations of the Thigh, Leg, and Arm, in the Parisian Hospitals, from 1836 to 1841.*

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Per Centage of Deaths.	No. of Cases.	No. of Deaths.	Per Centage of Deaths.
Thigh,	48	34	70	153	92	60
Leg,	80	51	63	112	55	49
Arm.....	30	17	56	61	24	39
TOTAL,.....	158	102	64	326	171	52

These three tables of large collections of cases by Phillips, Lawrie, and Malgaigne, may be properly considered as giving a correct idea of the general mortality of these amputations in hospital practice, and might be used with justice as subjects of comparison with any series of cases similar to them in the whole series of circumstances, except that one whose influence upon the results is to be decided. After, however, I began to collect the results and mortality of the same amputations upon patients in an etherized state from various British and other hospitals, it was objected to the inquiry that it would be unsatisfactory in two respects, viz. that the amputations compared were possibly performed in different classes of hospitals, and at dates so different that I did not consider in my investigation the changes and improvements which might possibly have been introduced into the very methods of operating.

In order, then, at once to enlarge the basis of data for comparison, and to obtain a series of cases still more exactly similar to the collection of ether amputations which I was making, I procured from various British hospitals, through the kindness of different correspondents, and from published data, returns of the latest amputations that had been performed in them immediately previous to the introduction of etherization. These returns are given in detail in the opposite page (p. 705). All of the operations have been performed within the eight years, from 1839 to 1846 inclusive. By having this collection of cases as an additional standard, I hoped to avoid all cavil on the ground of any supposed difference in the time, and other collateral circumstances, in which the compared operations were performed.

The data in the following table, No. V. (p. 705), when condensed into the tabular form, afford the results in table, No. VI. (p. 706.)

¹ Archives Générales de Médecine, Vol. LVIII. 1842, p. 40.

No. V.—TABLE SHOWING, IN DETAIL, THE RESULTS OF 618 AMPUTATIONS, IN 30 DIFFERENT BRITISH HOSPITALS, IMMEDIATELY BEFORE THE INTRODUCTION OF ETHERIZATION.

No.	Name of Hospital.	Date of the Observations.	Name of Reporter.	Amputation of Thigh.				Amputation of Leg.				Amputation of Arm.				TOTAL CASES.			
				Primary.		Secondary.		Primary.		Secondary.		Primary.		Secondary.		Primary*.	Cases.	Deaths.	Secondary.
				Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.				
1	Aberdeen Royal Infirmary,	1841-1846	Dr Macintosh,	5	3	7	2	5	2	15	4	5	0	0	15	5	27	6	6
2	Bristol, St Peter's Hospital,	1844-1846	Dr Rogers,	1	0	3	0	2	0	12	1	3	0	1	6	0	3	0	3
3	Bristol General Hospital,	1845-1846	Mr Morgan,	7	5	2	1	3	0	—	—	3	0	0	—	0	2	1	1
4	Bristol General Hospital,	1846	Mr Mason,	1	0	2	1	—	—	—	—	—	—	—	—	—	6	0	0
5	Berks Royal Hospital,	1840-1845	Mr May,	7	5	5	0	3	0	—	—	3	0	0	13	5	6	1	1
6	Cork Northern Infirmary,	1845-1846	Dr Bullen,	1	0	2	1	—	—	3	0	2	0	—	1	0	6	1	1
7	Cumberland Infirmary,	1845-1846	Mr Page,	—	—	4	1	—	—	2	0	—	—	—	2	0	6	1	1
8	Dumfries Infirmary,	1842-1846	Mr Borthwick,	—	—	6	1	—	—	2	0	1	1	1	—	—	9	2	2
9	Derbyshire General Infirmary,	1845-1846	Mr Fox,	—	—	5	2	—	—	6	0	—	—	—	—	—	11	2	2
10	Dundee Infirmary,	1844-1846	Dr Monro,	1	0	2	0	1	1	4	0	3	0	—	5	1	6	0	0
11	Edinburgh Royal Infirmary,	1840-1845	Dr Peacock,	8	8	35	13	4	3	12	4	5	4	1	17	15	49	16	16
12	Elgin Infirmary,	1844-1846	Dr Paul,	—	—	8	2	1	1	1	0	7	—	—	1	1	9	2	2
13	Glasgow Hospital,	1840-1844	Dr King,	11	10	14	6	14	5	6	1	4	4	—	32	19	20	7	7
14	Glasgow Hospital,	1844-1846	Dr Orr,	10	3	22	8	17	5	13	4	12	3	5	39	11	46	17	17
15	Hull General Infirmary,	1846	Mr Crayen,	—	—	2	0	3	0	—	—	2	0	—	5	0	2	0	0
16	Inverness Infirmary,	1845-1846	Dr Manford,	1	1	—	—	—	—	—	—	1	0	—	2	1	—	—	—
17	Ipswich Infirmary,	1845-1846	Dr Durrant,	—	—	6	0	1	0	—	—	—	—	—	4	0	6	0	0
18	London, St Bartholomew's Hos.,	1846	Mr Haig,	2	1	7	2	3	0	4	1	3	0	0	8	1	14	3	3
19	London University College Hos.,	1841-1846	Mr Cadge,	5	3	16	7	1	1	15	2	7	1	1	7	4	38	10	10
20	Leicester Infirmary,	1845-1846	Mr Page,	2	0	3	1	1	0	4	0	3	0	0	6	0	9	1	1
21	Liverpool Northern Hospital,	1845-1846	Dr Bainbrigge,	5	3	8	4	4	1	6	2	2	0	—	11	4	14	6	6
22	Liverpool Southern Hospital,	1846	Mr Morris,	1	1	1	1	2	1	1	—	—	—	—	3	2	1	0	5
23	Newcastle Infirmary,	1840-1846	Mr Benson,	7	3	10	2	8	2	10	3	10	2	0	25	7	22	8	8
24	Perth Hospital,	1847	Dr McFarlane,	—	—	13	5	—	—	7	2	2	2	1	—	—	22	6	2
25	Sussex County Hospital,	1844-1846	Mr Parson,	2	2	2	2	2	1	3	0	3	1	0	7	4	6	7	1
26	Salisbury Infirmary,	1845-1846	Mr Young,	2	0	3	0	3	2	3	1	1	0	—	6	2	3	0	0
27	Stockport Infirmary,	1845-1846	Mr Rayner,	2	2	2	0	—	—	1	0	2	0	—	4	2	3	1	0
28	Sheffield Infirmary,	1845-1846	Mr Jackson,	—	—	1	0	3	0	—	—	—	—	—	—	0	1	0	0
29	Worcester Infirmary,	1846	Mr Sheppard,	—	—	5	0	1	0	3	0	4	2	1	5	2	9	0	1
30	York County Hospital,	1845-1846	Mr Hey,	—	—	1	0	1	1	3	0	2	1	1	3	2	5	1	1
Total,				73	45	211	62	80	26	135	23	77	17	42	10	230	88	388	95

No. VI.—*Table of the Mortality of 618 Amputations of the Thigh, Leg, and Arm, without Etherization, performed during the last few years in 30 British Hospitals.*

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Per Centage of Deaths.	No. of Cases.	No. of Deaths.	Per Centage of Deaths.
Thigh,	73	45	63	211	62	29
Leg,	80	26	32	135	23	17
Arm,	77	17	22	42	10	24
TOTAL,	230	88	38	388	95	24

GENERAL MORTALITY OF AMPUTATIONS OF THE THIGH, LEG, AND ARM UPON PATIENTS IN AN ETHERIZED STATE.

In the preceding lengthened Table, No. I. (p. 701), I have given from forty-nine different hospitals the detailed reports of 302 amputations of the thigh, leg, and arm. When these 302 amputations are reduced into a tabular form, similar to those which I have used for stating the data of similar amputations without ether, they present the following results:—

No. VII.—*Table of the Mortality of 302 Amputations of the Thigh, Leg, and Arm, under Etherization.*

Seat of the Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Per Centage of Deaths.	No. of Cases.	No. of Deaths.	Per Centage of Deaths.
Thigh	24	12	50	121	25	20
Leg	32	9	28	81	13	16
Arm	17	4	23	27	8	29
TOTAL	73	25	34	229	46	20

I shall now proceed to contrast these results with the results of the same operations in the same class of hospitals, and when performed upon patients not in an etherized state.

Before doing so, however, let me observe in passing, that the data I have adduced in Tables No. I. and V., have been objected to on the ground that they are collected from too many different hospitals, and too many different sources. But, on the contrary, I believe all our highest statistical authorities will hold that this very circumstance renders them more, instead of less trustworthy. Professor Chomel of Paris, after pointing out the first requisite for a successful statistical

comparison of therapeutic or other results—viz. 1. a sufficient similarity between the number of collated cases—adds, as the second condition, “that the data be numerous, collected at different times, in different places, and, if possible, by several observers. It is easily seen (he adds) that the results of a number of facts too limited, collected in a short space of time, in a single place, and by a single observer, however exact as regards that individual series of data, may yet be very different from, or even the reverse of conclusions drawn from a larger series, and one collected under various circumstances.”¹

COMPARISON OF THE MORTALITY FOLLOWING THE LARGER AMPUTATIONS OF THE LIMBS, 1. WITHOUT, AND 2. WITH ETHERIZATION.

The major amputations of the limbs, including those of the thigh, leg, and arm, are generally fatal in hospital practice in the proportion of about 1 in every 2 or 3 operated upon. In the Parisian hospitals, the fatality, according to Malgaigne, amounts to upwards of 1 in 2. In Glasgow, it is $2\frac{1}{2}$. In British hospitals, I found that under these amputations 1 in $3\frac{1}{2}$ died. The same operations, performed in the same hospitals, and upon the same class of patients, in an anæsthetic state, present a mortality of 23 in 100, or 1 in 4, only. The following table shows the amount of the individual cases, and the per centage of deaths in different collections, with the corresponding proportion of deaths in those operated on in an etherized state.

No. VIII.—*Table of the Mortality of Amputation of the Thigh, Leg, and Arm.*

Reporter.	No. of Cases.	No. of Deaths.	Per Centage of Deaths.
Parisian Hospitals—Malgaigne,...	484	273	57 in 100
Glasgow Hospital—Lawrie,.....	242	97	40 in 100
General Collection—Phillips,.....	1369	487	35 in 100
British Hospitals—Simpson,.....	618	183	29 in 100
<i>Upon Patients in an Etherized State,.....</i>	302	71	23 in 100

The evidence which the preceding table affords in favour of the greater safety of amputation with ether than without it, is sufficiently strong and striking. While 23 in 100 died under the amputations named, when the operations were performed upon patients in an anæsthetic state; 29 in every 100 died under the same amputations in the same hospitals when the patients were not etherized;—in the Glasgow hospital as many as 40 in 100 died; and in Paris, as many as 57 per cent. In other words, out of every 100 persons submitted to amputations of the thigh, leg, or arm, the lives of 6 were, by the employment of etherization, saved above the average number of the same operations in British hospitals;—17 lives in each 100 were saved, if we take the Glasgow returns as a standard of comparison; the average mortality was, under ether, less by 34 in every 100 cases

¹ Bulletin de l'Acad. Roy. de Médecine. Seance du Mai 2, 1837.

than that which was found by Malgaigne to accompany the same operations in the Parisian hospitals.

But probably, to most minds, this comparison would be rendered more clear and simple, if we took not a class of operations, but a single operation as a standard and medium of comparison. For this purpose, let us select amputation of the thigh as the *individual* operation regarding which we possess the largest series of observations.¹

COMPARISON OF THE MORTALITY FOLLOWING AMPUTATION OF THE THIGH, 1. WITHOUT, AND 2. WITH ETHERIZATION.

There are few or none of the operations deemed justifiable in surgery, that are more fearfully fatal in their results than amputation of the thigh. "The stern evidence (says Mr Syme) of hospital statistics shows, that the average frequency of death is not less than from 60 to 70 per cent;"² or above 1 in every 2 operated on die. Out of 987 cases of amputation of the thigh collated by Mr Phillips, 435 proved fatal; or 44 in every 100 were lost.³ "On referring (observes Mr Curling) to a table of amputations in the hospitals of London, performed from 1837 to 1843, collected with care by a private society to which I have the honour of belonging (the Medical Society of Observation), I find 134 cases of amputation of the thigh and leg, of which 55 were fatal, giving a mortality of 41 per cent."⁴ Out of 201 amputations of the thigh performed in the Parisian hospitals, and reported by Malgaigne, 126 ended fatally. In the Edinburgh Infirmary 21 died out of 43. Dr Lawrie found the mortality attendant upon this operation in the Glasgow hospital to amount to 46 deaths in 127 cases. In the collection of cases from 30 different British hospitals which I have published in table No. V., 284 cases of amputation of the thigh are reported; 107 out of these 284 operations proved fatal. On the contrary, I have collated 145 cases in which the same operation has been performed during the past year in British hospitals upon patients in an etherized state. Out of

¹ One objection may be urged against the comparison of the results of a single operation, with or without etherization, that I am now about to institute, on the ground, viz.—that the number of cases (145) is too limited to afford a result that is perfectly decisive. I am perfectly willing to admit the justice of this remark in a statistical point of view, and to hold this part (and indeed the whole of the present inquiry) as, so far, the commencement and nucleus merely of a more full and lengthened investigation by other hands. At the same time, I have, during the course of the inquiry, had the conviction impressed upon me, that future results will more and more confirm those that I have here stated in the text, and be still more in favour of etherization; for no small number of the operations reported to me were, in the first periods of the new practice, doubtlessly performed upon patients in whom the anæsthesia was by no means entire and complete, in consequence of imperfection in the forms of apparatus, in their management, in the dose given, &c.; and, I believe, that as the profession becomes more accomplished and certain in the use of such measures, the resulting effects will become proportionally happier and more favourable.

² Monthly Journal for May 1845, p. 337. ³ Medical Gazette for 1844, p. 805.

⁴ Address to the Hunterian Society of London, 1848, p. 31.

these 145 cases of amputation of the thigh, only 37 proved fatal. Or, in other words, the fatality was not greater than 1 in every 4 operated on when the patients were previously etherized. It was as high as 1 in every 2 or 3 operated upon when the patients were not previously etherized. The following table presents these results in a more clear form:—

No. IX—*Table of the Mortality of Amputation of the Thigh.*

Name of Reporter.	No. of Cases.	No. of Deaths.	Per Centage of Deaths.
Parisian Hospitals—Malgaigne,	201	126	62 in 100
Edinburgh Hospital—Peacock,	43	21	49 in 100
General Collection—Phillips,	987	435	44 in 100
Glasgow Hospital—Lawrie,	127	46	36 in 100
British Hospitals—Simpson,	284	107	38 in 100
<i>Upon Patients in an Etherized State,</i>	145	37	25 in 100

The preceding figures speak in a language much more emphatic than any mere words that I could employ in favour of anæsthesia, not only as a means of preserving surgical patients from pain, but as a means also of preserving them from death. Between even the lowest mortality in the table without ether, 36 in 100, and the rate of mortality with it, 25 in 100, there is the difference of 11 per cent. That is to say, according to this standard, out of every 100 patients submitted to amputation of the thigh without anæsthesia, 11 more would die from the operation than if the same 100 patients were submitted to the same operation in a state of anæsthesia. And if the condition of anæsthesia effects thus a saving of 11 lives in every 100 amputations of the thigh;—then out of every 1000 such operations the lives of 110 patients would be preserved by the use of antipathic means.

If we compare these results with the standard of Mr Phillips, the contrast is still more startling. Out of 987 amputations of the thigh collected by him, 435 proved fatal; or 44 in the 100. Out of 145 amputations of the thigh under anæsthesia, 37 proved fatal, or 25 in the 100. According to this comparison, the amount of persons saved from death in amputation of the thigh by the patients being rendered anæsthetic during the operation, amounts to 19 lives in every 100 operations performed.

In conclusion, let me add, that when etherization first began to be employed in surgical operations, it was eagerly argued that its adoption produced a greater tendency to primary and secondary hemorrhagy, to imperfect union of the wounds, to pneumonia, &c. If my space had permitted, it was my intention to show, from the analysis of the three hundred cases of amputation reported to me, that these various allegations were foundationless and imaginary,¹—that

¹ Some of my correspondents, who expressed the strongest opinions in regard to the reality of these supposed evil consequences, have, I know, now abandoned such opinions as utterly untenable.

such consequences were not so frequent after amputations with ether as after amputations previously performed without it,—that as the casualties were reduced in number, so were also the attendant accidents and complications.¹ But I believe such proof to be at the present day superfluous, as few or none now maintain such opinions. When writing to me as early as in June last on this subject, the late lamented Mr Liston stated what all the subsequent experience of our ablest surgeons here and in London has confirmed. “The ether (says he) produces no bad effect, as far as I can see. There is no change in the blood, nor in the vessels, or muscles. The recoveries are, at least, quite as good as before it was employed.” An excellent surgical pathologist (Mr Curling, surgeon to the London Hospital)² has more recently afforded still stronger testimony to the same effect. “I have carefully watched (says he) the progress of cases, after operations of various kinds performed upon patients in a state of anæsthesia, and I can with confidence declare that, so far as my present experience has reached, the constitutional symptoms have been milder, and the cases have proceeded more satisfactorily, than after operations in which no means had been taken to prevent pain. Several of my surgical friends can fully confirm this statement.”

I have also avoided entering into the theoretical question,—How does anæsthesia render severe operations less fatal and dangerous in their consequences? I have already shown (*Journal* for September, p. 164) that the endurance of severe pain is in itself depressing and destructive; and apparently the anæsthetic state saves the patient from this suffering and its effects; as well as saves him, in some degree, from the shock of the operation and its consequences. When writing, in 1839, on the subjects of pain and shock, and on certain states connected with or produced by wounds or injuries, Professor Burns of Glasgow offered some remarks bearing directly on the present subject, and which are more valuable as they were written without any theory, and without any prospect of such a state as he speaks of being capable of being artificially induced. I shall quote them in his own words:—“The mere lopping off of the member, by the immediate abridgement of the quantity of living body, the instant loss of so large a portion, which was formerly acting along with the system, is productive of serious evil to it, from the sympathy which universally prevails. But if the *nervous system become in part torpid, so as to prevent this sympathy*, or to be incapable of maintaining it, the loss of a member, or what is, in one respect, the same, the loss of its connexion with the system, and its failure in power, and action, and sensibility, may not have the same bad effect.”—*Principles of Surgery*, Vol. I. p. 493.

¹ In my communication to the Medico-Chirurgical Society, in June last, I went over this ground at some length.

² Address to the Hunterian Society of London, 1848, p. 23.

ARTICLE II.—*On the Diagnosis and Relief of certain Species of Amblyopia.* By HENRY GOODE, M.B., Cantab.

It is a singular fact, that the resources of optical science have hitherto been almost totally neglected by oculists, except for the relief of myopic or presbyopic vision. Errors which depend on malformation, either congenital or accidental, cannot be affected by any medical remedies; some of them, however, may be relieved by the appliances of the optician. As this application, however, requires more mathematical skill than most oculists or medical practitioners possess, the matter, up to this time, has been handled only by two or three scientific men, who were led to examine the question by defects existing in their own eyes, and producing dimness of vision.

The only structural error which has been successfully relieved, is that designated "*astigmatism*." An account of this error, and of the mode of correcting it, was published more than twenty years ago by Professor Airy, in the reports of the Cambridge Philosophical Transactions.

McKenzie has taken notice of this paper in his book on the Eye, and has added a few other instances of the same defect. From the paucity of instances adduced, it might be supposed that this particular defect was very rare. Such, however, I believe to be by no means the case, having myself, during the last year or two, met with no less than eight cases, merely by examining occasionally the eyes of such of my friends as I discovered casually to have an eye dim-sighted. A detailed account of some of these cases appeared in a paper I lately sent to the Cambridge Philosophical Society (See Vol. VIII. Part IV. No. 36). A person practising as an oculist, must, without doubt, meet with many persons who, possibly from their birth, may have had one or both eyes dim-sighted: such cases are not, by any means, indiscriminately to be pronounced incurable, under that convenient generic term amblyopic. In many, examination will show that the retina is in a perfectly healthy state,—the optical apparatus alone being defective. Possibly, in such cases, *astigmatism* may be found to be the prevailing error, as I have found it to be in those which I have examined.

As the name which has been applied to this error does not define it, but might have been applied with equal justice to any case in which the refracting surfaces of the eye deviated from their true sphericity, I will briefly explain it.

Let a luminous point be made, such as may be furnished by a pin-hole in a card held between the eye of the observer and the sky, or an illuminated sheet of paper. If such a point be viewed by an eye in which the optical apparatus is defective, the image of the point depicted on the retina will not be a point, as it should be, when placed at the proper distance, but will assume some form dependent on the nature of the error. In the particular error to which I refer,

when the card is held at some certain distance, the image of the point will be a straight line; when the card is removed still farther, the line gradually disappears; but the image becomes elongated in the form of a straight line again, in a direction perpendicular to that of the former. In some cases, the card must be so far removed, before the second line can be depicted, that no distinct image of that line can be obtained.

When any object is held in the position of the card at which the point appears as a line, either at the nearer or farther distance, those lines alone are distinctly visible which are parallel in direction to the line forming the image of the point at that distance; while those lines crossing the direction of this set of lines perpendicularly, appear broad and faint. If now the object be removed to the other distance, this second set of lines appear sharp and well defined, and the first set appear, in their turn, broad and dim. Such is the method of investigating this error.

It is to be observed, that some persons affected with this malformation, find diplopic vision of any minute object placed at a certain distance when the sound eye is closed.

The nature of the defect, as shown by these experiments, appears to be this:—If we take a section of the eye by any plane, the intersections of this plane with the refracting surfaces of the eye will be curved lines lying in this plane. Let a section be taken by a plane passing through the axis of the eye, and parallel to the line representing the image of the point at one distance; and again a second section by another plane, also passing through the axis, and parallel to the line at the other distance, and therefore perpendicular to the former plane,—the curvature of one or more of the refracting surfaces in one plane will be greater than the curvature in the other plane. The curvatures in sections made by any other planes passing through the axis, will be intermediate between these two curvatures. One or more refracting surfaces, therefore, instead of being parts of solids of revolution about the axis of the eye, must be of an ovoid form. In one case in which the error was very strong, I examined the curvature of the cornea, by observing the distortions produced in the image formed by reflection from it of a small luminous square, but could detect no want of symmetry about its axis. The error, therefore, probably exists in the lens, which may perhaps have suffered lateral compression before birth, as the error in this case was congenital.¹

The same effect might be produced by the lens being placed in an oblique position in regard to the axis of vision.

An eye with such a defect may be said to have two foci; the object to be effected is to unite these foci. This is effected by employing a lens shaped like a portion of a cylinder; such as would be ob-

¹ *Quære*—Is the form of the cornea spherical? Is it not rather a portion of an hyperboloid of revolution.

tained by cutting off a slice of a cylinder by a plane parallel to its axis. If we take a section of such a lens by a plane parallel to its axis, and again, a second section by a plane perpendicular to its axis, it is evident that in the first plane the section will show two parallel surfaces, and therefore no change in refracted rays; the second section will show a curvature on one side, and therefore the changes produced by refraction in the second plane may be calculated in the same way as in the case of an ordinary lens. Hence the curvature of the cylindrical lens may be computed, such as to cause rays diverging from one focus to appear to diverge from the other. Or, in other words, the two distances from the eye at which the point appears as the two lines, are the conjugate foci of the lens.

If the defective eye be furnished with such a lens applied close to it, and an object be placed at the distance at which one set of lines appear sharp and distinct, let the lens be turned so that its axis is parallel to these lines, it will produce no effect upon them; but the image of the second set of lines will be thrown to such a distance, that they also will become sharp and distinct, and hence a distinct vision of the object is obtained.

Professor Airy proposed to measure the distances of the foci in the following way. Let the card with the pin-hole be fixed so as to slide along a graduated rod, one extremity of which is applied to the cheek-bone, and the other directed towards the light; the card may then be slid along, and the distances at which the point has the appearance of the two lines successively, may be noted.¹

I have tried this and other means of measuring the distances; but I find that it is almost impossible to prevent the eye from changing its state of adaptation to distances during the course of the experiment; so that little certainty can be attained. I have found, however, that when a glass of the required strength was procured, such a glass could correct the error in all states of adaptation of the eye. Hence the error is independent of the state of adaptation; and therefore, by trying glasses of different strengths, in viewing objects at any distance, the suitable strength may be found.

I may here remark, that if the cornea undergoes a change during the adaptation of the eye to distances, as is probable, this would of itself be sufficient to show that the error of formation did not exist in the cornea in those cases which I have examined; and, therefore, was to be looked for in the shape or position of the lens.

In applying the lens to the eye, if the cylindrical surface is convex the axis of the cylinder must be turned so as to be perpendicular to the line at the nearer focus; if it is concave, the axis of the cylinder must be placed parallel to this line.

¹ Let u and v represent the two distances, and f the focal length of the lens, then $\frac{1}{v} - \frac{1}{u} = \frac{1}{f}$ = also $\frac{1}{2r}$ where r is the radius of the cylinder, the lens being of ordinary glass, and plane on one surface.

If a person applies a cylindrical lens to a sound eye, the appearances produced will be precisely the same as those produced by this malformation, and the more readily if the curvature is convex. This might be readily shown by a diagram. In order to correct the dimness of vision produced by such a glass, it will evidently be necessary, supposing a convex glass to have been applied, to apply either a concave glass of equal curvature, with its axis coinciding with that of the convex glass, which will obviously nullify its effect; or a second convex glass with its axis perpendicular to that of the applied glass, and of equal curvature. The two glasses in this position will combine to form a common magnifier, since both are convex.

I have not been able to hear of any person in Britain who makes glasses with cylindrical surfaces; there is, however, at Paris, a person by name Chamblant, who occupies himself solely in the construction of lenses with cylindrical surfaces, and has basins of all curvatures for convex or concave glasses. A glass being ground on the two surfaces in the cylindrical form, with the axis of the two cylinders crossing at right angles, produces a lens which acts with less distortion of the images than those with spherical surfaces usually constructed; they are employed for spectacles, eyeglasses, pocket-lenses, &c., where high powers are not required. It will readily suggest itself to my readers, that if a person affected with astigmatism be obliged to employ also convex or concave glasses, the curvature necessary to correct the astigmatism may be combined on one surface with the curvature otherwise necessary, the resulting curvature being computed by the ordinary methods of calculation in optics. I find that such a glass may be fixed in a pair of spectacles, with its axis in the requisite position, or may be worn conveniently enough as an ordinary eyeglass.

I have myself the misfortune to have an eye astigmatic; it appeared to me, when I first began to observe the eye, that much use for two or three months diminished the error of the organ, but I have not found any perceptible change for some time, since I have been possessed of a glass to remedy it.

An error, so great as to cause total incapacity for distinguishing objects, has been completely remedied by a glass of the nature described.

I have seen cases in which the image of the point was very different from those above described, one of which was sufficiently regular to warrant the expectation of relief; but as I was unable to proceed to the experiment, it is unnecessary to trouble the reader with the details.¹

¹ Professor Airy's first paper was entitled, "On a Peculiar Defect in the Eye, and on a Mode of Correcting it."—*Trans. of the Philosophical Soc. of Cambridge*. Vol. II. Part II. p. 267. His second paper is entitled, "On a Change in the State of an Eye affected with Malformation."—*Ibid.* Vol. VIII. Part III. p. 361.

ARTICLE III.—*Contributions to the Pathology of the Kidney.* By
WILLIAM T. GAIRDNER, M.D.INTRODUCTION.—ON THE NORMAL STRUCTURAL ANATOMY OF
THE KIDNEY.

THE researches of modern anatomists have shown, that the secreting substance of the kidneys may be considered as essentially consisting of the ramifications of a mucous membrane, which is disposed in the form of minute tubes, clothed internally with epithelium, and closely embraced by a network of capillary vessels. These tubes, which in the cortical substance have an exceedingly irregular and tortuous distribution, pass through the pyramids, forming straight converging ducts, which unite together two by two, and finally open on the surface of the papillæ, where their epithelial lining becomes continuous with that of the calyces, pelvis, and ureters. In every part of their course, these tubes are accompanied by vessels, the ramifications of the renal artery and vein being distributed copiously in the substance of the pyramids, and over the external surface of the organ; while the cortical substance is chiefly occupied by the capillary plexuses surrounding the tubes, and by the Malpighian bodies, which have been ascertained to consist of globular tufts of vessels arising from the arteries, but whose anatomical relations and function are still the subject of much discussion.

The labours of anatomists having been thus far successful in elucidating the normal structure of the gland, it became necessary for pathologists to follow in their steps, and by the same modes of investigation to trace out the changes induced by disease in those structures which had been found to be the most essential anatomical elements of the kidney. This branch of the investigation was first taken up by Valentin,¹ Hecht,² and Gluge,³ who from 1839 to 1842 published various microscopic observations on Bright's disease and other morbid conditions of the kidney. These became extensively known in Germany, and gave rise to several other contributions of much interest, but appear to have excited no attention in this country till after the publication of the papers by Dr Johnson and Mr Simon of London, in the *Medico-Chirurgical Transactions* for 1846 and 1847. It is proper, however, to state, that in 1842, Mr Goodsir addressed to the *Medico-Chirurgical Society* of this place a communication on the anatomy of the kidney, and the changes in Bright's disease, which was, so far as I am aware,

¹ Valentin's *Repertorium*, Bd. II. 290.

² *De Renibus in Morbo Brightii Degeneratis*, Berolin. 1839; and in Casper's *Wochenschrift*, 1839.

³ *Observationes Anatomico-Pathologicæ*.

the first contribution to the literature of that subject in this country; but of which an abstract only was published in the *Monthly Journal* for that year.¹

The researches which I am now about to publish, were at first undertaken, and indeed had proceeded a considerable way, with a very imperfect knowledge, on my part, of what had been previously observed by others. In consequence of this circumstance, I was led to the minute and careful investigation of some minor points in regard to the normal anatomy of the gland, which I might perhaps have neglected had I possessed the confidence which the examination of Gluge's drawings would have given me. Nevertheless, as no one appears to me to have observed and defined with sufficient exactness the *standard of health* in the human kidney with relation to microscopico-pathological researches, I have thought it might be desirable to introduce the strictly pathological part of this inquiry by a few observations on the healthy structure, premising that I mean to notice only such points as are important in reference to pathology, and by no means to attempt any thing like a complete account of the normal anatomy of the gland.

1. *On the Vascular System of the Kidney.*—It is well known to all who have given attention to the subject, that nothing is more variable, even in cases where no disease can be suspected, than the vascularity of the kidney. Nevertheless, there is no doubt that in some instances its abnormal character is most important as leading us to detect disease of the organ. It is therefore of some consequence to understand the conditions under which the most marked alterations of vascularity take place.

The veins of the kidney are disposed chiefly in two situations; viz. on its surface, and in the substance of the pyramids. The cortical substance contains exceedingly few. On the surface, they form a peculiar net-work, visible with the naked eye, and known to anatomists from the time of Ferrein, forming, by their intersections, the boundaries of small pentagonal or hexagonal spaces, in the interior of which the natural pale colour of the cortical substance appears, about the size of a very small pin's head (half a line). The larger veins are scattered over the surface, and tend to a stellate distribution. In this situation, the venous injection is liable to the greatest irregularity of distribution and amount (as has been well described by Rayer). The veins may appear in the highest degree distended, or, on the other hand, perfectly anemic, in correspondence with the degree of fulness of the general venous system; and no change is more frequent than the distension of veins on the posterior side of the organ, from gravitation of the blood. On the other hand, great irregularity of injection, amount-

¹ Dr H. Bennett informs me that he has, for several years, explained in his classes the views on steatorosis of the kidneys contained in Gluge's work; which he had also verified himself.

ing to marbling of the surface, and great increase in the size of the stellar vessels, are generally tokens of disease, as they are the result either of partial obliteration of the venous network, or of the extrusion of the blood from it through over-distension of the loops of tubuli, which form the intervening pale spaces.

The engorgement of the capillaries and Malpighian tufts give rise to two conditions :—*first*, a generally diffused heightened colour of the cortical substance; and, *second*, increase and greater distinctness of the vascular striæ running from the base of the pyramids to the external surface. This latter species of injection often exists, to a great extent, without any corresponding injection of the rest of the kidney, and, in some instances, the red points composing the striæ are so much increased in size as to form considerable petechiæ (one line in diameter or upwards), in which case the petechiæ usually extend to the surface, occupying the intervening spaces of the venous polygons above mentioned. This appearance was supposed by Rayer to occur from simple hypertrophy and vascular injection of the Malpighian bodies; but Bowman,¹ who has shown that the Malpighian bodies do not exist on the surface of the kidney, has also given a better explanation of such petechiæ, which he holds to arise from rupture of the Malpighian tuft, with extravasation of blood into the neighbouring tubes. He argues that the petechiæ are of irregular form, and of much larger size, than the Malpighian bodies have ever been observed to acquire. He gives, also, a figure, representing the occurrence of a similar appearance from artificial injection at the surface of the kidney. In this figure the loops or knuckles of the tubuli are seen filled with injection, presenting themselves at the surface, and surrounded by the venous network. The correctness of this explanation cannot be doubted, and it is therefore evident, that the occurrence of these petechiæ must be considered as invariably morbid.

The blanched and anemic state of the cortical substance is a very frequent condition. In many cases, no trace of red colour is to be seen, and the vascular striæ and points are wholly imperceptible; in others, a uniform light rose colour mingles with the pale yellowish tint peculiar to the renal substance, and the vascular points indicating the Malpighian glands are faintly visible. These conditions may coexist with abundant vascularity on the surface, and in the pyramidal portions of the kidney; and they have, since the publication of Dr Bright's researches, been looked upon by all pathologists as of the greatest importance in estimating the healthy and diseased states of the gland, particularly in reference to the granular degeneration. Rayer points out that inflammatory and other diseases of the kidney, are the most frequent of all causes of decoloration of the cortical substance; but he has also noticed the occurrence of anemia as an independent lesion in the kidney, and

¹ Philosophical Transactions, 1842.

says, with great justice, that both in the anemic and hyperemic conditions of the organ, the partial character of the vascularity is much more decidedly indicative of the presence of a morbid product than its absolute amount.

I have had numerous opportunities of examining, microscopically, kidneys in which the cortical substance was decolorized, both where this occurred independently, and where it was connected with abnormal deposits in the organ. In such cases, the Malpighian coils of vessels, which, in a strictly normal specimen, may be observed filling the capsule, particularly towards its circumference, with red injection, are pale, bloodless, and compressed, sometimes maintaining their rounded form,—at other times, more or less angular. Along with this condition of the Malpighian bodies, I have generally observed distension of the urinary tubules, either by morbid deposit, or by the accumulation of their own secretion. In the latter case, the kidneys have usually been above the normal size, and of more or less diminished consistence. On the other hand, in cases in which the kidneys have been about or under the usual size, and firmer in texture than ordinary (without morbid deposit), I have several times observed the amount of vascular injection to be greater than usual.

In considering these phenomena with reference to their cause, it is not difficult to show, that from the anatomical constitution of the kidney, the fulness of the urinary tubules must of necessity induce, as its first consequence, compression and emptying of the Malpighian vessels. For whether we adopt the view of Bowman, who asserts the capsule of the Malpighian body to be the dilated extremity of the urinary tubule—or that of Gerlach,¹ who regards it as a diverticulum—or of Toynbee,² who considers it as a separate membrane retaining the tubule and the vascular coil in contact with each other, we find that the close connexion of the Malpighian vessels with the urinary tube is maintained by the majority of modern observers (although denied by Reichert and Hyrtl). If this be admitted as probable, then it follows that fluid pressure arising within the tubules must fall back upon the Malpighian vessels. Moreover, from the exceedingly firm character of its fibrous investment, the kidney cannot be suddenly increased in bulk without considerable pressure being exerted on its substance; so that, as a consequence of the anatomical disposition of the gland, the sudden engorgement of its secreting tubes must necessarily be followed, even in health, by the diminution of its vascular supply. I shall afterwards have to adduce numerous instances of the occurrence of this in the pathological states of the gland.

On the whole, it appears from the analysis of the variations in the vascular system, that the most interesting of these, in reference

¹ Muller's Archiv. 1845, No. IV.

² Med. Chir. Tran., Vol. XXIX.

to pathological inquiries, are those of the Malpighian bodies; and that the varieties of the superficial venous plexus are of little consequence, excepting in the case where it is so unequally filled as to give rise to mottling or marbling of the surface.

2. *On the Tubuli Uriniferi.*—The tubes, within which the urine is secreted, are composed of an extremely delicate, translucent, and brittle membrane, the exterior of which is in contact with the capillary vessels, and the interior with a layer of nucleated cells. That these cells are intimately connected with the function of secretion has long been considered probable; and the researches of Goodsir comprise observations extending over so wide a series of secreting structures, and so apposite, as almost to amount to demonstration,¹ that the epithelium of the ultimate glandular ducts is the immediate agent in the process of secretion. Hence the pathological alterations of these structures have become of peculiar importance.

In the kidney of the human subject, the appreciation of the normal characters of the tubuli, and of their epithelium, is a task of no small difficulty. It is not always easy, especially in hospitals situate among the population of large towns, to find organs which can be relied upon as furnishing a standard of health; and, even in those which present no obvious marks of disease, the variations observable in a minute examination of the tubes, are so frequent and considerable, as to present the greatest difficulties to the unpractised observer. With the view of familiarizing myself with these variations, I examined, during nearly two months (with the kind concurrence, and frequently also the valuable aid of Dr Bennett), all the kidneys, with few exceptions, which were removed at the *post-mortem* examinations in the Edinburgh Royal Infirmary. The following results of this inquiry may be useful to those engaged in similar observations, by preventing the mistake of healthy for diseased conditions.

The lining membrane of the tubuli, which is the homogeneous or basement membrane of Bowman, is never seen in the fresh and healthy kidney uncovered by epithelium-cells, the nuclei of which are ranged, at pretty nearly equal distances, over its internal surface. In certain diseased states, and also as the effect of maceration, there may sometimes be seen in the kidney considerable portions of tube having a perfectly homogeneous character, and perfect transparency, with no appearance of structure. Much more commonly the tube is seen destitute of epithelium-cells, but retaining in its walls a few scattered oval nuclei, about one-third smaller than the nuclei of the epithelium-cells. These are the

¹ This is peculiarly evident from his observation on the testis of the *Squalus Cornubicus*, where the actual process of secretion may be said to take place under the eye.—*Anat. and Path. Observations*, No. V., and *Trans. Royal Society of Edin.* 1842.

young epithelium-nuclei of Bowman, the germinal centres of Goodsir. They appear imbedded in the substance of the membrane, and are very rarely separated from it even when, in diseased conditions of the kidney, the tube has ceased altogether to perform its function. Observations illustrative of these facts will be given in a succeeding part of this memoir.

The membrane of the tubuli appears to be possessed of considerable elasticity, so as to be capable of accommodating itself to the greater or less amount of secretion within them. In no case is it thrown into folds when the tubes are *in situ*, even when the calibre of the tube is very much narrowed. In the strictly normal kidney, however, the diameter of the tubuli varies much less than might be supposed, being generally, in all parts of the organ, from 1-25th to 1-15th of a millimetre. This is no doubt owing to the constant nature of the secretion, and the freedom with which it escapes as it is secreted, on account of which the tubes are not, like the ducts of the mammary gland, subject to alternate distension and relaxation.

The epithelium-nuclei of the tubule are, as above stated, in the normal state arranged at somewhat regular intervals on the inner surface of the membrane, the intervening spaces being occupied, and entirely filled up by the cell-walls, which, when *in situ*, assume an irregularly polygonal form from mutual pressure, according to the amount of distension of individual cells. The cavity of the tubule appears to be entirely filled up by these cells and by the secretion which distends them, and which, when freed, filters away between them.

The size of the nuclei is pretty constantly from 1-120th to 1-100th of a millimetre. They are circular, and have an extremely clear, well-defined edge, which is perfectly smooth when the kidney is fresh; but occasionally, from putrefaction or other causes, becomes slightly irregular, destroying the circular form of the nucleus. They appear quite flat by every arrangement of the light, and when seen sideways become oval or nearly linear. By transmitted light they have a slight uniform shadow, and present one or two central dark points, which, however, are not constant in their occurrence and position. I have not observed the nuclei to present the phenomena of endosmosis and exosmosis. The addition of water produces little change on them; acetic acid generally makes them clearer than before, but rather by dissolving away surrounding obscurities than by any change in the nucleus itself.

The cell-wall is extremely delicate, sometimes indeed so much so as to be scarcely visible, even with the most careful management of the light; but if a current be produced in the fluid, when the nuclei are floating free on the field of the microscope, the presence of the cell may always be recognised, even when it is most delicate, by its preventing the complete approximation of the nuclei to one another. In a certain proportion of the nuclei, also, it appears to be

absent even in the most healthy kidneys; and I have frequently seen organs presenting no other apparent change, in which the proportion of free nuclei was so large that it was difficult to find a complete cell among them. Of this circumstance, and also of the extremely different degrees of tenuity of the cell-wall, where it is present, I am not able to offer an explanation, further than that the latter seems to have a relation to the rapidity of development of the cell; inasmuch as when the cell-development is evidently sluggish, and the tubes obstructed with granular matter, the cell-wall is in the majority of instances denser than usual.

The size of the entire cell varies considerably; it being sometimes but little larger than the nucleus, while at others it attains a diameter of 1-50th or even 1-40th of a millimetre. Its shape, when free, is spherical; within the tubule, however, this shape is modified by the pressure of surrounding parts. When floating free in fluid, the cells frequently roll over, showing the position of the nucleus, which is attached to the side.

The fluid contained in the cells of the tubules, being in fact their own secretion, is, in the strictly normal state, perfectly transparent. Nevertheless, it is exceedingly common to find it clouded and rendered opaque by a minutely molecular deposit, which may be so abundant as entirely to obscure the nucleus, or may even appear distinctly granular, being at the same time scattered over the field of the microscope, and resembling very closely some of the morbid deposits to be hereafter noticed. This molecular shading of the cells is, in the greater majority of cases, owing to a deposit of lithate of ammonia, which is removed almost instantaneously by the addition of an excess of acetic acid. Such a deposit, when moderate in quantity, can scarcely be called morbid, as it takes place from the cooling of the urinary secretion under the most various circumstances, and without any other trace of the presence of disease.

It is not yet certain whether the act of secretion implies the disappearance and subsequent removal of the cell-wall, or whether the cell gets rid of its contents by a process of exosmosis, in the same way as by endosmosis it receives them from the vessels. But if the former view be correct, it is clear that the effete particles must be removed by the urine in a molecular form or in solution; as no epithelial debris of any kind can be detected in the tubes of a perfectly healthy kidney, and the existence of such debris is one of the most unequivocal and ordinary signs of disease. It is not at all improbable that the molecules of effete secreting epithelium may constitute a considerable part of that impalpable sediment which subsides from normal urine, and which is so fine as frequently to present, even under the microscope, nothing but a cloud of almost invisible molecules.

Whatever be the destiny of the cell-wall, the nucleus must be

regarded as a permanent structure, whose function is the perpetual renewal of the membranous cell-wall, and of its secretion. Accordingly, the nuclei have a greater power than any other part of the organ of resisting decomposing agencies; and they are never observed in the urine except when the tubes are the seat of disease.

3. *On the connecting Tissue or Parenchyma, and the general Structural Arrangement of the Kidney.*—Toynbee, who ascribes great functional importance to the parenchyma, describes it as consisting in part of peculiar cells, similar to those within the tubes, to which bloodvessels and nerves are distributed, and which he supposes may have the office of effecting some change in the blood preparatory to the secreting process. Bowman and Goodsir describe the different anatomical elements of the kidney as connected together by a delicate fibrous tissue, which forms a sustaining skeleton for the organ.

The general arrangement of the tissues of the kidney is readily seen by making careful sections through the cortical and tubular substance with Valentin's double knife. Where such a section is made through the cortical substance, the tubes are seen sometimes in section, and sometimes presenting to view more or less of their sides, enclosed in the areolæ of an extremely delicate and lax fibrous tissue, which is so disposed as completely to fill up the interspaces. Here and there a Malpighian body is seen surrounded by its capsule, and enclosed in an areola two to four times the size of that of the majority of the tubules. By rubbing the section between plates of glass, some of the tubes may often be displaced, leaving the areolæ clear and empty, and displaying the fibrous network unaccompanied by the other tissues. These appearances are represented in the woodcuts below, although it is extremely difficult to delineate this very delicate tissue without some degree of exaggeration.

Fig. 1.

Fig. 2.

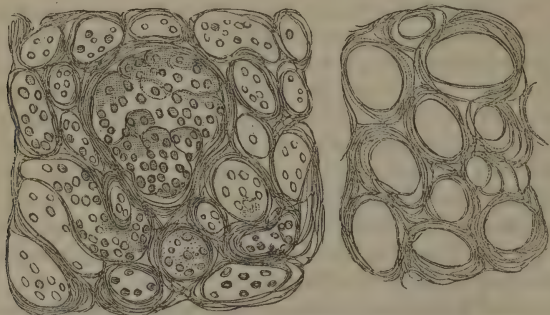


Fig. 1. Section of the cortical substance (by Valentin's knife), treated with acetic acid, showing the tubes and a Malpighian body, with the intervening delicate areolar tissue. The nuclei are seen scattered over the field, being brought out in strong relief by the acetic acid. Magnified 180 diameters.

Fig. 2. Portion of a similar section, from which the tubes have been squeezed out. The areolæ are seen empty. Magnified 180 diameters.

When the section is made through the striæ of the cortical substance, a divided bloodvessel is occasionally visible, and, whether filled with blood or not, is known by the large amount of fibrous tissue which enters into its walls. In the pyramids a similar structure is seen when they are cut across the axis of the tubes; but, as might be expected, the fibrous tissue is much more abundant, from the greater number of large vessels included in the section.

When the capillaries are distended by natural or artificial injection, they are seen to form a close network round the tubes, running in the midst of the intertubular areolar texture above mentioned, and almost filling up the intertubular spaces. Indeed, so much of these spaces do they occupy, that I think it not improbable, that the whole, or nearly so, of the delicate fibrous tissue alluded to, is made up of the walls of the capillary plexus of vessels. Mr Goodsir and Mr Bowman, however, seem to consider it as an independent structure, the former regarding it as analogous to the capsule of Glisson in the liver.

As to the parenchymal cells of Mr Toynbee, I have not been able to observe them either in the healthy or diseased states of the kidney, and must hesitate about admitting their existence, especially as they are not described with such minuteness as to enable us to distinguish them from the epithelium of the tubes.—(*To be continued.*)

ARTICLE IV.—*On Erosion, or Notching of the Teeth.* By DR REID, Dentist to the Merchant Maiden Hospital, Edinburgh.

THE appearance on teeth known under this designation, or that of “decay of the teeth by denudation”—“removal of the enamel by the denuding process”—“abrasion of the enamel,” &c., has been described by many writers on dental pathology; but no one has as yet attempted to assign a positive cause for it; consequently, so long as that remains unknown, no available preventive or remedial means can be adopted. To those who will take a comparative view of the different authors treating of this subject, it will become evident that there are two distinct appearances; and thus there exists considerable discrepancy in the accounts given by them. Hunter¹ is the first who has taken notice of the affection. He describes it as “a want of enamel, whereby the bony part is left exposed, but neither the enamel nor the bony part alter in consistence;” and again he says,² he had met with cases “where it appeared as if the outer surface of the bony part, which is in contact with the under surface of the enamel, had first been lost; so that the attraction or cohesion between

¹ Practical Treatise on the Diseases of the Teeth, p. 24.

² Ibid. p. 25.

the two had been destroyed." There is so much discrepancy betwixt this and the former description as to leave no doubt that either applies to a separate affection; and this opinion is confirmed by Bell,¹ who says, "Mr Hunter describes very accurately the result of superficial absorption of the bony structure, a circumstance which I have occasionally seen, though more rarely than the present abrasion of enamel, with which it cannot for a moment be considered as identical. In one case the enamel is gradually and slowly removed by a regular and uniform excavation; in the other, the abruptness and irregularity of the edges show that it had broken away at once, from having lost its subjacent support. The cause in the former is external, in the latter it is within the enamel." That Bell is here no less in error than Hunter, does not admit of doubt. He ascribes the breaking away of the enamel to absorption of the subjacent bone, whereas it is the effect of caries of the bone; and this view is supported by Harris,² who sweepingly disposes of all assigned causes, while he avoids giving any solution of the difficulty himself.

The exposition of the appearance, as given by Fox,³ corresponds with that first described by Hunter, saying, "it consists in a removal of the enamel from the bone of the tooth, as if by solution and gradual abrasion."—"The first appearance is in the enamel of one or more of the incisors becoming thinner, and appearing as if a small portion had been scooped or filed out, occasioning a slight depression."—"In other cases, there is an appearance as if a small round file had been applied to the anterior surface of the teeth close to the gums, removing a considerable portion of them, but leaving the surface exceedingly smooth." He confesses his inability to assign any cause for these appearances, conjectures that the saliva has some influence in dissolving the enamel, and that the friction of the lips may contribute to its removal. If such was the case, the whole surface of the tooth would be equally affected, instead of presenting a grooved appearance.

It will thus be seen that there are two distinct conditions of the teeth recorded by dentological writers; and without going into the descriptions of other authors, which could only be a repetition of the quotations already given from those that are acknowledged to be the best writers on the subject, let us proceed to examine into the cause of these appearances. In the condition last described by Hunter, it would appear that there is a predisposing or constitutional tendency, aided by an artificial cause. In some constitutions there exists a want of density in the bone of the teeth, which will be most frequently observed in those of a strumous diathesis; and this peculiarity placing them more under the influence of external causes, whether mechanical or chemical, when once an impression is made on the surface of

¹ Anat., Physiology, and Diseases of the Teeth, p. 188.

² Practical Treatise on Dental Surgery, p. 207.

³ Natural History and Diseases of the Human Teeth. Part II. p. 54.

such teeth, decay commences, and proceeds steadily, if not rapidly, aided by the heat and moisture of the mouth, the acid condition of the saliva, and the effect of portions of the food that have lodged and become putrescent in the dental cavities. The bone of such teeth becomes softened under these influences, wastes away, becoming excavated under the enamel, and thus, in the words of Hunter, "the attraction or cohesion between the two is destroyed;" or, as Bell expresses it, "the enamel has broken away at once, from having lost its subjacent support. It may be contended that this condition is the result of local constitutional causes, a late author (Robinson) having stated, that "the labours of recent pathologists seem to show that an acid liquor is secreted by the glands of the gums contiguous to the necks of the teeth;" but before entertaining such speculative views, we would ask the question, if the condition alluded to be the result of physiological structure, how is it that the under teeth are affected in the same manner, unless the acid secretion, contrary to the laws of nature, flows upwards? In that condition first described by Hunter, and designated by other writers as notching, there appears to be no decay or disorganization of the teeth; but the appearance presented is as if a file had been applied to the front of the tooth close to the gum, and cut into it a groove, or in some cases a notch, with the deep side next the cutting edge of the tooth, the surface of which is even and highly polished. The teeth which appear to be most extensively notched are the canines superior and inferior, also the bicuspes; yet in several cases that have come under my observation, the whole of the teeth partook more or less of it. This circumstance led me to suspect that it was occasioned by friction, and my enquiries into the habits of those whose teeth were more strongly marked brought out the fact, that undue use of the brush, aided by gritty dentifrices, was the sole cause; and, in corroboration of this statement, I may say that I never met in with the notched appearance in the mouths of those who, from want of attention, allowed calcareous deposits to accumulate on their teeth, but have found it in the mouths of such only as are scrupulously clean in their habits as regarded their teeth. It is a well-established fact, that a very short time will suffice to make an impression on a tooth by means of a brush, if water is used with any rough powder. Many authors could be quoted in support of this; but one will suffice, namely Berdmore, who in the space of an hour took the greater part of the enamel off a tooth by brushing it with a very hard brush moistened and charged with a certain dentifrice. It may be asked why are the canines most deeply grooved; and the answer will suggest itself to any one who will look at the position of those teeth in the mouth. They stand prominently forth, and form, as it were, the keystone of the arch described by the five teeth on either side of the mesial line; thus, in the use of the tooth-brush, they come in for much more than their share of the scrubbing process. I have already alluded to the theory that an acid liquor is secreted by certain

glands in the gums; but it cannot be supposed that the notched appearance is the result of chemical action either constitutional or external, and any arguments to prove the existence of such action may be set at rest by reference to the features of the notch as described by various authors. They all agree that it presents a smooth *highly polished* surface, and such a condition can be produced by mechanical action only, erosion being the result of chemical action; and the belief in the existence of a chemical agent has led those authors to employ the term "erosion" instead of "decay." Moreover, a similar appearance has frequently been found by me on the teeth of those wearing badly constructed artificial pieces; the clasps of which, having cut into the teeth, produced a notch or groove, the surface of which was smooth and polished, although the shape of the cavity did not, in every instance, resemble that of the appearance above mentioned.

Among the many substances made use of as dentifrices, there is, in my opinion, none so mischievous in its effects as charcoal; and in support of this assertion I could quote from many authors who have written clearly and distinctly on the subject, among whom are Berdmore, Plenck, Murphy, Fuller, Duval, Fitch, Lefoulon, &c. On inquiry among those whose teeth present the above appearance, I find that in almost every instance they have at some period of their lives made use of charcoal. In one instance that lately came under my notice, some of the teeth were cut half-way through, and the profuse use of charcoal was acknowledged to have been the cause. There may be other substances employed that are equally active in the effects, yet they are not used in the concentrated state, while charcoal is, its powers of attrition being very considerably increased by high pulverization.

In denouncing the use of charcoal or other gritty substances as dentifrices, I wish not to be understood as disapproving of the practice of brushing the teeth; on the contrary, I would advocate it as highly conducive to their preservation, and to the comfort and cleanliness of the individual, that the brush be used at least once a-day. The occasional use of a carefully prepared tooth-powder, free from gritty particles, could do no harm; but its use might be dispensed with by those who are in the daily habit of brushing their teeth. In support of my views, I quoted the opinion of Berdmore as to the effect of gritty matter on the teeth, and the statement appears somewhat extraordinary; but I have fully tested the accuracy of it, and have preserved, for the inspection of those curious in such matters, specimens of teeth notched to various depths, similar to those described by the above-mentioned author, and which has been accomplished in the space of fifteen minutes, solely by the friction of a common tooth-brush moistened with water, and charged with finely pulverized charcoal.

I do not take to myself any merit for originality in my views as to the effects of charcoal or other gritty substances on the teeth; but I

trust I have satisfactorily traced, to their undue use, the cause of appearances, to account for which has baffled the attempts of the best writers on dental pathology.

ARTICLE V.—*Cases in Surgery.* By JAMES SPENCE, Surgeon.

CASES OF STRANGULATED HERNIA.

CASE I.—*Congenital Hernia.*

J— M—, aged eighteen, a coachmaker by trade, whilst engaged at his usual occupation overstretched himself, and immediately felt severe pain at the lower part of the belly. On putting his hand to the part he felt a hard swelling, and became sick and vomited. He managed to walk home with great difficulty, and sent for Dr Cowan, Rankeillar Street, at whose request I visited him about four hours after the accident, Dr Cowan having already tried the taxis and other measures to relieve him without success. On examination, I found a tense elastic swelling extending from the inguinal region into the scrotum on the left side; there was evidently a considerable quantity of fluid in the scrotal portion of the swelling, the testicle of that side could not be felt, and there was only very slight impulse communicated to the tumour on coughing. The patient said, "That when a child there used to be something the matter at that part, but he did not recollect much about it, as he had not been troubled with it since he grew up, only the testicle had never been down on that side." As my attempts to reduce the hernia by the taxis failed, I proceeded to operate. On opening the sac, a large quantity of serous fluid escaped, and then I found a small knuckle of intestine tightly constricted, situated at the upper part. On dividing the resisting edge of the external abdominal ring, and passing my finger upwards, I felt another very tight stricture at the neck of the sac, which I divided, and returned the bowel into the abdomen. On this being done, the testicle came into view, lying within the inguinal canal, in such a position that its serous surface must have been in contact with that of the intestine, the posterior layer of the hernial sac investing the gland forming the tunica vaginalis testis; in other words, the intestine lay within the tunica vaginalis, which was still open towards the abdomen, and continuous with the general sac of the peritoneum. I drew the testicle and chord gently downwards into the scrotum, and, after bringing the edges of the incision together by sutures, I bandaged the patient in the usual manner, and directed him to have an opiate draught immediately, and a dose of castor-oil in the morning, if the bowels were not opened previously. Next day I found he was going on favourably, the bowels having been freely opened, and no rapidity of pulse or tenderness of abdomen being present; I removed the stitches on the third day after the operation, when I found the wound had nearly healed by the first intention; the testicle could be felt at the upper part of the scrotum, but sufficiently far down to be removed from the pressure of the pad and bandages. From his parents, who had now come to town, I learned, that the absence of the left testicle had been noticed at his birth, that it had never since come down, and that, when a child, he had occasionally a swelling at the part, which they then attributed to "wind." Every thing went on well till the twelfth day after the operation, when, on visiting him to get a truss fitted for him, I found him complaining of an acute throbbing pain in the scrotum, which he said had commenced about two days previously; the lower part of the scrotum was inflamed and tense, with evident fluctuation. Accordingly I made an incision into it, and gave vent to a quantity of pus; this afforded him immediate relief, the part soon healed, and he left Edinburgh completely cured at the end of four weeks. As I was anxious to watch the future progress of the case as re-

garded the development of the testis, I gave him a note to a medical friend in Glasgow, but he did not apply to him ; during the short time, however, that he was under my own observation, there was no apparent increase in the size of the testicle.

Remarks.—This case is interesting, in as much as operations in cases of congenital hernia are not very common, but more especially as calling attention to some points in the diagnosis of such cases.

It will be noticed that the local appearances were different from those usually met with in hernial tumours ; the scrotum was not only tense and elastic, but evidently contained fluid like a hydrocele ; whilst the testicle (which can always be felt, in ordinary herniæ, behind and below the hernial tumour) could not be found, and the impulse on coughing was by no means the direct, distinct impulse which we generally feel in hernia.

In this instance, the history of the case, and the urgent symptoms of strangulation, left no doubt as to the presence of hernia, and consequently of the proper course to be pursued ; but suppose that, instead of this, the protrusion at the upper part had occurred more gradually and without strangulation, and the patient had applied on account of the swelling in the scrotum, the surgeon, finding a smooth, elastic tumour, transparent to light, like a hydrocele (which in one sense it really was), the testicle not to be felt as in ordinary scrotal hernia, and no distinct impulse on coughing, might mistake it for an ordinary hydrocele ; and this shows the necessity of careful examination in every case, first, to ascertain distinctly the position of the testicle, and also to inquire carefully into the previous history of the case ; for if, content with the evident transparency of the scrotal swelling, the surgeon were to draw off the fluid and inject as for hydrocele, in such a case as I have supposed, I need scarcely remark how disastrous the consequences would, in all probability, be.

CASE II.—*Strangulated Femoral Hernia.*

On the 1st December 1846, I was requested by Dr Menzies to visit Mrs C., who had been suffering under symptoms of strangulated hernia since two P.M. on the afternoon of that day. I found on my arrival, that the vomiting had somewhat abated, although still continuing occasionally, and that her bowels had not been opened since the symptoms commenced. Her pulse was eighty-six, and soft ; the belly not much distended, but the hernial tumour felt tense, and there was twisting pain at the umbilicus before the attacks of vomiting. She stated that she had noticed the swelling for several years ; but as it gave her no uneasiness she took no notice of it until about two years ago, when a medical gentleman who was attending her for another complaint, told her its nature, and advised her to wear a truss ; but it resisted all attempts to reduce it then, and, as it gave no trouble, she thought no more of it until the present attack. I again attempted the taxis, but ineffectually. As the symptoms did not seem urgent, whilst the history of the case showed that the hernia had been irreducible for some years, I considered a short delay admissible in case the symptoms might be dependent upon some abdominal irritation unconnected with the hernia, as had occurred to me in a case of which I have given an account in a former collection of hernial cases. I accordingly ordered an opiate draught to be taken, and a sinapism to be applied over the epigastrium, and left her, desiring to be sent

for should her symptoms become more violent. I saw her early next morning, when I found that, although the vomiting and pain had been relieved by the opiate during the night, her pulse had increased much in frequency, and the hernia and abdomen were more tense and painful, and her countenance more anxious. Under these circumstances I recommended her to submit to the operation without further delay; it was only with great difficulty she gave her consent, so that it was nearly one P.M. before the operation was performed, being about twenty-four hours from the commencement of the attack.

On performing the operation, the parts over the sac were found matted together and adherent. On opening the sac, a portion of intestine of very dark port-wine colour was exposed, tightly constricted; I divided the stricture, and after separating some slight adhesions between the intestine and sac about its neck, returned the bowel, dressed the wound, and bandaged her in the usual manner. An opiate was then given; in the evening I found the vomiting had ceased, but the abdominal tenderness still continued, and the bowels had not been moved. She was ordered an ounce of castor oil and a cathartic enema. As the pain in the abdomen increased during the night, Dr Menzies was sent for and bled her freely from the arm, and at my morning visit I found her somewhat easier, and her bowels had been freely opened, but still there was great tenderness of the abdomen, thirst, quick pulse, and hot skin, and occasional retching; leeches were applied to the epigastrium, and she was directed to have two grains of calomel and half a grain of opium every two hours. Next day she seemed much worse; there was great distension of the abdomen, and the feeling as if effusion had taken place; her face was of a deep yellow colour, and flushed on the cheeks; pulse 120, and sharp; tongue furred. A large blister was applied over the belly, and the calomel and opium continued; the blister rose well, and on the fourth day after the operation the mercury began to affect the mouth, and shortly afterwards she had a copious dark evacuation from the bowels. Next morning I found her much relieved, and her bowels had been opened freely several times with great relief; the tenderness of the abdomen was almost quite gone.

From this time she continued to go on progressively to recovery, although it was a considerable time before the abdominal effusion disappeared, and she required the cautious use of stimuli and nourishing diet to complete her cure, which was effected in about five weeks.

CASE III.—*Strangulated Femoral Hernia.*

On the 18th June 1847, I was sent for by Mr Falconer of Loanhead to visit Mrs F., aged sixty-four, who had been seized that morning with the usual symptoms of strangulated hernia. This patient had been the subject of a femoral hernia for several years, for which she had constantly worn a truss, and which she could always readily reduce when it came down; on the present occasion she had left off the truss, and the hernia came down whilst she was making some exertion, and she failed in her attempts to reduce it. Sickness, pain in the part and at the umbilicus, accompanied by vomiting, soon came on, and she sent for Mr Falconer, who tried to reduce it, but without success. I saw her about five P.M., when I found her with a quick pulse, anxious expression of countenance, and occasional vomiting; the tumour was small and very tense, but the abdomen was not much distended, nor very painful except just before the attacks of vomiting.

I attempted the taxis again steadily but very gently, as the peculiar tense feeling of the swelling showed that it was tightly constricted at the neck; the attempt to reduce it failed, and I then recommended her to submit to the operation at once. This she was very unwilling to do, as she thought the bad symptoms were not increasing; but on explaining to her and her family her great danger, she consented, and I accordingly proceeded to operate, assisted by Mr Falconer and Dr Keiller. The incision was made in the usual form, and nothing peculiar appeared until the cribriform fascia was divided, when a structure came into view which appeared like the hernial sac; on dividing the superimposed fascia more

freely, I found that this was not the sac, but consisted of two or three small cysts, in appearance not unlike very small grapes. On opening one of them, a small quantity of fluid escaped, and the interior was seen to be smooth, as if lined with serous membrane. I then punctured the others, and, on dividing another layer of condensed cellular tissue, exposed the sac; on opening it a knuckle of small intestine was exposed, tightly constricted, and of a dark colour. I then divided the stricture, reduced the intestine, and dressed the wound in the usual manner.

She felt immediate relief from the pain and vomiting, and expressed herself thankful that she had submitted to the operation. An opiate draught was given, containing forty drops of the solution of muriate of morphia.

On the third day after the operation I again visited her, and dressed the wound, and removed the sutures. Her bowels had been freely opened within twelve hours after the operation, and she had suffered no pain in the belly since then. She ultimately recovered in about three weeks, without any bad symptoms supervening.

Remarks.—I have placed these two cases together, as contrasting the advantage of early operation with the dangers to which even a comparatively short delay exposes the patient, although the symptoms may not appear to be urgent. This danger arises from peritoneal inflammation being lighted up before the operation is performed; and then, as in the case of Mrs C——, we have to combat the most violent form of that disease. Few patients have made a more narrow escape, and I consider that much of the credit is due to the unremitting attention and active medical treatment employed by Dr Menzies.

In both of these cases there will be noticed a feature of not uncommon occurrence, but one which may throw the surgeon off his guard; viz., the remission of the vomiting and urgent symptoms, whilst there is a very tight constriction, and the morbid action is really progressing. I have in my possession a preparation of a strangulated femoral hernia, where ulceration of the intestine had occurred above the stricture; and yet so slight apparently had the symptoms been, that the death was reported to be the result of general debility! And this could scarcely have arisen from the medical man overlooking the symptoms; for the woman had a large femoral hernia on the opposite side, for which she had formerly undergone an operation; so that she herself would have drawn attention to the symptoms had they been present in an urgent form.

In cases like that of Mrs C——, where the hernia has never been reducible, it becomes of course more difficult to judge whether the symptoms depend on strangulation or not; for such patients are as liable as other people, if not more so, to intestinal irritation and inflammation; and the case of this kind which I have already referred to as published in this Journal for August 1845, is a good example of this.

But whilst this warrants some deliberation, and cautious watching of the symptoms, and a little longer delay than in ordinary cases, lest we should find an operation had not been required, we must also recollect the great danger to which we expose the patient by undue delay; and for this reason I still adhere to the opinion I expressed in

my remarks on that case, viz. that "if, along with increased tension of the tumour, we find all the urgent symptoms of strangulation, I think that, in spite of occasional exceptions, such as the case I have detailed, the surgeon will act wisely for the safety of his patient, who, after using active preliminary treatment, proposes and urges the operation being performed."

CASE IV.—*Strangulated Femoral Hernia—Division of the Stricture, and Reduction of the Bowel without Opening the Sac.*

Miss R——, the subject of the present case, was first troubled with hernia on the 1st December 1846. On that occasion she was seized with violent vomiting and obstinate constipation, which she attributed to a bilious attack. On sending for Dr Menzies, he discovered a small femoral hernia, which he easily reduced by the taxis; but although the vomiting ceased on the hernia being reduced, violent pain in the belly supervened, and the constipation continued for some time. Dr Menzies mentioned the case to me as I happened to be attending another case of hernia with him at the time, and from his description of the continued severity of the symptoms, I feared that the bowel might have suffered at the constricted part before reduction. She, however, got better, had a truss fitted to the part, and continued quite well until the evening of the 1st December 1847, when she found that, the bandage having slipped down, the hernia had protruded, and she could not return it. Next morning she felt sick, and vomited whenever she took any fluid. In the forenoon she sent for Dr Menzies, who being ill, Dr Burn saw her, and tried to reduce the hernia by taxis, but failed in doing so. I saw her at his request in the afternoon, five P.M., and tried to reduce the bowel, but could not, though cold and other means were used. As the symptoms were not very urgent, and as Dr Burn had not arrived, I directed her to have an enema, and saw her again at half-past seven P.M.

Dec. 2, half-past 7 P.M.—The enema had come away without opening the bowels. She continues sick, vomits whenever she drinks; her tongue dry, pulse sharp and quicker than in the afternoon, and there is pain in the abdomen on pressure. The taxis was again tried after placing her in the warm bath; but without effect. After some persuasion she consented to submit to the operation, which I performed, assisted by Dr Handyside and Dr Kirk. I made an incision over the swelling, and divided the different coverings down to the sac. As it was my intention to try and relieve the stricture without opening the sac, I dissected the parts over the falciform process of the crural arch, which I divided carefully till I could pass the point of my finger between it and the hernia, and then divided it more freely; but still found it impossible to reduce the contents of the sac from a stricture situated apparently in its neck. As the stricture was evidently at one point merely, as I could draw down more of the sac above it, and as I was unwilling to open the sac if it could be avoided, I raised a thin layer of cellular tissue lying upon the sac, divided it horizontally, and touched the strictured part lightly with the point of my scalpel, when I found that the constriction had been caused by a few filaments of condensed cellular tissue closely adherent to the sac, which had escaped the knife in dividing the fascia propria. The sac was held firmly whilst I reduced its contents, which was now done without the least difficulty; the incision was then closed by sutures, and compress and bandage applied as usual. I gave her a draught of solution of muriate of morphia, thirty-five drops.

Dec. 3, 11 A.M.—Has passed a good night, slept well, pulse eighty-four, tongue more moist than yesterday; has had no more vomiting, no pain in the abdomen, bowels not opened since operation. To have castor oil.

Dec. 3, half-past 7 P.M.—Bowels freely opened by the castor oil, which was long in acting, and had caused some griping (probably owing to her repeating the dose thrice to obtain a motion); skin a little hot, pulse eighty, soft; no pain on pressing the belly, no sickness, headach, or vomiting; to have the opiate

draught at bed-time, and a sinapism so be applied if there is any increase of pain.

Dec. 4, half-past 11 A.M.—Slept well, pulse eighty, no tenderness of abdomen, tongue cleaning, bowels not open since last report. Dressed the wound, as the bandages had become loose. To have an enema to open the bowels in the afternoon.

6 P.M. (Dr Kirk's report).—Bowels opened by enema, no tenderness of abdomen, pulse eighty, tongue moist, and cleaning, much less thirst than yesterday. *Half past 9 P.M.*—Received a message to visit Miss R—, as she was suffering from uneasiness in the bowels, and was very restless. On going, I found that, shortly after sending for me, her bowels had again been freely moved, with great discharge of flatus, and relief from the pain, and since then she had been sleeping; her pulse was about ninety, bowels a little distended with flatus, but no pain on pressure, tongue clean and moist, expression of countenance natural. To have a dose of castor oil in the morning, and opiate draught to-night, if she was again troubled with uneasiness, and a sinapism to abdomen.

Dec. 5, half-past 10 A.M.—She had just had a motion from an injection, (for, in her anxiety to get her bowels opened again, she took not only the dose of oil ordered, but two more doses and the injection between the hours of four and ten A.M.); has passed a considerable quantity of flatus, and the injection has come off mixed with feculent matter; belly much less distended, no pain on pressure, tongue moist and cleaning, pulse ninety, but evidently excited by her exertion, less thirst than yesterday. I cautioned them against further giving any medicine or injections without special orders, or until the medicine had time to act, and directed that she should take nothing more till I saw her again.

9 P.M.—Dressed the wound, and removed two of the sutures; wound looking well, pulse eighty, skin natural appearance; no pain on pressing abdomen, but some flatulent distension of bowels, though less than formerly; general appearance favourable. Her sister stated, that she is always in the habit of using large doses of purgatives to obtain action in the bowels; she herself says, that she has suffered much less uneasiness and pain since the operation than she did last year after the hernia was reduced by taxis; she has never had the least tendency to vomit since the operation. I left orders to give her no medicine to-night except she was suffering much from flatulent distension, when they might administer an enema containing a teaspoonful of tr. of assafoetida.

Dec. 6, 11 A.M.—Slept well, and has had two evacuations, which are liquid, but natural colour; tongue moist, has had a good deal of cough, for which Dr Menzies had ordered her to apply a sinapism. To have twenty drops of morphia if any uneasiness of the bowels continued, as she complained after the last evacuation, but there is no pain on pressure.

Dec. 7.—Pulse continues more rapid, ninety-eight to a hundred, and a good deal of cough and pain in chest; tongue moist and clean, belly somewhat distended with flatus. To have a stimulant expectorant mixture, a blister applied to chest,—

Pil. Colocynth, gr. vi.

Assafoetide, gr. ii. ex. hyosciami, gr. iss. To be made into two pills, to be taken immediately.

Dec. 7, 8 P.M.—Pulse quick and not so regular, occasionally intermitting; pills have not acted. To have a small injection containing a table-spoonful of turpentine.

Dec. 8, half-past 10 A.M.—Pulse intermitting, sometimes at three and generally at every ten beats, does not expectorate freely, tongue still moist, has had a motion from the injection; as far as the frequency of pulse can be judged of, it is about 114 per minute. Under these circumstances, Dr Menzies and I judged it proper to use stimulants, and accordingly she was directed to have a teaspoonful of brandy in a little water every hour.

Dec. 9.—Pulse still weak and irregular, breathing rather easier, less pain, tongue clean and moist; has had an evacuation, and passed much flatus after a

small assafœtida injection. To have beef-tea or chicken broth, and to increase the stimulus to a teaspoonful of brandy and a dessert-spoonful of wine every half hour.

P.M.—Pulse improved in regularity, but frequent and weak; dressed the wound, which is not looking so well; there is want of action in it, tongue moist, no distension of bowels.

Dec. 10.—Dr Menzies saw her this morning in my absence; she seems much sunk, but the pulse does not intermit, and the heat of the skin is good; tongue moist and clean; had passed a little feculent matter in bed without knowing it, complained of headach. Dr Menzies ordered cooling applications to the head, and to continue the stimuli.

3 P.M.—I saw her with Dr Menzies; she seems very low and weak, but her headach is easier, answers questions sensibly; pulse 120, weak, but no intermissions; has passed water freely. To have spt. etheris. nitrosi. and ammon. aromat. thirty drops every two hours, and to continue the brandy and wine.

Dec. 15.—Has been rather improving for some days; bowels have been freely opened, and evacuations of natural appearance; pulse still frequent and weak, but no intermissions; tongue clean and moist, and general expression of countenance good; the wound exhibits a want of action; has no appetite for food. To have sulph. quinine, gr. 1, every forenoon; stimuli and diet as formerly. She continued much in the same state till the 18th December, when she sunk rather suddenly, and died at ten A.M. I had seen her the preceding afternoon, and her pulse at that time, though weak, did not intermit in the least; her tongue was moist and clean, and the expression of her countenance very little changed; but during the night she became restless, with difficult breathing, and after continuing in this state for some time, she gradually sunk and died, as I have already mentioned, about ten A.M.

Remarks.—I have given the particulars of this case in detail exactly as they stand in my notes taken at the time; and my reason for doing so is, that the plan of operating which I adopted in this case, not having been very generally practised hitherto, and many prejudices still existing against its revival, I wished to enable the profession to judge how far the fatal result of this case was connected with the operation, or particular plan of operating. At the same time, I think there can be little doubt that the result was not dependent on the operation; but that the patient died from the then prevailing epidemic, which was so fatal to old persons even when uncomplicated with other disease; and I consider that it affords strong evidence in favour of the particular plan of operating, as the patient repeatedly stated that she had suffered less after the operation than when the rupture was reduced by taxis, whilst the post-mortem examination showed that there had been no peritonitis or other organic affection of the abdominal viscera.

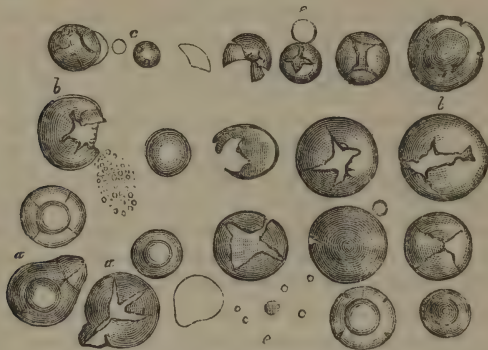
ARTICLE VI.—*Case of Peculiar Vesicles observed in Urine.* By SPENCER THOMSON, M.D., Haunton, Burton-on-Trent.

IN the *Monthly Journal of Medical Science* for March 1848, p. 661, Dr Bennett has given figures of some "very peculiar bodies" observed by him in a cancrroid tumour taken from the cerebellum of an aged subject.

Dr Bennett describes these bodies as "round or oval in shape," and as varying "in size from the 1-100th to the 1-20th of a millimetre in diameter." "Solid, transparent, with dark abrupt edges, shadowed gradually towards the centre." Direct pressure caused them to crack in various ways, but generally in a radiated manner, from the centre to the circumference, with two or several segments. —(See figures 58, 59, 60, p. 661.) Examined by Dr Douglas Mac-lagan and others; incineration destroyed the investing membrane, and left a white mineral ash, which still retained the original globular form of the body; this ash was found to consist principally of phosphates. Dr Bennett remarks that the true nature of these bodies "is involved in some obscurity, although it seems certain that nucleated cells have been formed which afterwards became impregnated with mineral matter."

The following may perhaps assist the elucidation of the real nature of these bodies, which I believe to be identical with some observed by me in the urine of a patient suffering from very extensive scrofulous abscess of the thigh. The similarity will at once be perceived on comparing Dr Bennett's figures with those I made from the urine vesicles. The mode of rupture of the investing membrane is precisely similar; but the vesicles observed by me, instead of being filled with mineral matter, were in an active state of reproduction, and, on rupturing, discharged numerous granules or secondary vesicles.

Fig. 1.



The following is a brief account of the case, and of the vesicles, taken from notes made at the time.

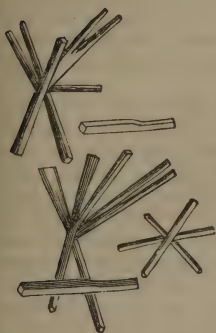
1846.—David Sidwells, æt. twenty-four. Temperament strumous; hair and eyes dark.

In the spring of the present year did some hard digging, since when he has suffered deep-seated pain in the thigh, and been unable to extend the leg fully. About the beginning of August had an attack of British cholera, followed by gastric fever. On recovering, the symptoms from the thigh attracted more attention, the limb became more painful, swollen, motion more impeded. September 10.—Deep-seated fluctuation being detected, a trocar was passed

two inches deep into the substance of the thigh, and about a quart of thick pus evacuated; in seven days more another quart, in six days three pints more; after this a permanent drain was established. On the 11th of October phlegmon came on; on the 14th, the greater part of the integuments and of the superficial cellular substance of the thigh was in a state of slough; in a few days more the patient died. No examination of the body could be obtained.

During the progress of the case the following observations were made on the urine:—About the 23d of September the urine was loaded with pink urate of ammonia; this disappeared in a few days, the secretion remaining clear, but dark-coloured. October 4th, a portion of urine was passed in my presence. Muddy when passed; colour moderately deep; specific gravity 1.015; sub-acid. Deposit copious, flocculent, white, amorphous, unaffected by heat; soluble with bluish effervescence in dilute acids. Clear supernatant urine rendered muddy by heat;

Fig. 2.



on the addition of acetic acid the transparency restored. On slightly heating the clear urine, bubbles of gas are quickly disengaged, and the slight acid reaction lost. October 6th, urine of 4th re-examined; reaction neutral; iridescent pellicle formed on the surface, containing a few scattered crystals of triple phosphate, and truncated needles of urate of magnesia.—(Fig. 2). This specimen of urine examined under the microscope was found to contain slightly coloured, transparent, perfectly globular vesicles of various sizes (see Fig. 1). Put aside in a covered vessel. October 12th, the yellow transparent globules or vesicles have become considerably altered; very many of them have burst, or rather appear to have been torn open by some elastic force, and to have discharged a quantity of minute granules.—(Fig. 1, *b*). Previous to rupture the investing membrane becomes prominent or gibbous at one or more points (Fig. 1, *a a* and *c*), and it appeared to me as if the rupture was effected by means

of an elastic ring (Fig. 1, *b*), similar to that which tears open the thecæ of many ferns; but of this fact I could not make myself certain. The granules or cells evidently increased in size as represented in various stages.—(Fig. 1, *e e*).

The above described bodies appear to me to be identical with those described by Dr Bennett, the only difference being such as might be compared to that which exists between the living plant or animal, and the fossil.

Perhaps it is not unworthy of notice, that the urine in this case contained a considerable proportion of free carbonic acid, to which its slight acid reaction at first was owing, and by which it was enabled to retain in solution phosphates and carbonates of both lime and magnesia.

ARTICLE VII.—*Contributions to Pathology and Rational Medicine.*

By JOHN HUGHES BENNETT, M.D., F.R.S.E., Lecturer on Pathology and the Practice of Physic, Director of the Poly-Clinic at the Royal Dispensary, Edinburgh, &c.

NO. XIV.—CLINICAL AND HISTOLOGICAL RESEARCHES ON CANCEROUS AND CANCROID GROWTHS.—(*Continued.*)

THE following case appears to me exceedingly important from the

circumstance that, without the slightest vestige of cancerous formation, a tumour in the leg ran the exact course, and presented, both before and after amputation, all the appearances which practical surgeons have been in the habit of considering as characteristic of "malignant disease." It occurred in the practice of Mr Norman of Bath, and I am indebted to Dr Davies of that city for the following particulars of it.

OBSERVATION XXIII.—*Cancroid Tumour of the Leg—Amputation—Recovery.*

A young woman, about twenty-five years of age, delicately formed, and of small stature; limbs somewhat bent in consequence of imperfect ossification when young; her general health had been tolerably good, though far from robust; she had been subject to frequent but irregular attacks of intense headach with intolerance of light and sound, generally lasting for two or three days, and then terminating rather suddenly, uninfluenced by any particular kind of treatment, and unconnected with any irregularity of the menstrual discharge, or any disturbance of the alimentary canal.

About thirteen months before the limb was amputated, there appeared a small puffy swelling on the inner side of the gastrocnemius muscle of the left leg; the tumour continued for several months to increase very slowly, and gave very little inconvenience. About six weeks before the operation the tumour was the size of a hen's egg, and yielded to the touch an indistinct sense of fluctuation, so as to induce Mr Norman to consider it as a collection of matter situated beneath the fascia. A small puncture was made, but nothing flowed out except a little blood. The opening healed kindly; but, shortly thereafter, an erysipelatous blush spread over the limb below the seat of puncture, which point subsequently became involved, reopened, and gave issue to a little bloody discharge; the skin sloughed, and a dark-looking fungous mass protruded. Part of this mass sloughed off (some of which was the first specimen I sent you), which was followed by a considerable oozing of blood. From this time the tumour increased very rapidly, thrust its foul bleeding surface beyond the level of the sound integuments, and at the end of a week from the time the first portion sloughed off, the limb was amputated above the knee by Mr Norman. The stump healed rapidly and perfectly; and the patient is at the present time (Jan. 8, 1848) in as good health as she has ever enjoyed.

The appearances presented on making a section of the limb, including the tumour, were as follows:—Externally there was a layer of dark-coloured matter, hard and glazed, about an eighth of an inch in thickness, obviously coagulated blood. Then came a mass of white, or rather pinkish-white, brain-like looking matter, intersected in various parts with streaks of blood. Internal to this mass, in one half of the section, there was a quantity of healthy pus separating it from the muscle; in the other half, it lay in contact with the muscle. The tumour was the size of an ordinary orange.

The muscular structure in which the tumour was situated, appeared paler than the other muscles in the neighbourhood. This paleness extended upwards and downwards for some distance in the course of the muscular fibres, but did not cross the nearest intermuscular septum.

The case has gone on well since amputation.

On the 15th of March, Dr Davies informs me that the patient still continues in good health,—better, indeed, than she had enjoyed for some time previous to the removal of her leg. Since the general wound of the stump healed, there have been a few pustules, or small abscesses, appearing on the face of the stump from time to time; but when they have discharged their matter they heal kindly, and produce no other ill effect than slight annoyance.

The portion of the tumour first sent to me for examination, was contained in a bottle of spirit, and its structure, therefore, could not be determined. The second

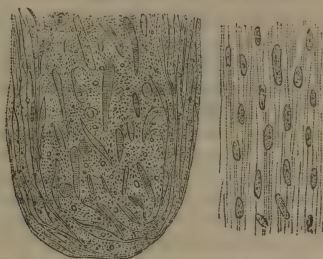
portion was fresh, and consisted of a soft, pulpy, cream-looking mass, about an inch square, readily breaking down under the fingers. I exhibited it to Drs Alison and Duncan, both of whom pronounced it to be part of a cerebriiform tumour, to which it bore the closest resemblance.

Microscopic Examination.—A small portion of the soft, pulpy matter, simply squeezed between glasses, was seen, under the microscope, to consist of fusiform corpuscles in different stages of development, mingled with a multitude of molecules and granules, and a few blood globules—Fig. 62.

Some corpuscles were of oval form, others elongated and caudate, others truncated at one end, or spindle-shaped. None exceeded the 1-200th of a millimetre in breadth; and in length they varied from the 1-100th to the 1-25th of a millimetre. Several contained a minute nucleus in the form of a granule, about the 1-500th of a millimetre in diameter. On the addition of acetic acid, most of the loose molecules and granules were dissolved, and a faintly fibrous structure produced, in which oval bodies were scattered, varying, in their longest diameter, from the 1-100th to the 1-50th of a millimetre.

Fig. 62.

Fig. 63.



Remarks.—Although the local lesion in this case was considered by all who saw it to be cancerous, and although the morbid alteration exactly resembled a so-called malignant structure to the naked eye, it became evident, on a microscopic examination, that it was really innocent in its nature. There were none of the cells we have seen to be present in undoubted cancerous growths, and no kind of structure which, in accordance with our present views, could be supposed to have the power of self-development. The whole consisted of an imperfect fibrous structure, composed of fusiform corpuscles, more or less broken down, which had arisen either in a quantity of coagulated exudation, or in the clot following a hemorrhage. The former is the more probable, as we are told that the tumour, small at first, gradually increased in size, which it could only have done from fresh additions of exudation.

Without presuming to criticise the practice in this case, which under all the circumstances was doubtless most proper, we may be permitted, in order to throw light upon any similar one occurring in future, to ask the following questions. Had the true nature of the local lesion been positively ascertained before amputation, would the operation have been performed? and if not, what other means of cure could have been adopted with rational hopes of success? The reply to these queries we for the present leave to the consideration of practical surgeons.

ARTICLE VIII.—*Case of Dislocation of the Astragalus.* By JONATHAN TOOGOOD, M.D., Torquay.

IN the eleventh volume of the Transactions of the Provincial Medical and Surgical Association, my friend, Mr Turner of Manchester, has published an elaborate and excellent paper on dislocation of the astragalus, in which he states that it is an accident of unfrequent occurrence, but, in its characters and consequences, extremely serious. Every additional example, therefore, of the successful treatment of this important accident, cannot fail of being acceptable to the profession. The following case occurred lately in the practice of my son, Mr J. B. Toogood, with whom I attended the patient, who thus describes it :—

I met with the accident (dislocation of the astragalus) about five months since, and, after having set it, Mr Wilkieson and his assistant bound it up in thick pasteboard splints. On the third day he wished to look at it; but, as soon as the bandages were loosened, my foot turned slightly round. On the following day, he thought it necessary to bind it up in wooden splints, and it remained thus for a fortnight; but during the interval spasms commenced, which gradually turned my foot round, until a dislocation again took place, followed, as before, by a resetting. It was then bound up in a starch bandage, which unfortunately proved too tight, and, owing to the great swelling and discoloration, the bandage was obliged to be cut open, and the foot left comparatively loose for two or three days. After the swelling had sufficiently subsided, it was tightly bound on a thick wooden splint, to which was attached a piece of wood the size and form of the foot, and it remained there until my arrival at Torquay, the 16th of September 1847.

It was now that the spasms came on with such intense violence as to break the splints and dislocate the bone; and although every means were tried to keep it in its place, all were found ineffectual until it was determined to strap it down to a surgical bed, and keep up permanent extension. It was with the greatest possible trouble, and the exertion of great ingenuity, that it was now kept in its place, and the spasms then gradually became less frequent, and, after a time, entirely ceased. After keeping it strapped down for a month, it was loosened by degrees, and a starch bandage applied; but this not proving sufficiently strong, a shoe was invented, made partly with iron, with steel supports up the leg, and a screw attached to a pad immediately over the astragalus, which may be worked down at pleasure. The leg was now unstrapped, and, after a few days, I found that I could stand with ease, and walk with very little support. I was advised to rest it entirely for another month, at the expiration of which time I found I could stand without the slightest pressure from the screw. At present, the bone feels perfectly in its place,—quite as much so as the other foot; and I can walk with ease without any other support than an elastic sock and such a boot as I have usually worn. I do not feel the slightest pain, spasm, or inconvenience, and I can conscientiously affirm that the cure is complete.

RYDE, ISLE OF WIGHT, December 18, 1847.

This case is an interesting one, and the accident happened in a singular manner. The patient was a muscular lady, aged twenty-four. She had had a severe attack, which made it necessary, in the opinion of her medical adviser, to take repeated doses of opium, which so completely narcotized her as to excite the greatest alarm for her life. In this state she was forcibly dragged about her room,

and it was not until some time after all apprehension for her safety had subsided that the accident was discovered.

I did not see her on her first arrival here; but, as the case proved a very intractable one, my son called me to his assistance. My impression was, that although the dislocation might have been reduced at the time, the bone had never remained in its proper situation; and I imagine that this was also her conviction, for I observed on my first visit that the foot was turned round, and I learned that, however securely it was bound up, the splints and bandages were speedily torn to pieces by the violence of the spasms.

On removing the bandages it was evident that it was a dislocation forwards and outwards. The spasmodic action of the muscles was so violent as to render all attempts at reduction hopeless, and could only be compared to those which occur in tetanus or puerperal convulsions. She was put under the influence of ether, and the bone was replaced without difficulty; but it was not until she was placed on one of Earle's beds, and the leg firmly fixed below the knee, and permanent extension made from the foot, and kept up for a long time, that it could be returned in its normal position. But even then the tendency to displacement was so great, that if the foot had not been secured in the iron shoe, and the screw applied over the dislocated astragalus, it would not have been possible to have conducted the case to a favourable termination.

I have just been informed that this lady had lately been at a ball, and danced without the least inconvenience.

TORQUAY, *February* 16, 1848.

ARTICLE IX.—*Case of Laceration of the Cæcum, and Rupture of the Bladder from External Violence.* By WILLIAM M'LEAN, Esq., Surgeon, Kilmalcolm.

PETER ARMOUR, aged sixty, a labourer of dissipated habits, laboured for a number of years under severe attacks of dyspnœa. On the 7th October 1847, while driving a loaded cart, the animal, being spirited, became restive, and, in giving the animal the whip to increase its progress, suddenly sprung forward and ran off. He, still holding on by the halter, and being lame in one of the legs in consequence of a former injury of the right ankle joint, was unable to retain his hold, in consequence of which the horse and cart, with its load, pressed him against the dyke, the wheel resting over the abdomen. He, however, regained his hold of the halter, and got partial command of the animal, it being at full trot for about two or three hundred yards, when he suddenly fell beneath the wheel, which passed over the abdomen, about an inch below the umbilicus, the cuticle being abraded over the whole of the hypogastric region. There was a lacerated wound of about two inches over the right tibia and knee joint, also over the occipital region, laying bare the pericranium. Immediately on receipt of the injuries, he passed both urine and fæces involuntarily. On being carried home and placed in bed, I was immediately sent for, about six o'clock in the evening, when I found him cold, pulse weak and tremulous, with urgent vomiting, hiccup, and cold perspiration over the whole body. I ordered warmth to be applied to the extremities, and the application of a dozen leeches to the abdomen;

administered thirty drops of the Sol. Mur. Morphię in cold water; but all was unavailing. He lingered in great agony until seven o'clock morning, when he expired.

Permission being granted to inspect the body, I proceeded to do so twenty-four hours after death. On opening the abdomen, the peritoneum was found thickly covered with effused blood; the belly contained about a pint of fluid mixed with blood. On turning over the intestines, the cœcum came into view, when it was found torn in two separate places, to the extent of two inches,—the one tear being distant from the other for about three inches; there was a rent at the fundus of the bladder about an inch long. On examining the liver, it was found in a very diseased state, being easily torn with the fingers. Throughout the whole tract of the intestinal tube, there were traces of high vascular excitement.

I regret that, owing to the impatience of the relatives, the other viscera were not inspected. The examination even of the above was not so minute as I could have wished.

It is surprising, owing to the severe lesions of the viscera, that he survived so long. With all its defects, being, as I conceive, rather rare in occurrence, I humbly submit this case to the profession.

KILMALCOLM, 16th February 1848.

ARTICLE X.—*Notes on the Anæsthetic Effects of Chloride of Hydrocarbon, Nitrate of Ethyle, Benzin, Aldehyde, and Bisulphuret of Carbon.* By J. Y. SIMPSON, M.D., Professor of Midwifery in the University of Edinburgh.

DURING the last few months two or three different substances have been mentioned as additional anæsthetic agents; but our medical journals have afforded little or no detailed notice of their effects. The few following notes, however imperfect, may not therefore be uninteresting; more particularly as they are the result of direct experiments upon myself and others with the agents in question. In most of these experiments, I had the kind and able assistance of Dr Keith and Dr Duncan.

When first publishing, in November last, upon the anæsthetic properties of Chloroform, I stated that, "in making a variety of experiments upon the inhalation of different volatile chemical liquids, I have, in addition to perchloride of formyle, breathed chloride of hydrocarbon, acetone, nitrate of oxide of ethyle, benzin, the vapour of iodoform, &c. I may probably (I added) take another opportunity of describing the result."—(See *Lancet* for 20th November 1847, p. 549.)

Three of the substances which I named in the preceding list, produce, when inhaled, a state of anæsthetic insensibility:—viz. chloride of hydrocarbon, nitrate of oxide of ethyle, and benzin.

CHLORIDE OF HYDROCARBON.

Chloride of Hydrocarbon, or *Dutch liquid*, as it is often termed in consequence of it being first discovered by the Dutch chemists of the last century, is one of the various fluids to which the name of *Chloric Ether* was for some time given.

When equal parts of olefiant gas and chlorine are mixed together, the two gases rapidly disappear, and produce a colourless oily liquid, of a peculiar sweetish taste and ethereal odour. Its specific gravity is 1.247. It boils at 148° . It is composed of four atoms of carbon, four of hydrogen, and two of chlorine. Hence its formula is $C_4 H_4 Cl_2$.

When its vapour is inhaled, the chloride of hydrocarbon causes so great irritation of the throat, that few can persevere in breathing it for such a length of time as to induce anæsthesia. I have latterly, however, seen it inhaled perseveringly until this state, with all its usual phenomena, followed; and without excitement of the pulse, or subsequent headach. When I myself attempted to inhale the chloride of hydrocarbon, it produced an extreme degree of acrid irritation in the throat, which did not disappear entirely for many hours afterwards.

NITRATE OF ETHYLE.

When two parts of alcohol, and one part of pure nitric acid, are distilled together, with the addition of a small quantity of urea, *Nitrate of Ethyle*, or, more properly, *Nitrate of Oxide of Ethyle*, is produced. It is a transparent colourless liquid, with a sweet taste, and very agreeable odour. Its specific gravity is 1.112; it boils at 185° . It is a compound of four proportions carbon, five of hydrogen, six of oxygen, and one of nitrogen; and its formula is, $(C_4 H_5) O, NO_5$; or $Ac O, NO_5$.

Nitrate of ethyle is easy and pleasant to inhale, and possesses very rapid and powerful anæsthetic properties. A small quantity, such as fifty or sixty drops, when sprinkled on a handkerchief and inhaled, produces insensibility after a few inspirations. But during the brief period which elapses before the state of complete anæsthesia is induced, the sensations of noise and fulness in the head are in general excessive; and much headach and giddiness have usually followed its employment, and persisted for some time.

BENZIN.

Benzin or *Benzole* was first discovered by Faraday, as one of the products in his experiments on compressing oil gas, and was designated by him *Bicarburet of Hydrogen*. Mitscherlich afterwards obtained it by distilling, at a high temperature, benzoic acid with an excess of slaked lime.

It is a clear colourless liquid, of a peculiar ethereal odour; with a specific gravity, of 0.85; and boils at 186° . It is believed to be composed of two proportions of carbon and one of hydrogen. Its formula is, $C_2 H$; or perhaps, more properly, $C_{12} H_6$. It is polymeric with the hypothetic radical formyle.

In my experiments with benzin I found it capable of producing anæsthesia; but the ringing and noises in the head accompanying and following its inhalation, were so excessive, and almost intoler-

able in the case of myself and others, as to seem to us to render its practical applications impossible, even had there been no other objections to its use. Latterly, Dr Snow has tried its employment upon some patients for tooth-drawing; and in one instance of amputation. In this last case it produced convulsive tremors.—(*Lancet* for 12th February 1848, p. 180.)

ALDEHYDE.

Aldehyde, or Hydrate of Oxide of Acetyle, was first noticed by Doebereiner in distilling together sulphuric acid, alcohol, and peroxide of manganese; but it was left for Liebig to fix and determine every thing about its chemical nature. It is a colourless limpid liquid, of specific gravity 0.791. It is very volatile, boiling at 72°. It spontaneously changes when long kept, and is converted into two substances, a solid and a fluid, metaldehyde and elaldehyde. Liebig found it to be composed of four atoms of carbon, four atoms of hydrogen, and two of oxygen; and its formula is $C_4 H_3 O + aq.$

Professor Poggiale of Paris, has lately made some experiments with dogs on the inhalation of the vapour of aldehyde, and from these has concluded that its anæsthetic effects will be found more prompt and energetic than those of sulphuric ether or chloroform. It certainly possesses, like some of the preceding agents, well-marked anæsthetic properties; but it assuredly will never come into use, as very few will be found capable of inhaling a sufficient dose of its vapour. In fact, out of five of us that attempted to inhale aldehyde, very carefully prepared and purified, four were driven to suspend the respiration of it in consequence of the coughing and insufferable feelings of dyspnœa which it immediately induced. The sensations of difficult respiration and constriction in the chest which the vapour produced, resembled precisely those of a severe fit of spasmodic asthma. In the fifth case, the experimentalist, after perseveringly breathing the aldehyde for a minute or two, became entirely insensible; the state of anæsthesia lasted for two or three minutes; during it, the pulse became excessively small and feeble. On recovering, the bronchial constriction and coughing, which had disappeared as the anæsthesia was induced, returned immediately, and was annoying for some time.

BISULPHURET OF CARBON.

Bisulphuret of Carbon, or Alcohol of Sulphur (as it was at first termed), was accidentally discovered in 1796 by Lampadius, when experimenting on iron pyrites. Different opinions of its composition were held by different chemists; but Berzelius and Marcet, in 1813, at last fully confirmed the previous idea of Clement and Desormes, Vauquelin, &c., that it consisted only of sulphur and carbon. It is composed of two atoms of the former to one of the latter; consequently its formula is CS_2 .

The most easy method of procuring it is by transmitting the vapour of sulphur over fragments of charcoal heated to redness in a closed porcelain or iron tube. The resulting bisulphuret of carbon, when purified by distillation, is a clear, colourless liquid, of a pungent taste. Its specific gravity is 1.272. It is very volatile, boiling at 108°.

It has been stated in various literary journals, that bisulphuret of carbon has lately been used as an anæsthetic agent at Christiana; but no particulars regarding its employment in Norway have, as far as I know, been yet published.

I have breathed the vapour of bisulphuret of carbon, and exhibited it to about twenty other individuals, and it is certainly a very rapid and powerful anæsthetic. One or two stated that they found it even more pleasant than chloroform; but in several it produced depressing and disagreeable visions, and was followed for some hours by headach and giddiness, even when given only in small doses. In one instance I exhibited it, with Mr Miller's permission, to a patient, from whom he removed a tumour of the mamma. It very speedily produced a full anæsthetic effect; but it was difficult to regulate it during the operation. The patient was restless in the latter part of it; but felt nothing. Like several others when under it, her eyes remained wide open. After the operation she was extremely sick, with much and long-continued headach; and, for fifty or sixty hours subsequently, her pulse was high and rapid, without rigour or symptoms of fever.

I tried its effects in a case of midwifery, in presence of Dr Weir, Dr Duncan, Mr Norris, and a number of the pupils of the Maternity Hospital. It was employed at intervals during three quarters of an hour. The patient was easily brought under its influence, a few inspirations sufficing for that purpose; but it was found altogether impossible to produce by it the kind of continuous sleep attending the use of chloroform. Its action was so strong, that when given, as a pain threatened or commenced, it immediately affected the power of the uterine contractions, so as often to suspend them; and yet its effects were so transient that the state of anæsthesia had generally passed off within a minute or two afterwards. The patient anxiously asked for it at the commencement of each pain. During its use she was occasionally sick, and vomited several times. Latterly her respiration became rapid, and her pulse rose extremely high. I then changed the inhalation for chloroform, and, under it, the patient slept quietly on for twenty minutes, when the child was born. During these twenty minutes there was no more sickness or vomiting, and the pulse gradually sunk down to its natural standard; and a few minutes after the child was expelled, and while the mother still slept, her pulse was counted at 80. Next day the mother and infant were both well, and she has made a good recovery.

While these experiments prove the strong anæsthetic properties of

bisulphuret of carbon, they at the same time show its disadvantages. I have not alluded to another strong drawback upon its use, viz., its very unpleasant odour. "It has (says Dr Gregory) a peculiarly offensive smell of putrid cabbage."—(*Outlines of Chemistry*, p. 130.) By dissolving various essential oils in the bisulphuret I tried to overcome this disagreeable defect, but without much success.

None of the five anæsthetics which I have mentioned in the present communication are, I believe, comparable with chloroform or sulphuric ether, either in their manageableness or in their effects. And the after-consequences which all of them tend to leave, are too severe and too frequent to admit of their introduction into practice. They are more interesting physiologically than therapeutically.

Part Second.

REVIEWS.

Physiologisch-Pathologische Untersuchungen über die Erscheinungen an den Arterien und Venen. u. s. w. Von Dr JOSEPH HAMERNJK. 8vo. Pp. 314. Prag. 1847.

Physiologico-Pathological Researches into the Phenomena of the Arteries and Veins, &c. By Dr JOSEPH HAMERNJK. Prague: 1847.

THE practical character which the author claims for his researches, cannot be appreciated without some preliminary knowledge of his peculiar doctrines regarding the circulation of the blood, and it will save much repetition if we at once lay before our readers a sketch of his theory on this subject.

From experiments with Poiseuille's hæmadynamometer, made by Magendie and Mendelssohn (Berlin, 1845), he concludes, that the respiratory movements of the thorax exercise upon the propulsion of the blood a power far superior to that of the heart's contractions. So little importance indeed does he attach to the structure of the heart, that (p. 17) he expresses his conviction, "that the circulation may go on if the pulmonary semilunar valves alone exist, provided the veins at the periphery of the thorax be closed during expiration." We find constant allusion made to these veins, and to the mechanism by which their closure is effected. The jugular and subclavian veins, where they unite to form the brachio-cephalic trunks behind the sternal extremity of the clavicle and the first rib, are firmly fixed in their position by fasciæ, and provided with a sufficient valvular apparatus, whose object is to prevent regurgitation from the vena cava superior. The vena cava inferior, the azygos, and azygos minor, are firmly connected to the diaphragm at the points where they perforate the muscle. During the ascent of the diaphragm, Dr Hamernjk believes, that its muscular fibres are so disposed as to

close the azygos and compress the inferior cava, and that what remains of the calibre of the latter vessel is shut by the pressure of the liver. Now, as all these veins are securely attached to the periphery of the thoracic cavity, it is argued, that when this is dilated by the art of inspiration, the vessels must be proportionately elongated, and, as a physical consequence, must suck in blood from their tributaries external to the chest; that, when the capacity of the thorax is again diminished during expiration, the vessels must be shortened, their contents subjected to increased pressure, their valves all closed, and the blood consequently propelled into the right auricle. Hence the auricle (which, along with the *venæ cavæ*, may be regarded as a reservoir of blood for the use of the right ventricle) becomes highly distended during *expiration*, and its contents pass into the right ventricle with considerable force, effecting the diastole. The diastole must not be regarded as dependent upon the *muscular* contraction of the auricle, nor upon the *active* dilatation of the ventricle. The latter clumsy theory can only be defended by supposing the heart to resemble a caoutchouc bottle, with a muscle outside: the former is at variance with direct observation. During inspiration, the cavity of the thorax enlarges, the veins and right auricle become more capacious, the diaphragm and liver cease to obstruct the inferior cava, the above-mentioned venous valves are opened, and blood rushes into the right heart from the veins external to the chest. It is by the stream thus created, that the diastole of the right ventricle is effected during *inspiration*. The velocity, or force of this stream, is due partly to the *vis a tergo*, but chiefly to the enlarged capacity of the auricle and great veins within the chest, which causes a species of suction towards the heart, termed by our author *aspiration*. The blood which during a diastole enters the right ventricle, exercises a greater pressure upon the parietes of that cavity during expiration than during inspiration,—at least when these acts are performed in a normal manner. The systole of the right auricle provokes, or is immediately succeeded by that of the ventricle, whose contents pass into the pulmonary artery. We must next examine the action of the movements of the thorax and lungs upon this vessel and its branches. During inspiration, the dilatation of the lungs may cause some rarefaction of the air within the chest,—so trifling however in amount, that its consideration may be totally disregarded, (p. 23). Proportionate however to the dilatation of the chest and lungs, is the elongation and consequent increased capacity of the pulmonary arteries and veins, (p. 27). There is thus created a force of suction or *aspiration* in the direction of the lesser circulation precisely analogous to that which we have already described in the *venæ cavæ*. During expiration again, as the dimensions of the pulmonary vessels become diminished, their contents are subjected to an increased pressure; and any retrograde movement of the blood being prevented during the systole

by the tricuspid, during the diastole by the pulmonary semilunar valve, its course in the direction opposed to these valves, *i. e.* towards the left ventricle, is consequently facilitated. It is obvious, that when the valves of the right heart are in a healthy condition (p. 29), the suction or "*aspiration*" induced in the pulmonary vessels by the movements of inspiration can have no direct influence upon the blood in the right auricle and *venæ cavæ*; it can only operate by removing from the pulmonary vessels a part of the pressure to which they are subjected, and thereby promoting the flow of blood from the right auricle.

Dr Hamernjk believes, that neither auricles nor ventricles can contract so perfectly as to expel *all* their contents (p. 32), that on the contrary the quantity of blood propelled into the arteries at each systole is very variable, and depends chiefly upon the degree of *pressure* under which the fluid moves in consequence of the respiratory movements of the chest and lungs. The pressure being greatest during the act of expiration, it follows, that at this period the propulsive force of the heart acts with greatest efficacy: when during inspiration the pressure is again lowered, the current of the blood experiences a proportionate retardation. "According to my view," says Dr Hamernjk (p. 26), "the action exercised upon the blood in the *venæ cavæ*, and in the lesser circulation by the expiratory movements of the thorax, is analogous to the action of muscles upon the circulation in the valved veins in their vicinity. If we follow out this analogy, and compare the thorax with its motor apparatus to a muscle, and the blood-canals within the chest to a valved vein, we shall find besides in the middle of the vein a particular mechanical contrivance, an accelerator, *i. e.* the heart, by which the action of the muscle is promoted."

In the above sketch, we have endeavoured in accordance with our author's doctrines to trace the course of the blood from the great systemic veins through the lesser circulation into the left ventricle of the heart. Analysis of the evidence on which this ingenious theory is founded, would in the ordinary compass of a review be impossible. As a specimen of the views to which its adoption may lead, we extract the following passage from the work of Dr Mendelssohn, altered, where the italics occur, by Dr Hamernjk.

"*The aspiration of the contents of the jugulars, &c., caused by the elongation of the cave*, explains the mechanism by which digestion and nutrition are connected with respiration; for the chyle and lymph are placed under its influence by the union of the thoracic duct with the subclavian vein. *By dilating the capacity of the chest*, we pump the material of nutrition into the blood; hence the thoracic duct is exposed like a twig of the subclavian vein to the entire suction influence *of the thorax*, which we may measure by inserting the nozzle of the hæmadynamometer into the jugular vein and directing it towards the heart. We shall find that the chyle must, during inspiration, be sucked towards the *venæ cavæ* with a force equivalent to about eighty millimetres of mercury. The fluid being thus abstracted from the lymphatic vessels, the advance of what passes into them by endomose can be explained, for we have a mechanical as well as a chemical cause for its motion. This moving force is important, for it demonstrates the mechanical connexion between respiration, digestion, and nutrition; it not only explains a

series of physiological and pathological conditions, but may render a complete revision of the received doctrines of nutrition and resorption necessary."—(P. 33.)

In confirmation of the view which represents the ventricular diastole as dependent upon the stream of blood induced by the respiratory movements, we find allusion made to the auscultatory signs of contraction of the left auriculo-ventricular orifice. It is well known that in certain cases of this pathological condition the diastolic murmur at the apex of the heart is a long protracted sound, which has in general two or three periods of remarkable intensity; indeed, these *crescendos* may be the only audible portions of the murmur. If they were always *two*, they might be accounted for upon Gendrin's theory, by supposing the diastole of the two ventricles to be completed at different times (see Skoda, 1842, p. 188); but their number is variable, and careful examination often shows that they merely mark periodic accessions of force in a continuous diastolic murmur. Their true mechanical cause, according to our author, is the varying force with which the blood is propelled into the left *ostium venosum* during the acts of inspiration and expiration; for as the velocity of the current is ever varying with the amount of pressure upon the *venæ cavae* and *pulmonales*, it may in its passage over the roughened margins of the orifice at one time induce a murmur, at another fail to do so. We are not quite satisfied with this explanation; for it does not account for the *regularity* with which the different morbid sounds occasionally succeed each other during successive diastoles of the heart, whether performed during inspiration or expiration: no corresponding regularity has been observed in the undulations of the great veins, and we are rather inclined to believe, that the real cause of the phenomenon must be the varying form assumed by the diseased orifice at different periods of the ventricular diastole. At the same time, we are far from disputing the nature of the mechanical cause to which Dr Hamernjk attributes the diastole.

We next proceed to the more practical part of our author's work, which treats of the arteries. These vessels are continually distended by a force dependent upon various *factors* (p. 42), *e. g.* the respiratory movements, the systolic force of the heart, the elasticity of the arterial tunics, the quantity of the blood, the condition of the capillaries, &c. If any one of these *factors* be altered, a corresponding alteration is effected upon the capacity (Umfang) of the arteries. Let us suppose, for example, that the arterial elasticity or contractility becomes diminished, while all the other factors remain unchanged, an increase in the capacity of the arteries will result. If again the volume of blood in circulation is suddenly lessened, as by profuse hæmorrhage or by an attack of cholera, the capacity of the arteries is diminished. The large pulse so frequently observed in acute diseases is, according to our author, a rough measure of increased arterial capacity. He endeavours to account for it by supposing, that in most diseases the distensibility of the vessels at ordinary pressures is augmented, and that it is perhaps assisted by

the increase of animal heat. He maintains (but merely on theoretical grounds) that no severe disease, except perhaps cholera, and certain profluviae and hæmorrhagies, can run on to a fatal termination without first occasioning considerable increase in the capacity of the arterial system; and, on the other hand, asserts that no improvement can be looked for in a case of acute and dangerous disease, until the arteries commence gradually to resume their ordinary dimensions. Under the influence of certain mental emotions, as anxiety or fear, of low temperatures, &c., the vessels become contracted in their calibre, while the agents which usually effect their distension continue to act with undiminished energy. Dr Hamernjk attributes this diminution of calibre to a "lessened distensibility at a given pressure," *i. e.*, to an increase of contractile or elastic force in the coats of the vessels. It may be objected to these doctrines that acute peritonitis is usually characterised by a small pulse, indicating a small degree of arterial distension; our author meets this objection by referring to the inactivity of the diaphragm and abdominal muscles in this disease, and argues that the arteries may be preternaturally *distensible*, but that the pressure to which they are exposed is, from the passive state of these respiratory muscles, more than proportionately enfeebled.

The abstraction of blood is, according to the observations of Parry (p. 40), at first attended with a contraction of the calibre of the arteries; but when depletion is carried to a certain extent, the *modulus* of the arterial elasticity seems to undergo a change, in virtue of which the vessels again become comparatively flaccid and distensible. Will this observation assist in accounting for the pulse of reaction after bloodletting, so well described by Marshall Hall, and to which we are surprised to find no allusion made in the work before us?

The blood in the arteries Dr Hamernjk regards as a constantly progressing current, experiencing considerable acceleration at each act of expiration, and slighter acceleration at each systole of the heart. The distension of the vessels consequently attains its *maximum* during expiration, and is little influenced by the comparatively trifling increment of pressure derived from the contraction of the ventricle. This view is confirmed by experiments made by various observers with Poiseuille's hæmadynamometer.

When the superficial arteries, such as the radial, are in a healthy condition, the finger is sensible of their presence only at the moment of their pulsation; when their form can be felt during the interval between pulsations there is some reason to suspect rigidity or disease of their tunics (p. 55). In chapters which treat of the pulse (p. 57 and 170) our author labours to prove, that what we perceive by a finger applied over a healthy artery never can be the progressive motion of its contents nor the locomotion of the vessel itself, but that the *pulse* consists in wavelike vibrations of the blood, propagated from the heart at each systole, and communicated to the coats of the vessels. He holds, that synchronously with these undulations

a change takes place both in the calibre and in the length of the artery—that the tube is widened, and at the same time elongated, and consequently thrown into curves. In meagre subjects this locomotion of the artery may often be *seen* through the integuments, and we cannot conceive the physical grounds on which Dr Hamernjk denies that it can be *felt*.

The remarks upon the interval observed between the pulsations of the heart and different arteries are full of interest. It has been long known that these pulsations are not synchronous, as the older physiologists believed; and it has been shown, that in certain diseases of the arterial system, such as incompetency of the aortic valves and aneurism of the aorta, the interval between the cardiac and radial pulsations is apt to be prolonged. Our author asserts, that in all cases in which the arteries are unusually distensible by ordinary pressure, the interval between the pulse at the apex of the heart and at the wrist becomes more appreciable (p. 81), and professes to have observed this phenomenon in chlorosis, scorbutus, typhoid fever, pneumonia, tuberculosis, puerperal fever, &c. In a few cases of typhus and *short fever* we have ourselves confirmed the accuracy of these observations. There is, however, especially when the action of the heart is rapid, great difficulty in judging accurately of the interval between the pulse at the apex and at the wrist; and we believe with our author, that medical science would be promoted were an instrument invented capable of measuring and recording such fractions of time. Weber has shown, that if the arteries were rigid tubes no retardation of the pulse could be observed, and thinks that it depends upon the elasticity of the vessels. “In consequence of the arteries admitting of some extension, particularly in length, the impulse given to the blood by the heart distends, first, merely the arteries nearest to the heart. These, by their elasticity, again contract, and thus cause the distension of the next portion of the arterial system, which also in its turn, by contracting, forces the blood into the next portion, and so on; so that a certain interval of time, although a very short one, elapses before this undulation, resulting from the successive compression of the blood, and the dilatation and contraction of the arteries, reaches the most distant branches of the arterial system.”¹

This view probably justifies the conclusion of Dr Hamernjk, that the *interval* and capacity (*Umfang*) of an artery bear a direct ratio to each other, and to the distensibility of the artery at a given pressure. Hence, had we an instrument which could measure the interval, it would give us direct information regarding the capacity of the vessel under examination. Arteries equidistant from the heart pulsate at the same moment, unless some pathological cause interferes. Thus, we are told that the pulsation of the iliac artery at Poupart's ligament is in general synchronous with the radial

¹ See Muller's Physiology, by Baly, Vol. I. p. 201.

pulse, but that if an aneurism exists in the descending aorta the iliac pulse is retarded,—that in carotid aneurism the pulse of the temporal artery on the affected side beats after that on the sound side, (p. 84). On theoretical grounds we believe that these statements must be correct; should observation confirm them, we shall have acquired a most valuable sign for the diagnosis of aneurism. Such evidence would be peculiarly valuable to the surgeon meditating the ligature of an important vessel, while to the physician it would be no less satisfactory, were he able always to assure himself and patient of the true nature of abdominal pulsations, which sometimes so closely simulate aneurisms.

We find Dr Hamernjk complaining of the vague terms *irritation* and *stimulus*, which are currently employed in accounting for the increased frequency of the pulse in certain diseases, and which are mere confessions of our ignorance. Endocarditis, he argues, ought to be a most powerful *stimulus* to the heart, yet many cases of this disease are not accompanied with rapid pulse; cases of rheumatism without endocarditis are often characterized by excessive rapidity of the circulation; hence, Bouillaud and his followers err in attributing to *endocarditis* the rapid pulse occasionally observed when that formidable complication exists in rheumatism. He admits, that different causes may influence the rapidity of the heart's contractions (p. 115), such as diseases of its nerves, of the brain and spinal cord, certain passions, mental efforts, changes of temperature, hunger, thirst, sleep, powerful muscular exertion, &c.—and that it is probable (though not proved) that the influence of all these causes must act through portions of the nervous system whose function is to govern the motions of the heart. What a contempt for modern physiology is expressed in the following sentence! “Hitherto the centripetal and centrifugal properties of the nerves, including all the movements which have been termed *reflex*, are by no means demonstrated, the very functions of the nerves are yet a problem.”—(P. 119.) After remarking, that in most acute diseases, as rheumatism, typhus, scarlatina, and pneumonia, the pulse soon attains great rapidity—that in all such diseases thirst, diminution of the urinary secretion, dryness of the skin and mucous coverings of the mouth, throat, and nose are constant symptoms—that in such patients the weight of the body is rapidly reduced, and may, within eight days, be diminished by a full third, the author states his conviction, “that a rapid wasting of the body, or progressive emaciation, is always accompanied with increased rapidity of the pulse; and that conversely, a pulse increased in frequency for several days, is uniformly associated with progressive emaciation.”—(P. 93.) The emaciation he believes to depend upon a loss of liquor sanguinis, and the following seem to be his chief doctrines regarding the changes which the blood undergoes in acute diseases. We must premise that he considers albumen and fibrine as protein compounds existing in the serum in the state of *fibrine* only, and merely separating from each other in blood drawn from the

body, as crystallizable and non-crystallizable sugar do from a solution in water. Hence, when he mentions fibrine, we must understand that he means fibrine and albumen. Healthy serum he assumes to be saturated with fibrine; if then, from such serum water be abstracted, precipitation of fibrine must result, or fibrinous exudation must take place. These theoretical views (quite unconformed by chemical evidence) have been obviously constructed to account for the more striking phenomena in cases of acute inflammation. To take an instance from pneumonia, we are told, that first there is a rapid loss of liquor sanguinis, that the blood becomes thickened (*Bluteindickung*) in consequence of the loss of its fluid constituents, that the actual (not the relative) quantity of fibrine in circulation suffers a diminution proportionate to the amount of water with which the blood has parted, and that the fibrine is abstracted partly in the way of exudation, and partly in some *unknown* manner. What results should analysis of the blood give in such a state of matters? In a thousand parts we should expect to discover a great increase in the amount of blood-globules, and a proportionate diminution in that of the water, fibrine, and albumen. We need only remark, that these are not the results arrived at by Andral and other trustworthy observers.

When an acute morbid process is on the decline, water is, according to our author, rapidly absorbed into the circulation to supply the place of the expended serum; the blood-corpuscles, being more soluble in water than in serum, suffer an apparent diminution in quantity, and as the original solid constituents of the serum are diminished both absolutely and relatively, this state of things is termed *defibrination*. We are told that a dilution of the blood similar to that consequent upon acute disease, but of unexplained origin, is observed in cases of marasmus senilis, and of chlorosis. As there is sometimes a difficulty in distinguishing these affections from organic disease of the heart, we are recommended, in all cases of doubt, to ascertain the size of the *spleen* by percussion, and assured, that in valvular disease of the heart, we shall find the viscus to be enlarged, while, in the less serious cases, we shall find it of less than average dimensions. To the diluted state of the blood the occurrence of dropsies is attributed; but we are not informed *how* this condition produces them. We are left to infer that the circulation first takes up water, and then, for some unassigned reason, parts with it again. We shall spare our readers further analysis of the doctrine of *defibrination*, and of the critique upon Andral's *Hématologie* to which it leads (p. 121 to 166), believing that, unsupported as it is by chemical evidence, it is not likely to acquire popularity.

In that part of the work which treats of the auscultation of the heart and arteries, we find, at the very outset, a mechanical distinction laid down between *sounds* (tone) and *murmurs* (geräusche). The sounds are accounted for by a sudden increase of tension in membranous structures capable of entering into sonorous vibrations,

the murmurs by the friction of two bodies moving in different directions. To the class of murmurs we must refer pericardial friction and the morbid sounds produced by the roughened or contracted state of the orifices of the heart. To the class of sounds, our author refers the "tik-tak" of the heart, believing that the systolic sound is caused by the closure and tension of the mitral and tricuspid valves, the diastolic sound by the flapping back of the semilunar valves of the pulmonary artery and aorta. If a portion of a valve be less fitted than the rest for entering into sonorous vibration, or if the vibrations into which it enters are not *similar* to those of the rest of the valve, there must result, he says (p. 181), an ill-defined or protracted sound, which it may be impossible to distinguish from a true murmur. Such ill-defined sounds are of frequent occurrence in the course of severe diseases, such as pneumonia, typhoid fever, &c.; but although their mechanical cause has not been determined, we have no reason to believe that it resides solely in the valves. We are glad to find our author calling attention to the fact, that a sound quite undistinguishable by the ear from a morbid murmur, may be heard over a perfectly healthy heart, and inculcating caution in our conclusions as to the existence of endocarditis in such cases.

In the arteries, Dr Hamernijk supposes that true murmurs cannot in any circumstances *originate*. The ostium arteriosum of the ventricle being smaller than the calibre of the aorta, directs a jet of blood into the centre of the vessel; this jet does not come into immediate contact with the sides of the artery, but is surrounded by a stratum of fluid in a state of comparative rest. A similar provision in the transparent capillaries has been described by Poiseuille and other physiologists, and must, our author infers, exist throughout the whole arterial system. The mechanism which we have already described as effecting the arterial pulse, equally effects the first sound of the arteries according to Dr Hamernijk, and any cause which renders different parts of arteries incapable of entering into *similar* sonorous vibration, may cause modifications in the character both of the pulse and of the sound. Thus the temporary dilatation which occurs in the course of many diseases, the more permanent dilatation of aneurism, and the atheromatous or calcareous deposit, may cause the first arterial sound to be protracted or undefined, or even closely to simulate a true murmur. The second arterial sound is regarded as a propagation of the diastolic sound from the aortic valves; we are told that it *never* is heard beyond the points where the carotids and subclavians cease to be felt; that it is conducted according to the laws of sound, not according to the laws of undulation in fluids, which only apply to the *first* arterial sound. In cases of the "pulsus dicrotus" the *second* beat of the radial is rarely accompanied with a sound; if, however, such a sound is present, Dr Hamernijk believes it to be totally different from the diastolic sound in the vicinity of the heart, and to depend upon the vibrations excited in the artery, during its return from a curved into a straight position.

The following seem the most important conclusions to which these views have led the author; 1st, That the absence of a second sound in the carotids, is a sure and decisive sign that the aortic semilunar valves are quite insufficient. 2d, That when there is evidence of aortic incompetency and a second sound is nevertheless audible in the carotids, there must still exist enough of the aortic valves to cause the diastolic sound—the insufficiency cannot be absolute. 3d, That *true murmurs* may be excited in the aorta during the systole of the heart, by the presence of roughness or excrescences round the ostium arteriosum, or on the *ventricular* surface of the aortic valves. 4th, That no amount of roughness of the interior of the aorta ascendens can occasion a systolic murmur in the aorta, unless the ostium arteriosum or lower surface of the semilunar valves be roughened. 5th, That when the aorta is rough and inelastic, and the aortic valves quite competent, the diastolic sound may be preceded by a murmur due to the passage of the column of blood through this rough channel in the direction of the valves. 6th, The diagnosis of aneurism chiefly consists in ascertaining “by percussion or palpation, the existence of a tumour in the known course of an artery on one side of the body, and in finding whether the arterial branches beyond the tumour have a different size, and pulsate at a different moment of time from their fellows on the sound side.”—(P. 216.) 7th, That an aneurism of the aorta ascendens, is not necessarily accompanied with a systolic murmur, provided the ostium arteriosum and lower surface of its valves are healthy; that it may occasion, in such circumstances, a prediastolic murmur exactly as a rough and rigid artery does. 8th, That what is audible in poplitæal and suchlike aneurisms, is a *sound* (tone) not a *murmur*; it is merely a large pulse due to vibrations communicated to the walls of the tumour, and may be succeeded by a second sound caused by the sudden subsidence of the tumour, and by the consequent vibrations communicated by its parietes.

The explanation of the “bruit de diable” given by Dr Hamernik is, we think, the most satisfactory which has yet been offered. He agrees with Drs Hope, Ward, and Aran, that its seat is exclusively in the veins. But he goes beyond these observers, in pointing out the fact, that it is usually confined to the internal jugular veins, and more especially to the right one. At page 233, he describes minutely the anatomy of the jugulars, calling attention to the great increase of calibre which they present at their lower portion, between the crossing of the omo-hyoid, and the points where they join the brachio-cephalics. These dilated portions he regards as receptacles for the affluent blood during the act of expiration, and believes that during inspiration large drafts are made by *aspiration* upon their contents. *Proportionate to the emptiness of the venæ cavæ will be the energy of the suction.* Let us suppose a forcible effort of inspiration to be made by an anemic patient, placed in the most favourable posi-

tion for the production of venous murmur, viz.—with the head erect and turned to the left side. The lower part of the right jugular is made as tense as possible, the suction-influence rapidly empties it of blood—it must consequently either collapse (which its position and connexions prevent), or be rapidly filled with blood from the periphery. A gush of blood from above is the result, and throws the walls of the vessel into sonorous vibrations, which (if the exhaustion effected has been considerable) may not have subsided when a new respiratory act takes place. In extreme cases, these vibrations may be felt with the finger, as well as heard through the stethoscope, between the sternal and clavicular attachments of the sterno-cleido-mastoid muscle. In cases in which the “bruit” is not *constant*, it is usually best heard towards the close of inspiration, and, in almost every case, undergoes modifications in force during the different periods of respiration. It ceases when the jugular is compressed above the point where the stethoscope is applied, and usually when the patient assumes the horizontal posture, or bends the head forward. It is most distinct in the *right* internal jugular, because the communication of that vessel with the *venæ cavæ* is the shorter and more direct. We have never regarded as sound that doctrine of Andral, which attempts to attach this bruit to a certain chemical composition of the blood; we can conceive no reason why an impoverished blood should cause a “bruit” more readily than a fluid richer in globules; and believe, with our author, that simple diminution of the mass of the blood is of itself sufficient to induce the murmur. Whatever causes congestion in the right side of the heart, must necessarily oppose the production of the venous murmur, by keeping up a constant pressure upon the interior of the *venæ cavæ*; hence we may regard the simultaneous occurrence of “bruit de diable” and valvular disease of the heart, as next to impossible.

We can warmly recommend a perusal of Dr Hamernik's work. Its author has undoubtedly the merit of great originality and ingenuity; and, if the accuracy of his theories be not always proved, he has, at least, exhibited no small degree of boldness in grappling with some of the most abstruse subjects in medicine.

Relation Médico-Chirurgicale de L'Expedition du Bon-Thaleb (Province de Constantine), et Notice sur le Service Chirurgicale de l'Hôpital Militaire de Sétif à la suite de cette Expedition, sur les Congelations Partielles, leur Traitement, &c. Par C. SHRIMPTON, M.D., Chirurgien en Chef de l'Ambulance Active de la Province de Constantine, &c. &c. 8vo. pp. 158. Constantine, 1846.

Medico-Chirurgical Account of the Expedition of the Bon-Thaleb, &c.
By C. SHRIMPTON, M.D. Constantine, 1846.

THE work, the title of which is cited above, is a contribution from Africa to medical science. It contains the medical history of one of

the unfortunate French expeditions against the native tribes of Algeria. Our author had the charge of the ambulance or moveable hospital attached to the force. The expedition set out from Constantine for the interior in the dead of winter (15th December 1845), and after about twenty days, what remained of it took refuge at Sétif. The force employed consisted of nearly three thousand men (2800). Of these upwards of two hundred perished on the march from the immediate effects of cold; and of the remaining 2600, not more than 250 escaped altogether from the effects of frost-bite. In 1800 the frost-bite was superficial, and was cured without difficulty, the average duration of the treatment being about thirty-five days. Of 532 severer cases admitted into the hospital at Sétif, fifty-five had operations performed on them, and the event was fatal in three instances; of the remaining 477 treated by purely medical means, nineteen died. Of those who perished, or were wounded in action, the number was but small.

For the first fifteen or sixteen days of the march, the weather was good, and the duties of the medical officers were confined to the care of the few who were wounded by the Arab rifles. On the 2d of January, a cold north wind set in with much snow, so that on the morning of the 3d the camp pitched the evening before was almost buried. On the evening of the 2d, three men were carried to the ambulance stiffened with cold. These were recovered easily by applying hot stones to the feet, wrapping them in flannel, and giving them some hot coffee. On the morning of the 3d, fifteen in the same state were conveyed to the ambulance, chiefly young soldiers, or such as had recently suffered from fever, and of these two died in no long time. The treatment successful with the rest, was envelopment in flannel, dry frictions, and the administration of a few drops of sulphuric ether, or a little of the wine of canella. The same day, after the camp was raised, the troops were exposed for many hours to a cold north wind, blowing about the snow which had fallen the night before, motionless, owing to the difficulty experienced in conveying the stores through a defile in front, and not having tasted food for more than twenty-four hours. Beyond the defile was a steep descent which compelled the abandonment of the provisions, a great part of the camp equipage, baggage, &c. Numbers of the troops perished from cold in this descent. "The signs preceding death in these were general stupor, pain in the limbs and arm-pits, feeble and unsteady muscular contraction, redness and tumefaction of the face; the lips blue, the eyes prominent, lividity of the surface, swelling of the hands, pulse small and feeble, respiration slow. All these symptoms underwent a rapid aggravation; the eyes took on an expression of wandering; the walk became unsteady; the soldier tottered and soon fell to rise no more. The skin of the hands then burst, and often there flowed out from two to three ounces of blood. The night between the 3d and 4th was passed by the troops with little shelter, and numbers perished. The 4th

was fine, but with a cold north wind, giving rise to much suffering ; but our author's party arrived at Sétif in the evening, after a long and painful march, without sufficient sustenance. Nevertheless, the detachment which had accompanied the ambulance during the previous twenty-four hours, escaped without loss even from frost-bite—which Dr Shrimpton ascribes to the greater care taken to protect the soldiers composing it from cold during the previous night, and to the occasional refreshment afforded to them by a little brandy and a few mouthfuls of bread. As soon as the garrison of Sétif became aware of the numbers who had been left behind, assistance was sent out, and the lives of many saved.

The military hospital at Sétif, then hardly finished, became greatly overcrowded, and hospital gangrene threatened to spread. Our author thinks that this danger was materially diminished by carrying as many patients as could bear it into the court during the day whenever the weather permitted, while in the mean time the wards were thoroughly cleaned, fumigated, and ventilated to the utmost that was possible.

Dr Shrimpton remarked, that more than a third part of the victims during the march had previously laboured under quotidian or tertian fevers, diarrhoea, or dysentery.

Next to these maladies, the lymphatic constitution, excesses in spirits, and deficiency of food, aggravate in a striking manner the influence of cold. The remark was made after the Russian campaign, that the soldiers of the Dutch regiments, inured to cold, yet of a lymphatic temperament, suffered more than the regiments recruited in the south of France, with men of a dark and bilious temperament. We have had occasion to note the truth of this observation. Yet in Russia, as in the snows of Bon-Thaleb, it is not merely owing to their excessive susceptibility to cold that lymphatic persons are particularly cut off, but also because they cannot bear the deprivation of food. Their soft white flesh needs continual repair ; their nervous system, easily depressed and unelastic, to prevent exhaustion requires a given amount of stimulation. We know that the degree of sobriety familiar to the man of the south would prove fatal to the inhabitant of the frigid zones ; and *vice versâ*, that the strong stimulating nourishment and the alcoholic excesses of the latter would quickly destroy the former. It was not without reason that Ross, on undertaking a voyage to the Polar seas with a crew composed of men of the north, that is to say, generally of lymphatic temperament, determined to seek out great eaters.

To affirm, however, that the deprivation of food is particularly hurtful to lymphatic persons, is not to maintain that such deprivation is supported with impunity by the nervous, the sanguine, or the bilious constitution. We are convinced of the contrary. Among the unfortunates who perished, and among those who were affected with severe frost-bite, we remarked a certain number belonging to these several temperaments ; and what proves that prolonged abstinence in them much aggravates the influence of cold, is, that the officers whose condition during the 2d, 3d, and 4th of January differed in nothing from that of the private soldiers, except in being not entirely without food and brandy, did not number a single victim.—Pp. 33-35.

Our author remarks that frost-bite, aggravated by the sudden application of heat, does not appear to differ from that produced solely by the action of cold, except that, in the former case, the gangrene became deeper in its seat, and more rapid in its progress.

Our author's details afford a convincing proof, if any were wanted, of the truth of the prevailing opinion as to the danger of applying sudden heat in the case of frost-bite. Out of 355 patients affected with frost-bite under Dr Shrimpton's immediate care, 72 declared that, during the march, they had enjoyed the benefit of bivouac fires; while 274 affirmed that they had not been near a fire from the beginning to the end of their disasters. Among the latter, there were 256 gangrenes of small extent affecting the skin partially, or throughout its thickness; 15 gangrenes of greater extent, but of similar depth; three of such depth as to render an operation necessary. Among the former, there were two gangrenes of little severity; 11 gangrenes of greater or less extent, laying the muscles bare at some points, and 59 deep gangrenes, which gave occasion for 33 operations. In the first of these two classes, or that where cold operated alone, there were but two deaths; in the second, nine deaths.

Dr Shrimpton terms frost-bite a partial or local asphyxia, and enters on some discussion as to the conditions under which gangrene supervenes. He considers it as the prevailing opinion among surgical pathologists, that reaction in frost-bite, whether spontaneous or produced by the application of artificial warmth, is the cause of all the ill effects that follow. Here he takes reaction in too limited a sense. Frost-bite, or his local asphyxia, undoubtedly sometimes is recovered from without passing into gangrene. But it cannot be recovered from without a certain amount of reaction, nor is it usual to deny the name reaction to that moderate degree of activity in the local circulation, by which the affected part recovers its healthy condition. If reaction does not take place at all, gangrene follows; if reaction be over violent, gangrene follows; if reaction be moderate, a cure takes place,—and this we think is the common doctrine. We shall conclude this brief account of Dr Shrimpton's interesting book with some of his practical observations.

He thinks that the employment of frictions with snow in frost-bite has hardly been reduced by practical authors to its proper principles. Snow should not be used, he says, unless the temperature of the air be below the freezing point. When the temperature is below that point, frictions with snow are proper, because the temperature of melting snow is somewhat warmer, being of the same temperature as the freezing point. In short, when it is necessary to guard against a dangerous re-action, friction with snow is the first of a series in the gradual application of a higher and higher temperature, which is carried out by means of water at successively elevated temperatures. In robust and healthy individuals the use of snow alone is usually sufficient; but in the debilitated, the gradual increase of temperature, by means of portions of water successively more and more warmed, is essential. It seems at first sight impossible to suppose that local asphyxia should occur at a temperature above the freezing point; but the facts collected in the Bon-Thaleb expedition

sufficiently show how readily it may happen, particularly when aided by previous fatigue or exhaustion. And when it does take place at this higher temperature, snow should not be used, but water of a somewhat higher temperature than the surrounding atmosphere.

We have not left room to notice the observations on amputation with which our author concludes his work. Altogether this is a very interesting book, and a valuable contribution to those parts of pathology and surgery of which it treats.

Dysphonia Clericorum, or Clergyman's Sore Throat: its Pathology, Treatment, and Prevention. By JAMES MACKNESS, M.D., &c.
London: 1848. 8vo. Pp. 125.

THIS little work treats of the sore throat so troublesome to the vocal powers of singers and orators. In it is pointed out the different pathological changes which cause the disease, their distinctive features, and their appropriate treatment. The most common existing cause is, according to the author, the inordinate action of the organs of speech themselves, but especially the long-continued strain which is kept up for a lengthened period, and by which the muscular and tendinous parts of these organs are kept on the constant stretch, without those intervals of rest which occur in common conversation. Mr Macready, in a letter to the author, observes, that relaxed sore throat is usually caused not so much by exercising the organ, as by the kind of exercise; that is, not so much by long or loud speaking, as by speaking in a *feigned* voice. He considers the disease attributable generally to the mode of speaking, and not to the length of time or violence of effort that may be employed; and says, "I have known several of my former contemporaries on the stage suffer from sore throat, but I do not think, among those eminent in their art, that it could be regarded as a prevalent disease."

We can recommend the work of Dr Mackness not only to members of the profession generally, but to all who are liable to the particular affection of which it treats.

Adventures of a Medical Student. By ROBERT DOUGLAS, Surgeon R.N. In 3 Vols. 8vo. London, 1848.

THESE adventures consist of remarkably well told tales, after the manner of the *Diary of a Physician*. We discover in them graphic powers of description, with great originality of conception. Medically, we could point out some very bad treatment, and ill-judged interference with patients, inexcusable even in a medical student. Still, such of our readers as require a quiet evening's amusement (and which of us occasionally does not?), may find it in a perusal of these volumes.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVII.

MEETING VII.—*Wednesday, March 1, 1848.*—Dr GAIRDNER in the Chair.

DESCRIPTION OF AND MODE OF APPLYING AN ELECTRO-MAGNETIC MACHINE. BY
DR THOMAS WRIGHT.

After describing the mode of action and construction of the more convenient forms of the Voltaic battery, Dr Wright proceeded to explain the rationale of the electro-magnetic coil; the instrument used for illustration consisted of a bundle of iron wires seven inches in length, over which was spirally coiled a thick insulated copper wire thirty yards in length. He stated that, on applying the poles of a simple Voltaic circuit to the ends of the spiral wire an electric current passed through it, and the bundle of iron wires in the centre became magnetic, but ceased to be so when contact with the battery was interrupted—that the cessation of magnetism in the iron wire had the effect of inducing a momentary impulse of electricity through the spiral, as was shown by a spark appearing, and a shock being felt at the part where the battery current was interrupted—that a second insulated wire of great tenuity and length, coiled over the thick spiral, but quite unconnected with it, had an impulse of current electricity induced in it by the cessation of magnetism in the same manner as in the thick spiral, each interruption of battery current with the latter being attended with shock and spark from the former—that the shock from the thin spiral was felt *only on the interruption of battery contact*, and not also at the time of making contact, as was the general opinion—that the quantity of electricity induced in the spirals was in proportion to the thickness, but the intensity of the current to the length of the wire of which they were composed. He then proceeded to describe the effects of different circuits or spirals coiled over the same magnet in preventing induction in each other, and showed that the union of the ends of the thin spiral had the effect of neutralizing the induced current in the thick spiral; the sparks and shocks produced by the latter at each interruption of battery contact instantly ceasing when the extremities of the thin spiral touched each other, and that a piece of thick brass tube, which formed a closed circuit, had the effect of gradually diminishing and stopping the induced current from both spirals when passed over them, contact being carefully avoided. He noticed, what he considered a singular fact not yet published, that the electro-magnet exerted the most powerful influence in the induction of electricity at its centre, a part which had no attractive force whatever for ferruginous bodies; by using a short coil capable of sliding to and fro on along electro-magnet the shock was found to decrease rapidly as it approached the ends, and to regain its former strength when it was returned to the centre. He then described his electro-magnetic machine, consisting of the coil first described, with self-acting apparatus for interrupting battery contact, and a brass tube sliding over the coil to moderate the shock. He stated, that the shock from the thick spiral caused the more severe spasm of muscles through which it passed; but that the thin spiral, when its ends were *lightly* applied to the skin so as not to give a shock, forced through it a torrent of sparks so pungent as to be perfectly intolerable, and was of great efficacy in the treatment of narcotism, an elegant substitute for the castigation and agitation often adopted in such cases. He did not then wish to proceed further in the subject of the application of electricity to the cure of disease, as it involved the necessity of entering into the consideration of animal electricity, which would trespass too much upon the other business of the evening.

Professor Allen Thomson stated that the paper he had just heard, appeared to him to contain several novel and interesting facts. He had tried Dr Wright's

instrument, and found it very serviceable, and of great power, while it appeared to be especially applicable to medical purposes. The circumstance pointed out by the author, that shocks could not be communicated in different directions, was of great importance. An expensive apparatus had been made to prevent this effect, which was now shown to be altogether unnecessary.

Professor Simpson had found *Dr Wright's* small machine to be stronger than any other, and had found it very serviceable in some cases of narcotism in children. He had lately observed in one case, however, that plunging in cold water was a more powerful stimulus than the battery. He spoke of the use of the galvanic current in cases of aneurism and varix. Some years ago, he had suggested its employment in ovarian dropsy, with a view of coagulating the contents of the cyst, and checking the growth of the tumour. He had found it impossible, however, to pass sufficiently strong shocks through the abdomen, on account of the pain produced. Since the introduction of ether, he had tried it in two cases. In one the tumour had become both much harder and smaller, and her general health was much improved. In the other case, the patient had died from an attack of pneumonia during the late influenza. If morbid growths consisted, as was now believed, of aggregations of living cells having a parasitic independent vitality, then probably the transmission of strong galvanism through them by acupuncture needles, &c., might be a means of destroying their life, and thus preventing their increase. As regards the influence of galvanism on the contractions of the uterus, he had found that they were not increased by it either in length or number. In one case they completely ceased while the battery was acting.

Dr Lee exhibited one of *Dr Wright's* machines, which he had employed with the best effects during a period of four years. In a case of narcotism in a child, he had applied the spark from the thin coil to the nares, a considerable quantity of mucus having collected, when strong sneezing was immediately induced, and the air passages effectively cleared.

Dr Bennett observed, that it was fortunate the subject of medical electricity had fallen into such good hands, as those of *Dr Wright*, and the profession in Edinburgh ought to feel much indebted to that gentleman for the pains he had bestowed upon the subject. He believed this agent to be one which, if judiciously used, was of great value in many important diseases, and that a new field in therapeutics had yet to be cultivated by those who would make themselves masters of the subject, and then energetically, but cautiously, applied in practice. The observations of *Petrequin*, *Abeille*, *Ciniselli*, and others, had proved that certain external aneurisms could be completely cured by electro-puncture; and he thought that the employment of this agent held out very rational hopes of cure in appropriate cases of internal aneurism. As regards ovarian dropsies, the contents of the cysts varied greatly, sometimes containing a serous, albuminous, or fibrinous fluid, blood, pus, &c., and at others a gelatinous celloid material more or less firm. He had found that these coloured fluids or gelatinous matters were coagulable by the addition of acetic acid in some cases, but not in others. The possibility of coagulating the contents of cysts, therefore, was dependent on the chemical composition of the contained fluid, and it would be evidently useless attempting it in all cases. To give the plan a chance of success, the nature of the fluid should be first accurately ascertained—which could only be done by extracting a portion of it by means of a trocar.

Dr Gairdner observed, that in a case of hydatids attached to the peritoneum, which he had recorded in conjunction with *Mr Henry Goodsir*, the fluid they contained was highly coagulable by heat and acid. He thought it would be important to ascertain whether the application of galvanism could check the progress of these growths in a living state.

Dr Wright stated that he had contented himself with describing the machine and its mode of operation. He had purposely deferred the subject of its medical applications, as being too extensive. He would observe that *Dr Simpson* had misunderstood him with regard to its application. In cases of narcotism it was not the shock, as used by *Dr Simpson*, that was serviceable, but the

sharp pungent pricking sensation, produced by the spark from the thin coil. He had never seen it employed in aneurisms; but in a case of hydrocele, in which it was used under the care of Dr Duncan, a considerable quantity of lymph had been deposited in the sac, while the fluid had disappeared. For coagulating blood, he thought it would be better to use a small battery of plates. In a case of very severe bronchitis in a child, he had seen it saved by exciting continued respiration. He had no evidence to prove that a galvanic current would destroy the growth of living cells. He had tried it on the polygastrica without producing any effect. In local paralysis it was very beneficial, especially in those cases where a feeling of coldness was complained of.

DISCUSSION ON THE TREATMENT OF OVARIAN DROPSY.

Dr Clay of Manchester (who was introduced to the Society by Dr Simpson), stated that he had in the morning performed the operation of ovariectomy for the twenty-eighth time.¹ Of the twenty-seven previous cases, eighteen had recovered. The practice of ovariectomy seemed to him to be as far behind in Edinburgh, as the inhalation of chloroform was in Manchester. He was satisfied that the preparation employed in the latter city must be very impure, because, although he had frequently seen it administered, he had never seen it succeed, until that day in the hands of Dr Simpson. On the other hand, it would appear that ovariectomy was always fatal in Edinburgh, whilst in Manchester it was tolerably successful. A state of weakness and long suffering was in his opinion no contra-indication of the operation. He thought strong and robust persons were those in whom it was least successful. He had found also, to his astonishment, that cases in which there had been extensive adhesions, had done best. In one instance where a portion of the abdominal linings, the size of a crown piece, had been removed, a rapid recovery had taken place.

Professor Simpson stated, that he did not by any means think ovariectomy warrantable in all the cases in which it was performed by Dr Clay. He would not recommend an operation fatal in the proportion of one in two and a half cases to a woman who could still enjoy life, as in a case he had shown Dr C., which had been of twelve years' duration, and had been tapped numerous times. The results of surgical statistics had shown that primary amputations were more fatal than secondary ones, which corresponded to the observation of Dr Clay, that robust did not bear ovariectomy so well as weak women. He was still of opinion, however, that where it was a simple and direct question between the operation and its chances of life, and the continuance of the disease with a certain and speedy termination in death, and where in addition there was no contra-indication, ovariectomy was proper and justifiable.

Dr Myrtle asked what would be the effect of introducing a tube with a stop-cock into the tumour, keeping it there, and allowing the contents to run off from time to time. He had lately seen a case where this practice seemed to him warrantable.

Dr Bennett stated in reference to Dr Clay's remarks on the mortality of ovariectomy in Edinburgh, that the case he had recorded with Dr Handyside, lived seventy days after the operation, and then died from a purely accidental cause. The girl was quite well and walking about, when, under the influence of hunger, she eat greedily every kind of food she could obtain, in the absence of the nurse, and enormously distended her stomach. In this way she produced unusual displacements of the intestines, and a small portion became mechanically strangled, in one of the filaments of chronic lymph, hanging to the peritoneum, which caused fatal ileus. That case would have undoubtedly recovered but for the untoward accident he had narrated. The objections to Dr Myrtle's proposition consisted in the fact, that ovarian tumours were very rarely composed of one cyst; so that the evacuation of one or even more cysts, or even the introduction of stimulating injections, as had been proposed, produced no permanent influence on the morbid

¹ The case terminated fatally on the following day.—Ed.

growth. Every rational plan of treatment must be founded on a knowledge of the manner in which these tumours grow and are developed. At an early period, they consist of innumerable secondary, or even tertiary cysts. As growth advances, these open into each other, and occasion larger cavities, and the tendency of their development is to go on opening one into the other, until the whole forms one enormous sac. In a few instances such takes place; but in the majority of cases, long before this has occurred, inflammation occurs within the sac, or peritonitis supervenes, which causes death. It must be evident, however, that it is only in this last stage that any proposition founded on the idea of acting on one sac can be beneficial. The whole question resolved itself into one of diagnosis. He thought it would be useful, after a tapping, to introduce a sound, and ascertain positively if there were one or more cysts. If only one existed, it might be warrantable to throw in a stimulating injection, and endeavour, with the assistance of pressure, to produce adhesion and obliteration of the adhering surfaces. Sometimes, under such circumstances, the tumour bursts, when a spontaneous cure has, in very rare instances, occurred. It was consistent with his experience to believe that, inflammation once commenced within the sac, the fatal termination was not far distant; and that, after local tenderness, rigors, and other symptoms indicating this, the operation of ovariectomy, if performed at all, should be executed as soon as possible. The fear of exciting this inflammation should induce the practitioner to delay tapping as long as possible; for although in some persons, from absence of constitutional irritability, or other causes, paracentesis was frequently had recourse to without injury, in the great majority of cases it hurried on the fatal termination.

Dr Clay remarked that he had never yet seen an ovarian tumour with one cyst. In one case he had injected ʒij of tincture of iodine with ʒvij of water. The fluid came out colourless, and the individual died.

Dr Makellar said, that in a case he had brought before the Society, successive tapplings seemed to have caused almost a spontaneous cure. At each operation the quantity of fluid removed was less, and, after death, the walls of the tumour were found contracted, thickened, and indurated.

EDINBURGH OBSTETRIC SOCIETY.

SESSION VII.

MEETING II.—*Dr Simpson*, President, in the Chair.

The following gentlemen were elected office-bearers for the ensuing Session:—*Dr Simpson*, *President*; *Dr Moir* and *Dr Malcolm*, *Vice-Presidents*; *Dr Cumming* and *Dr Keith*, *Secretaries*.

ON AN INSTRUMENT FOR THE INHALATION OF CHLOROFORM.

Dr Cornwall read a paper on chloroform inhalers; a variety of different kinds were placed on the table. He considered that the simpler the construction the better, provided it efficiently answered the purpose, as any thing complicated and imposing was very apt to startle and alarm patients. He showed a contrivance of his own, which had proved to be very suitable and convenient. It is a tin box which contains a phial of chloroform marked in drachms. At the bottom of the box are three layers of flannel, over which is a piece of perforated zinc. In using it, a drachm of the chloroform is poured in, and it is then applied over the mouth and nose of the patient; the edge being of a shape to fit closely to the cheeks, the requisite supply of air comes through an opening at the upper part of the box. In obstetric cases, where it is wished to keep up the soporific effect for some hours, or during surgical operations where there are few assistants, this inhaler can easily be retained in its place by means of a tape attached to the small rings at the ends of it, and, when so applied, any additional quantity of chloroform required can be put in by means of a filler.

Dr Cornwall stated that, in the hands of those expert in the use of this anæsthetic agent, a towel, or handkerchief, or sponge, could no doubt be made to do well; but it required both care and practice to arrive at proficiency in this, whereas, with his inhaler, he considered it almost impossible for any one to go wrong. He viewed the vapour of chloroform as a fluid—it being, in fact, much heavier than atmospheric air—and his inhaler as a drinking cup, which supplied each inspiration not only of the mouth, but of the nostrils also. In this way, and from the extensive evaporating surface, the full narcotic effect is at once induced; and this is of very great importance, preventing the awkward and injurious tendency of its exciting and sickening action. *Dr Cornwall* considered the snoring, so commonly induced, indicative of the state of insensibility to pain, and, therefore, of the proper time to commence operations. He advised, however, that, while deep snoring lasted, the supply of the vapour should be reduced by holding the inhaler at a little distance from the surface. He was of opinion, that more attention should be paid to the effect produced on the pulse and features than to the quantity of chloroform given. At the same time, with a narcotic so powerful, he thought it right always to measure the dose put into the inhaler.

The advantages of this inhaler are, efficiency and safety; it effects a very considerable saving of chloroform, and thoroughly prevents the contact of the fluid with the lips or face, and the dripping of it on the dress. Its portability also may be mentioned. It is carried conveniently in the pocket, and, as to its cost, it can be had for 3s. 6d. *Dr C.* found that rest in the recumbent position, quietness, and an empty stomach, are circumstances favourable to the chloroform taking proper effect speedily.

SOME REMARKS ON THE MODE OF ADMINISTERING CHLOROFORM.

Dr Simpson showed the Society a variety of instruments used in England for administering chloroform,—Salt's, *Dr Protheroe Smith's*, *Mr Robinson's*, *Hooper's*, &c. He considered *Dr Cornwall's* as good as any, and preferable from its simplicity; but he entirely objected to it, or any one, being tied upon the face of the patient, as he feared that accidents would inevitably happen from too large a dose being thus sometimes administered. In using the handkerchief even, he never applied it quite close to the face of the patient, but always allowed a sufficiently free access of air. As to the quantity of chloroform to be used, *Dr S.* judged entirely by the effect on the patient. He believed that, in surgery, the symptom most to be relied upon as indicating a thoroughly anæsthetic state, was slowness of respiration, or a degree of snoring, or stertorous breathing. After the appearance of this symptom, it is only needful to continue the inhalation, from time to time, by intermissions; and, by repeated applications of the handkerchief, the patient ought to be kept in the same sopor during the whole of the operation. In midwifery, *Dr S.* believed that this deep degree of anæsthesia was, in ordinary circumstances, not required. He had found that, when administered so as to cause stertorous breathing, the uterine contractions generally ceased, and did not return till the effect had, in some degree, passed off. He had found, therefore, that the chloroform given in a surgical dose was very useful whenever he wished to stop the uterine contractions, as in cases where turning was necessary, and many other obstetric operations. In cases of natural labour, he generally began with a large dose of chloroform, so as to bring the woman at once completely under its influence. This mode of proceeding prevents the chance of any excitement; and, although it occasionally may stop the pains for a few minutes, especially if the labour is still in an early stage, yet the contractions recur as soon as the deeper state wears off, which it usually does in two or three minutes. The depth of the sopor is allowed to diminish by merely withholding the handkerchief till a pain comes on; and then the anæsthetic state is kept up by its reapplication, for two or three inspirations every four or five minutes; or what is better, with each succeeding pain. The patient should be kept unconscious, but not deeply so, till the head was passing the vulva, when a deeper anæsthesia was required.

CASE OF DOUBLE CEPHALHÆMATOMA.

Dr Simpson shewed a child two weeks old, with a well-marked large and defined cephalhæmatomatous swelling on each parietal bone, with the hard rim well marked at different points. He had never seen it on both sides except in this case. In this, as in most other cases, the tumours had not been observed till the first washing of the child, having come on, or at least grown greatly for some hours after birth. The effused blood was already becoming absorbed, and, by leaving the case entirely to nature, a cure would soon be effected. The effusion was between the skull and pericranium. He had watched various cases during the process of a natural cure, and he several times found that a layer of bone is formed on the inner surface of the separated pericranium, which can sometimes be distinctly felt after a time to crackle under the finger like parchment,—and, as the fluid gets absorbed, the two plates of bone gradually approximate and come together. *Dr S.* believed that such cases were often mistaken and mistreated, by too active measures being employed. He had now had an opportunity of seeing a number of cases of cephalhæmatoma and had never seen any treatment required but time and patience. The difficulty in their management generally, consisted in keeping the friends and others from doing something or other to them when nothing, in reality, was required.

CONGENITAL CEPHALIC TUMOUR.

Dr Keiller communicated the following particulars of a case of *encysted congenital tumour*, which he had recently seen in Dundee along with Drs Moore and Malcolm, who were subsequently induced to attempt its removal by puncture and ligature.

The tumour, which was of a pyriform shape, elastic, and quite compressible, was situated in the region of the occiput, towards the mesial line, and equalled in bulk the entire head of the child, which was, in every other respect, fully developed and well formed. It was impossible to detect by manipulation the coexistence of any cranial aperture or spinal fissure, the tumour becoming too tense on pressure to allow the exact nature and extent of its root to be satisfactorily ascertained. It could be compressed, and indeed roughly enough handled, without apparently causing any inconvenience to the infant; no additional tenseness or any pulsation occurred in it when the child cried, nor was the slightest diminution of its size observed to follow the firmest pressure to which it could be subjected.

The absence of convulsions or other immediate symptoms of pressure on the brain or spinal cord, seemed to indicate that, whatever may have been the connexion originally existing between the cyst or cysts, of which the bulk of the tumour was composed, and the cerebro-spinal membranes, little or no communication now apparently existed. The result of the following operation, had recourse to by Drs Moore and Malcolm, sufficiently proved, however, the existence, or rather the persistence of a cranial fissure opposite the pedicle of the tumour, which, being doubly encysted, one within the other, in all probability prevented the phenomena of compression as usually observed in cases of non-encysted encephaloid or spinal tumours, consequent on partial protrusion of the cerebro-spinal axis or its membranes, through clefts or immaturities of the cranial bones or spinal canal. The operation consisted in puncturing the tumour, with a view to evacuating its fluid contents, and afterwards removing its entire substance by the ligature or the knife. On opening the tumour or outer cyst, about eight ounces of straw-coloured serum escaped, by which the bulk of the swelling was only slightly diminished. The aperture was immediately enlarged, and on introducing a finger into it another large distended sac was distinctly felt; it also was laid open, when an additional quantity of serum gushed out. On again introducing the finger into the now flaccid sac, a third tumour, about the size of a pigeon's egg, and very tense, was found protruding through a narrow fissure in the occipital bone; but which could be readily reduced within the cavity of the skull, and that without causing the slightest symptoms of compression or inconvenience. After pushing this deeply-seated cyst within the occipital fissure from

which it protruded, the larger or more external and flaccid sacs were surrounded by a ligature. The child sucked vigorously after the operation, and afterwards slept calmly for several hours. It gradually sank, however, on the following day. No *post-mortem* examination was permitted.

Dr Keiller, after citing other similar instances, and referring to the cases of congenital tumour of the neck, published by Mr Cæsar Hawkins, and those of congenital encysted tumour of the pelvis and spine, recorded by Mr Stanley, made some remarks upon the difficulty and importance of determining the true character of the various forms of congenital tumours in reference to the question of their cure by surgical interference, and particularly referred to the frequently observed fact, that in some cases of true encephalocele or spina bifida, (when the membranous cyst or tumour remains *continuous with the interior*, and not merely *connected with the exterior* of the membranes of the brain or the theca vertebralis), the insignificance of the aperture of communication, together with the extreme mobility of the pediculated tumour, and the total absence of symptoms of compression, even by the roughest handling, presented inducements to the surgeon to have recourse to operative procedure, which, however, notwithstanding the success which has attended the use of the knife in a few doubtfully and altogether exceptional cases, can scarcely be expected to terminate otherwise than unfavourably.

THE NEW MEDICAL BILL.

So many bills have been destroyed by the jealousies and opposition of different medical corporations, that we are agreeably surprised to find the London boards have at length determined on certain fundamental points, as the basis of a fifth attempt to settle the question of medical reform. We are indebted to the *London Medical Gazette* for the following account of the principles agreed upon by the joint committee of the Royal Colleges of Physicians and Surgeons of England, the Society of Apothecaries, and the National Institute of Medicine, Surgery, and Midwifery :—

I.—*New Incorporation*.—That a Charter of Incorporation should be granted to the Surgeon-Apothecaries of this country, under the title of “The Royal College of General Practitioners of England.”

II.—*Establishment of a Council*.—That a Council should be established to superintend the Registration of all Medical and Surgical Practitioners, and for the general control of Medical Education and Practice.

That the Council should consist of one of Her Majesty’s Principal Secretaries of State, in right of his office (who should also be President of the said Council, with power to appoint a Vice-President), and that the other Members of the Council (of whom not less than two-thirds should be Registered Members of the Medical Profession) should be such persons, not more than twelve, whom Her Majesty, with the advice of Her Privy Council, should deem fit to be members of the said Council.

That the Council should be empowered, with the approval of the Secretary of State, to appoint a principal Secretary and local Secretaries for Scotland and Ireland, and Clerks, Messengers, &c. ; and that the Members of the Council, the Secretaries, Clerks, &c., should receive such salaries as the Lords of the Treasury should think proper to allow.

III.—*Register to be Made and Published*.—That a Register should be made and published by the said Council, as soon as conveniently may be, of all Persons entitled, as after explained, to be registered as Physicians, Surgeons, or General Practitioners, and who shall apply to be so registered within one year if residing in any part of the United Kingdom, or within two years if resident abroad ; for which a fee of ten shillings shall be paid by each person so registered. And that to the Register should afterwards be regularly added the names of all persons who shall receive Letters Testimonial, as after explained, of their fitness to practise as Physicians, Surgeons, or General Practitioners ;

and that for such registration should be paid the sum of Five pounds in the case of a Physician or Fellow of the College of Surgeons, and of Two pounds in the case of a General Practitioner. All such fees should be applied towards defraying the expenses of this Act.

The Register to be formed on the plan of a specimen submitted to the Committee of the House of Commons in 1847, by the Registrar of the College of Physicians.

And all persons desirous of being continued on the Register should be required to send their names and residences to the Council annually, but without the payment of additional fees.

The Register to be received as evidence in Courts of Law.

IV.—*General Practitioners.*—That those persons should be entitled to be registered as General Practitioners who shall be enrolled as Members of the Royal College of General Practitioners within one year from its first Incorporation, according to the provisions of a Charter which has been prepared for that College; and that those persons should afterwards be entitled to be so registered who, having attained the age of twenty-two years, shall have received Letters Testimonial of their fitness to practise as General Practitioners, from the examining Board of the Royal College of General Practitioners, and who shall also have been examined and admitted as Members of the Royal College of Surgeons.

Surgeons.—That those persons shall be entitled to be registered as Surgeons who shall have been admitted as Fellows or Members by the Royal College of Surgeons. That the Members of the College of Surgeons who dispense medicines, or supply medicines to their patients, shall be required to enrol themselves in the College of General Practitioners, and to be registered as Surgeons and General Practitioners; and after the passing of the Act, Members of the Royal College of Surgeons shall not be registered as Surgeons unless they be also admitted as Members of the Royal College of General Practitioners, and registered both as Surgeons and General Practitioners.

Physicians.—That those persons should be entitled to be registered as Physicians who shall have been admitted as Members of the Royal College of Physicians, according to the provisions of a new Charter which has been prepared for the College of Physicians, and has also been submitted to the Government, which Charter it is expedient should be granted to the College of Physicians.

Registry of Special Cases.—That the cases of persons who have been engaged in practice prior to the passing of the Act, without being Members of any Corporate Body in the Profession, should be referred to the College of the department in which they have practised respectively, for special investigation of their claims to be admitted to register.

V.—*Reciprocity of Practice.*—That the Members of each class of the Profession, registered in each of the three kingdoms respectively, should be entitled to be registered and to practise reciprocally in either of the three kingdoms as Physicians, Surgeons, or General Practitioners, as the case may be, provided the education and examinations of each class respectively be assimilated and regulated by a certain standard common to each class; and provided that, previous to registration, they be enrolled in the College appropriated to their class, in the country in which they practise.

VI.—*Powers of the Council as to Bye-laws.*—That the Council shall be empowered to allow or disallow any new Bye-law which shall be made by either of the Colleges.

Registry of Students.—Also, to cause a Register to be made of Medical and Surgical Students, and to make such dispensing regulations as shall seem fit in favour of those Students who shall have commenced their professional studies before the passing of the Act.

Returns of Fees, Examinations, &c., for Securing Uniformity and Efficiency of Examinations.—Also to call for Returns respecting Examinations, and Fees for Letters Testimonial and Admission into the respective Colleges, and to take

such other measures as may be necessary in order to assimilate, as nearly as possible, the education, examinations, and fees for each class of the Profession respectively in each of the three kingdoms. And that the Council should be empowered, in any case in which it should appear to be necessary, in order to secure efficiency and uniformity of examinations, to depute one or more of its members to be present at the examinations of any of the Colleges; provided the Members of the Council so deputed be Medical Practitioners, and of the same class in the Profession as the Colleges to be so visited; and if, upon the Report of such visitor, or otherwise, the Council should be of opinion that its regulations are not complied with by any examining body, that it should be lawful for the Council to refuse to register, upon the Testimonials of the Body so in default, until the same be amended to the satisfaction of the Council.

As to Striking from the Register.—That the Council should be empowered to strike from the Register the names of any persons who shall be proved to have obtained the registry of their names by any fraud or false certificate, or who shall have been convicted of felony, or of having wilfully and knowingly given any false certificate in any case in which the certificate of a Medical Practitioner is required by law; also, the names of any persons who shall have been expelled from either of the Colleges, according to the terms of their respective Charters, provided such expulsion shall have been approved and allowed by the Council, as before required, respecting Bye-laws.

VII.—*Privileges of Persons Registered.*—That no one who is not registered should be judged capable of performing any act which is required by law to be done by a Medical Practitioner; nor should any but a registered person be appointed to any office which is deemed by the Council to be a public Medical or Surgical office; nor should any but registered General Practitioners be entitled to demand or recover fees for Medical and Surgical advice and attendance, or for medicines prescribed or administered.

VIII.—*Penalties on Unqualified Persons, and for false pretences of qualification.*—That penalties should be imposed by summary process on all unregistered persons practising Medicine or Surgery.

Also, on all unregistered persons falsely pretending to be registered.

Also, on all persons assuming any professional name or designation to which they are not by law entitled, or which implies that they belong to a class in the Register in which they are not registered, or that they are Members of a College in which they are not enrolled.

In thus laying down the principles on which a Bill should be framed for regulating the whole Medical Profession, the Committee has entered into particulars only as regards the Profession in England; but, under the fifth head, respecting reciprocity of practice in the three kingdoms, it has assigned the conditions which are clearly indispensable for the attainment of that desirable object.

The Committee is aware that some variations from the plan which is here drawn out for England may be rendered advisable or necessary, by local circumstances and the rights of existing institutions, in Scotland and Ireland.

And the Committee is contented that such variations should be made, provided the principle be not contravened, that equality of education and qualifications in each class respectively of the Profession in the three kingdoms should be first obtained, in order that the right of reciprocal practice may be justly allowed.

(Signed) J. A. PARIS, President of the College of Physicians.

BENJ. TRAVERS, President of the College of Surgeons of England.

EDW. BEAN, Master of the Society of Apothecaries.

R. R. PENNINGTON, President of the National Institute.

February 1848.

It would be premature to judge of this measure without a more intimate knowledge of its details. Much seems to depend upon the provisions of a new charter

to be granted to the Royal College of Physicians in London. The right of practising in every part of the British dominions is, of course, a *sine qua non*; but as there are only two colleges in Edinburgh, while there are three in London, there will be a necessity of modifying the bill in its application to the northern part of the kingdom. We consider, also, that a Fellow of either of the Royal Colleges, whether English, Scotch, or Irish, should be entitled, on changing his residence, to be admitted as a Fellow of the College under whose jurisdiction he may place himself, without examination. We do not object, however, to a ballot, as a test of respectability, or to the payment of a moderate fee, as an equivalent for any local privileges he may thereby possess. How, we should like to know, are the numerous and highly respectable physicians, scattered all over the country, to be considered, who obtained their degree from the Scotch Universities? Must they in future hold their right to practise from a College of Physicians, instead of directly from a University, and, if so, on what terms? These are very serious questions, to which we can discover no solution in the "Principles" before us.

BOOKS RECEIVED.

WE have received several letters from authors, informing us that they have forwarded works which have never come to hand. We invariably give a list of all works received up to the 25th of the month, and our correspondents may be assured that, if not mentioned, their books have been mislaid or not forwarded. The new post-office regulations point out a convenient mode of transmitting books, prepaid.

1. Contributions to Aural Surgery. By W. R. Wilde, M.R.I.A., &c. Part III. Dublin. 8vo. 1848.

2. De L'Insensibilité produite par le Chloroforme, et par l'Ether, et des opérations

sans douleur. Par le Dr C. Sédillot, &c. &c. Paris. 8vo. 1848.

3. Facts which prove the immediate necessity for a measure of Sanitary Reform. By John Charles Hall, M.D., East Redford, &c. London. 8vo. 1847.

POST TRANSMISSION OF THE JOURNAL.

WE beg leave to announce that, in consequence of arrangements made with the authorities, the Proprietors will be enabled to forward this Journal by post, to any part of Great Britain and Ireland, **FREE OF ANY EXTRA CHARGE.** All orders pre-paid to Messrs Murray & Gibb, Printers, Edinburgh, who have undertaken the management of the Post Edition, will be carefully attended to. This arrangement will be of the greatest advantage to our readers in remote districts, or in cases where present subscribers are not regularly supplied.

TO CORRESPONDENTS, &c.

PROFESSOR GREGORY'S LETTER ON THE CHEMISTRY OF FOOD.—After mature consideration, we have determined on not inserting the second letter of Dr Gregory. This resolution has been formed in no way out of disrespect to him, but from a conviction that it would open up in the pages of this Journal the whole of the Protein controversy; which, however interesting to chemists, would not be considered so by the majority of our readers.

LABATT AND M'ADAMS ON INSANITY.—In our February number we inadvertently made a mistake in stating, that Mr Labatt was said to have "disqualified himself for competing for the Sugden prize by divulging his name to one of the judges;" we should have said, that Dr M'Adams was alleged to have done so.

Owing to the number of original communications in the present number, we have been unable to insert the reviews now ready on the works of Hardy and M'Clintock, Smith, Heinrich, &c. &c.

The papers of Professor Pirrie, Dr Parker, and Mr Kerr, in our next.

None of our AMERICAN EXCHANGES have been received for a considerable time. The last number of the "Philadelphia Medical Examiner" which has come to hand, is for June 1847.

ERRATUM IN LAST NUMBER.

Vide page 643. 15 lines from the top—for "on the addition of a few drops of nitric acid"—read "on the application of heat, cleared by a few drops of nitric acid."

THE
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No. 23. NEW SERIES.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Remarks on Modes of Proceeding in regard to the Hernial Sac in the operation for Strangulated Hernia.* By WILLIAM PIRRIE, M.D., Regius Professor of Surgery in Marischal College; one of the Surgeons of the Aberdeen Royal Infirmary, and formerly Lecturer on Anatomy in the University of Aberdeen.

It is a question of great importance, in reference to the operation for strangulated hernia, which of the two following modes of proceeding in regard to the hernial sac is the more advisable; namely, that of opening the sac, and dividing the stricture from within; or that of dividing the stricture, and replacing the parts without opening the sac. Of these two, technically called the *intra-peritoneal* and *extra-peritoneal* modes of division, the former is that, which, except in a limited number of cases, has received the sanction and adoption of most surgical authorities in these islands. It appears certain that, in the great majority of cases, it is by that mode alone, that it is possible to accomplish the two grand indications which it is desirable to fulfil by the operation; namely, the removal of the pressure by division of the stricture, and the return of the hernia. The fulfilment of the former, namely, the removal of the pressure by division of the stricture, is essential to the safety of the patient; and the latter, the return of the hernia, exceedingly desirable when practicable and proper.

With regard to the FIRST indication, when the stricture is external to the sac, as is not unusual, it is possible to divide it by adopting either mode; but if formed by the sac, or within it, it is clear, that by intra-peritoneal division alone can the more impor-

tant indication be fulfilled, or any good effected. Cases belonging to the latter class are by no means of unfrequent occurrence. That the neck of the hernial sac occasionally constitutes the stricture, is a point regarding which surgeons are agreed, instances having been recorded by the great surgical authorities of this and other countries, and examples occurring frequently in the practice of many surgeons. The sac, necessarily narrower at its neck than in other parts, is liable to be still further diminished by effusion and organization of lymph, either on its outer or inner surface, as well as by a thickened and indurated state of its own substance, conditions which, separately, or in various degrees of combination, diminish the canal of the sac. For eighteen years I have availed myself of every opportunity of examining the condition of hernial sacs, and from my dissections I am led to conclude, that, in herniæ of considerable standing, thickening of the neck is of frequent occurrence. Although constriction, when sufficient to render a hernia irreducible, is usually at the neck of the sac, yet it is not invariably so. This fact is of little practical moment if a hernia be merely irreducible; but it becomes of the greatest importance if it be strangulated, and require an operation, as the paramount object of the operation is to divide the constriction, in order to relieve the symptoms of strangulation.

The stricture is occasionally found within the sac. In a very few instances it has been found to be occasioned by a loop of intestine; in some by a band of omentum; and in others by a band of lymph effused from the serous coat of the intestine, and surrounding and constricting it as by a ligature.

This last-mentioned condition has been described and delineated by Sir Astley Cooper. It has also been met with by other surgeons; and not fewer than four cases of it have come under my own observation.

The *first* case was that of a female about sixty years of age, of a full habit of body, and the subject of a strangulated umbilical rupture. Her medical attendant, a surgeon of long standing in Aberdeen, found it necessary to have recourse to an operation, and of that I was a witness. The hernia returned very suddenly as soon as the margin of the umbilicus was slightly divided; but the symptoms of strangulation continued, and the patient died in ten hours after the operation. I was requested to conduct the post-mortem examination; and, on opening the abdomen, found behind the umbilicus a swelling about the size of a small orange, formed of intestine, with a neck surrounded by a band of lymph, which embraced and constricted the part, as by a cord. The lymph had been effused from the serous coat of the intestine in consequence of the inflammation excited by the pressure of the margin of the umbilicus. In this case the hernia returned, but without the stricture having been divided.

The *second* case was that of a female, a patient of my own, about the middle period of life, on whom I had occasion, with the assistance of Mr Paterson, surgeon in Aberdeen, to perform the operation for strangulated femoral hernia. On carrying up the point of my finger between the hernia and hernial sac to feel for the stricture, I was struck with the circumstance, that the tightness of what I supposed to be the stricture, bore no ratio to the extreme urgency of the symptoms of strangulation, and that, after dividing some of Poupert's ligament, by cutting from within the hernial sac, the intestine on being gently

pressed still remained as tense as formerly, and its contents did not seem to be moved by the pressure. I therefore examined the neck of the hernia with my finger, and perceived a band of lymph keeping the part tightly constricted, and, in short, constituting the stricture. I gently drew down the intestine, and cut the band in several different parts, when the contents of the intestine could be easily made to move upwards. On being satisfied that all constriction was removed by dividing the band of lymph in various parts, the intestine was returned into the abdomen, and the patient recovered without an unfavourable symptom. If the hernia had been returned without this band of lymph having been discovered and divided, the object of the operation would have been unaccomplished.

The *third* case was that of a female, about sixty years of age, of a remarkably full habit, and who, about two days before I saw her, had been seized with symptoms of strangulation. When I first saw her, the abdomen was tympanitic to a great degree; the vomiting was most distressing; the bowels had not been moved for five days, and she had every symptom of sinking very rapidly. She stated that she had often on previous occasions had attacks of what she believed to be colic, and imagined at first that the illness from which she was suffering was only a return of that disorder, and, consequently, anticipated a speedy recovery. I was also informed that, for a considerable time, she had had a disagreeable feeling of tenseness in her left groin, though without swelling, so far as she could perceive; and that, some hours before I was called, while drawing up her limbs in a fit of retching, she felt, to use her own expression, as if something had given way in her groin, and from that moment was relieved from all feeling of tenseness. The symptoms of strangulation, however, continued. I made a most minute examination of all the usual seats of hernia, but could detect no symptom of such a lesion. I requested my colleague, Professor Macrobis, to attend the patient along with me, which he did, and he was also present at the post-mortem examination. On opening the abdomen, there was at its under part a small tumour of intestine seen, before any parts had been disturbed beyond merely turning down the abdominal parietes. It was of a livid colour, about the size of a walnut, and with a narrow neck, tightly embraced by a band of lymph, by which it was so constricted as to make it difficult to pass a probe from that part of the intestine which led to the swelling, into that which constituted the tumour. The intestine was also twisted over itself in form of a loop. On examining the femoral canal of the left side, a hernial sac was found in it; and the tumour of intestine had, no doubt, formed a hernia, but returned of itself. The stricture, however, formed by a band of lymph, still remained. Sir Astley Cooper records a case in which Mr Weston returned a hernia by the taxis without an operation; but the symptoms of strangulation continued, and it was found that the stricture was caused by a band of lymph which embraced the intestine. In the instance of my patient the hernia returned without any assistance.

The *fourth* case was that of a female, whom I had never seen during life, but at the post-mortem examination of whose body I was present, in consequence of the request of a medical man who had seen her a short time before death, and who had also often attended her on previous occasions, when in a state of great suffering from disease of the womb. The symptoms, I was informed, were those usually induced by a strangulated hernia; but the medical man could not detect any swelling in any of the usual seats of hernia. On examining the left groin before opening the abdomen, I thought I felt a very small swelling, which I suspected to be a hernia, and I therefore made a careful dissection of the parts in presence of the surgeon, who requested me to do so, and of one of my pupils. On cutting through Poupart's ligament from before backwards, the contents of a small hernial sac returned into the abdomen without being touched, and were found to consist of intestine strangulated by a band of lymph, embracing the neck of a small hernia. The hernia was not much larger than a walnut. If it had been discovered during life,

and been made the subject of operation, there would have been great risk of its returning into the abdomen without the real stricture being discovered or divided.

From what is stated above, it appears very clear, that the extra-peritoneal mode of herniotomy is quite unsuitable when the stricture is formed by the sac or within it, whatever be the nature of the stricture itself; and the above-mentioned examples of strictures formed by membranous bands, suggest very strongly the propriety of great caution in arriving at the determination of adopting the extra-peritoneal division; as well as in those cases in which the sac is opened, of examining very carefully, before the hernia be returned, whether membranous bands do or do not exist.

The *second* indication which it is desirable to fulfil by an operation, is the return of the hernia. With a view to facilitate the inquiry, as to which of the two modes of procedure is the more suitable, cases may be arranged into the three following classes:—

First, Those in which the stricture is external to the sac, in which it is neither impracticable nor improper to return the hernia, and in which no obstacle exists to that return after the stricture has been divided. *Secondly*, Those in which an obstacle does exist after division of the stricture; and, *Thirdly*, Those in which the return of the included intestine would be practicable, but improper.

First, In cases belonging to the first class, either mode is applicable; but extra-peritoneal division being attended with much less danger, is decidedly preferable.

Secondly, In regard to cases in which, independent of the stricture, an obstacle to reduction exists, it will be proper to consider what are the principal obstacles most frequently met with. These are adhesions of the protruded parts to the hernial sac, the natural means of connexion, in some rare cases; adhesions of the protruded parts to each other; and the large size of the hernia.

Adhesions of the protruded parts to the hernial sac often constitute an impediment to reduction. Of these adhesions there are three varieties.

1st, The protruded parts sometimes adhere to the sac through the medium of a layer of coagulable lymph. This form was described by Scarpa as the *gelatinous* or *glutinous* adhesion; and as it is a condition of parts which very quickly takes place, the surgeon should endeavour to return the hernia as soon as possible, in order to prevent the slight inflammation which gives rise to the effusion.

2d, Adhesions sometimes assume a membranous or filamentous appearance, varying greatly as to the number and length of the filaments. As a general rule, adhesions of this form are only found connecting moveable parts with each other, as the intestine with the hernial sac, or with the omentum. They are precisely similar to the bands we often find between serous surfaces in other parts of the body, and are produced by the effusion of coagulable lymph, which

ultimately becomes organized. In this respect they differ from the last-mentioned form. The lymph thus effused is drawn out into bands or filaments by the movement of the intestine, which accounts for the circumstance of their being found chiefly connecting moveable parts to each other, and for their being more frequent at the body and fundus of the sac than at its neck or mouth, where the parts are in a more confined space and have less motion.

3d, The third form of adhesion, which usually receives the name of the *close organized*, or the *fleshy*, is, like the gelatinous and membranous—a result of inflammation; but differs from them, inasmuch as the union is close and firm, so that the protruded parts and the sac cannot be separated from each other, but form a solid mass, the vessels of which are continuous. In a case of strangulated hernia, the subject of operation, this form of adhesion requires a very different method of procedure from the gelatinous or membranous, as will afterwards be stated. This species of adhesion is very frequently met with between the omentum and hernial sac; and then is generally at the body and fundus of the sac. When it is found between the intestine and sac, which is a rare occurrence, it is usually at the neck. Scarpa has described this form under the name of the *unnatural fleshy*, to distinguish it from what he calls the *natural fleshy*, which is of an entirely different character, as will presently be shown. These three forms of adhesion agree with each other in being caused by inflammation, and in being attended with effusion of lymph; but they differ, inasmuch as the lymph in the first form is not organized; while, in the second, it is organized and elongated into bands or filaments; and, in the third, although organized, it is not elongated, but effused between the sac and protruded parts, so as to convert them into a solid inseparable mass, the vessels of which are continuous.

The obstacle to reduction is sometimes furnished by the natural means of connexion between the intestine before its descent, and the peritoneum lining the surrounding part of the abdomen. It is of the greatest importance that the surgeon should have distinct ideas of this condition of a hernia; for, if it be not understood, and an operation be necessary, the most fatal errors may be committed. Scarpa has given an exceedingly clear and full explanation of this condition of a hernia. It has also been described by Pelletan, Cloquet, and Hesselbock, and with great distinctness by Mr Lawrence. The natural means of connexion of the hernia with the surrounding parts, may form the obstacle on the right side if the hernia be formed by the cœcum or head of the colon, or on the left if it be formed of the sigmoid flexure of the colon. These divisions of the alimentary canal are covered by peritoneum laterally and anteriorly, but are in a measure destitute of peritoneal covering behind; and the peritoneum is reflected from their lateral aspects to the parietes of the abdomen in the ileo-lumbar regions, with which parietes it is connected by loose cellular tissue

capable of great elongation. The natural means of connexion of these divisions of the alimentary canal with the parietes are short, and formed of the peritoneum extending between that portion of the membrane which furnishes a serous coat to the intestines, and that which lines the walls of the abdomen. If these portions of the alimentary canal descend to form a hernia, they will drag along with them the part of the peritoneum which naturally lines the parietes of the ileo-lumbar region to form the hernial sac; and if the hernial sac descend into the scrotum, and there form adhesions to the surrounding parts, the portions of peritoneum which, within the abdomen, preserved the intestine in its natural relations to the walls of the abdomen, will now retain it in the sac; and as, through the medium of these portions, the hernial sac and serous coat of the intestine, which forms the hernia, are continuous with each other, it is evident that reduction must be impracticable. It is as impracticable under these circumstances to return the intestine, as it would be to return the testicle into the abdomen; the intestine draws the peritoneum along with it to form hernial sac, and the testicle draws peritoneum to form tunica vaginalis; and the serous coat of the intestine has the same relation to the hernial sac as the tunica vaginalis propria has to the tunica vaginalis reflexa. Such a hernia, when it becomes strangulated, and the subject of operation, requires a particular method of treatment, which will afterwards be explained.

Adhesions of the protruded parts to each other, often form the impediment to reduction. The parts which form a hernia often glide down separately and to a great extent into the sac; and afterwards, by pressure and various accidental causes, adhere to each other, and cannot in mass be returned through the opening by which they separately left the abdomen.

A frequent impediment to reduction is the bulk of the protruded parts in relation to the opening through which they would have to be returned. The bulk is sometimes owing to the quantity of parts which have come out from the abdomen, especially in neglected cases, where means have not been used to give a degree of support. Another more frequent cause is the enlargement or growth of some parts constituting the hernia. The omentum and mesentery are the parts which, when protruded, present the impediment to reduction from growth, and their increase is occasioned mostly by deposition of fat in the portions of these tissues external to the opening through which they came out from the abdomen. Where they are embraced by the opening, the pressure prevents enlargement in that situation; but from the yielding nature of textures external to the opening, the increase of volume is often very considerable. In old herniæ, which have been long irreducible, this condition is sometimes met with to a great extent. Such are the principal conditions which, independent of the stricture, offer an obstacle to the reduction of a hernia.

If any of these conditions exist, and if the sac be not opened, re-

duction is in general impracticable. If the sac be opened, two of them may easily be overcome; namely, the soft recent adhesions formed by coagulable lymph and the filamentous,—the former can be broken down with the finger, the latter divided by the knife. Two of them present an insuperable impediment to reduction; namely, the natural means of connexion, and the close organized adhesions, if they be to a great extent, and the hernia large. With regard to the two remaining conditions, the possibility of overcoming them, and the propriety of attempting to do so, must depend entirely on the particular circumstances of the case; but in many cases it is more judicious not to interfere with them, unless they exist only to a limited extent, and in herniæ of moderate size. Most of these conditions, however, are principally met with in cases of large and old herniæ; and, on account of the risk of injuring the intestine in attempts at reduction, as well as that of inducing dangerous inflammation by much handling of the intestine, and the difficulty of maintaining the parts reduced, even should reduction be possible, the majority of surgeons seem now disposed to follow the advice of Sir Astley Cooper regarding such cases. His practice was to divide the stricture, which fortunately in such cases is, for the most part, external to the sac, and to leave the latter unopened, and the hernia unreduced. The stricture being divided, the principal cause of danger is removed. The coverings of the hernia should be replaced, and proper means taken for promoting the healing of the wound.

Thirdly, There are certain states in which it would be extremely improper to attempt reduction; namely, when the hernia is gangrenous, or when the intestine has given way from inflammation having gone on to gangrene, or when it has been torn, or accidentally wounded in the operation. The two last-mentioned conditions can only result from unskilfulness in the mode of procedure; but, should they exist, the hernia ought not to be returned. When the intestine presents such an appearance as to render it doubtful whether its return may be followed by fecal extravasation, the surgeon should content himself with carefully dividing the stricture. In all cases in which the intestine is gangrenous, or not entire from whatever cause, it ought to be allowed to remain, so that the feces passing off by the wound may form an abnormal anus, and extravasation into the abdomen be thereby prevented. When omentum forms the hernia, and it is gangrenous, the gangrenous portion may be removed, and the remaining part returned to the abdominal aspect of the mouth of the hernial sac. The practice of removing a portion of omentum, when from growth it renders a hernia irreducible after division of the stricture, is a proceeding which, in some cases, may be adopted with advantage. For cases belonging to this class, extra-peritoneal division is of course quite unsuitable. These remarks, it is to be hoped, will be sufficient to point out the proper mode of procedure when the hernia is sound, and reducible after division of the stricture; when it is irreducible after such division—and when

it is in any of the various conditions in which reduction would be dangerous and improper; and also to show, that to follow one method indiscriminately in all cases would be unwise; that intra or extra peritoneal division should be adopted according to the particular circumstances of the case; that in the majority of cases intra-peritoneal division is not only the more suitable mode, but the only one which is safe, or by which any good can be effected; and that the cases in which extra-peritoneal division is suitable are those of very short standing, where there is no reason to apprehend the existence of adhesions, or of an unsound condition of the hernia; and in cases of large and old herniæ, where the more judicious proceeding is to divide the stricture, and not to attempt reduction.

The plan of not opening the sac, although practised in certain cases by Franco and Paré, was first strongly recommended by Petit, and consequently has been designated the *method of Petit*, to distinguish it from the mode in common use. Petit practised this method as early as 1718. It was subsequently advocated by Garregeot; and, at a still later period, adopted and strongly recommended by Bonnet of Lyons.

In this country it was introduced by the second Monro, who advocated its adoption in cases of small and recent hernia, and mentioned four cases in which he resorted to that mode of proceeding. In one of them, however, adhesions prevented the return of the hernia, and in two of them he was obliged to cut the neck of the sac. In later times, the same proceeding was adopted by Sir Astley Cooper in cases of large and old herniæ, and strongly recommended by him as the decidedly preferable mode in cases of that class. Mr Lawrence, in his valuable *Treatise on Ruptures*, remarks, "The plan of removing the stricture, and returning the prolapsed parts without opening the sac at all, ought, I think, to be more frequently adopted than it has hitherto been, although it appears objectionable as a measure of general use, in the operation for strangulated hernia." To Mr Key, however, the merit undoubtedly belongs of having recommended a more general adoption of Petit's mode than had previously prevailed in this country. In his admirable *Memoir on the Advantages and Practicability of dividing the Stricture in Strangulated Hernia on the Outside of the Sac*, published in 1833, will be found much valuable information on this interesting subject. Mr Luke of the London hospital strongly recommends this mode, and his success is a decided testimony in its favour. Out of nearly forty patients he has not lost more than two. In October 1845, when I had occasion to be in London, Mr Liston showed me a patient in the North London hospital, in whose case he had adopted this mode; and, in a communication I afterwards received from him, he informed me that he had practised it in a few other instances, and felt convinced of its being the preferable mode when the hernia is small and recent, and when there is no reason to apprehend an unsound state of the intestine. And,

judging from the recorded statements of some other distinguished surgeons, this mode seems to be meeting with deservedly increased favour; and I have no doubt will continue to do so, if practised under the limitations already mentioned.

There can be no doubt that intestinal inflammation is the most frequent cause of death after the operation for strangulated hernia. Some of the advocates of Petit's method have assigned as the causes of that inflammation, when the ordinary proceeding is adopted, the exposure of the intestine to light and air, change of temperature, and handling. I agree with Mr Lawrence in ascribing it not to these agents, but chiefly to the long-continued pressure of the stricture, owing to the operation being *too long delayed*, and to an injudicious and *too frequent use of the taxis* previous to the operation. I remember being very much struck with an observation of Desault's; I have not his works beside me at present, but it is to this effect—"Think well of that hernia which has been little handled and soon operated on." The operation is justifiable and necessary when the taxis has been first tried alone, and has then, in combination with such auxiliary means as seem advisable, been again fairly and skilfully tried without producing the desired effect. The conviction being thus produced, that by no other means than an operation is there hope of saving the life of the patient, it ought to be resorted to as quickly as possible. Much handling must not only give unnecessary pain, but also increase the risk of hurrying on the inflammation to results, which, even though the operation should be performed, would render it unsafe to return the hernia. When therefore the taxis, and also along with it the proper auxiliaries, have been fairly and skilfully tried, no advantage can, but considerable injury may, result from the repetition of treatment already found to be unavailing. Many considerations show that the operation should be performed as soon as possible, after its inevitable necessity has been found to exist. Delay, like undue handling, increases the risk of inducing such a state of the hernia, in consequence of inflammation, as would render its return unsafe. From the short time in which a hernia may prove fatal, and from the depressed state which comes on in consequence of delay, rendering the patient less able to stand the shock of an operation, will be seen the importance of being as prompt as possible; but there is another, and a very urgent reason—namely, that, if the operation be delayed until intestinal inflammation has been induced within the abdomen, it is far from certain that this inflammation will subside on the removal of the hernia which caused it. I have performed the operation for strangulated hernia, according to the usual mode, a very considerable number of times, and in every instance with success; which I attribute to two things—namely, avoiding all undue and useless handling, and performing the operation early. My decided impression is, that the reason why the operation is so frequently fol-

lowed by death, instead of being one of the most successful of the great operations of surgery, is, too great delay in resorting to an operation, and the undue and injurious use of the taxis, even after its adoption alone, and in conjunction with the proper auxiliaries, has proved unavailing.

ARTICLE II.—*On the advantages of Turning in certain Cases of Narrow Pelvis.* By JAMES WILSON, M.D., Member of the Faculty of Physicians and Surgeons of Glasgow, and Consulting Surgeon to the Glasgow Lying-In Hospital.

[*Read to the Medico-Chirurgical Society of Glasgow, on the 12th October 1847.*]

WHEN the head, in certain cases of parturition, is prevented from entering the brim of the pelvis from defective form, or insufficient space, are we to resort at once to embryotomy—the general practice; or may we not, with some prospect of saving the child, accomplish the delivery by turning?

For upwards of thirty years I have been in the habit, in such cases, of effecting delivery by turning, and I have good reason to believe that the lives of many children have been saved by this practice, without the safety of the mother having been in any degree endangered.

In my opinion, turning can only be effected safely and successfully under certain conditions. I would not undertake its performance when labour had been long continued—the patient's strength exhausted—the uterine energy gone, or the uterus painfully and permanently contracted; nor would I recommend it when there was reason to suppose the child was dead, the pelvis *very* much contracted below its usual dimensions, or when the attendant was not familiar with the operation of turning.

Such is the deliberate opinion I have formed of turning in such cases; but at this distance of time it is impossible for me to state the circumstances which led me to adopt the practice. I rather think it was in consequence of having met with several breach presentations in the same individual, and where, by permitting the breach to come forward in the two first instances, the labours were tedious and severe, and the children were lost. In the third instance I saw the patient early, and, ascertaining the presentation to be the same as formerly, I introduced the hand as soon as the state of the os uteri would permit, brought down the feet, and the delivery was accomplished in half an hour: the powerful co-operation of the uterus greatly aided in the extraction. The child was quite lively. Two deliveries afterwards in the same individual were managed in the same way, and with like success.

The success attending these cases, led me to make some inquiries, which resulted in the following conclusions. When the head of the child presents at the brim of the pelvis, but cannot enter in consequence of the relative dimensions of the parts, the points of the

head pressed upon are likely to be the upper and posterior parts of the parietal bones, and the superior part of the occipital bone. Now, the effect of such pressure must be to lower the arch of the cranium, and to the same extent to increase the transverse diameter of the head—thus augmenting the disproportion between the relative size of the head and pelvis. This may be proved to a demonstration by applying a certain amount of pressure to points above specified of a new-born child's head.

On the contrary, after turning, when the child's head comes to the brim of the pelvis, the parts chiefly exposed to pressure are the squamous portions of the temporal and the sides of the parietal bones. Now, by experiment, it will be found that the result of such pressure is to diminish the transverse diameter of the head,—raise the sagittal arch and elongate the head, as we find occurs when the pelvis is rather narrow, or when the forceps are used. By this change, from a quarter to half an inch of space may be gained, and the head made to pass with comparative ease, when it could not have been made, by any means, to enter the pelvis with the vertex to the brim.

By turning early (and the sooner the better) we have the unimpaired vigour of the uterus to assist us in our extractive efforts:—the benefits of which, in these operations, I fear, is not sufficiently estimated. By turning, we have also in our power to bring the head into the best possible position for being brought through the varying diameters and changing axis of the pelvis. Besides, we can safely exert a considerable degree of extractive force by means of the body of the child, and that more effectually than we can do with the crotchet. Oftener than once I have been obliged to turn after the head was perforated, in order to finish the delivery when it could not be accomplished by means of the crotchet.

In turning, we simply follow the example set us by the uterus when it meets with insurmountable difficulties, either when the head or some other part of the child presents, and which it cannot force into the pelvis by its most powerful efforts. The presenting part becomes changed,—what Denman calls spontaneous evolution takes place, and the breach is ultimately expelled.

Dr Denman's description of such cases is more in accordance with my own experience than any other I have met with; it is my decided conviction that actual turning takes place.

The first case of the kind occurred in my practice in Carluke, thirty-seven years ago. The head, shoulder, thorax, and abdomen, passed in succession across the brim of the pelvis, and the breach was at length expelled.

From the preceding and other cases of the same kind, that have come under my observation, I feel convinced that the uterus often attempts turning when obstacles are opposed to the entrance of the head into the pelvis, and that a great proportion of the preternatural presentations met with arise from this cause.

Many cases might be adduced in favour of the propriety of turn-

ing, when the pelvis cannot, or will not, admit of the head passing through it entire; but I shall only mention shortly, six which came under my notice in the same individual.

Mrs ———, æt. twenty-seven, first pregnancy, had been in labour for several hours before I saw her. The funis umbilicalis was in the vagina, and the head presented at the brim of the pelvis; the cord could not be kept up, and from continued pressure the pulsation soon ceased. The labour continued for eighteen hours, when the child was expelled dead, and very flaccid. The pelvis, so far as could be ascertained, was under three inches from pubis to sacrum. This deficiency was not ascertained during the labour.

In the *second* pregnancy, I found the head and cord presenting as in the former instance; the liquor amnii had escaped, and the os uteri was so rigid as not to admit the hand till the expiry of twelve hours. Turning was then effected, and a still-born female child extracted without much difficulty. During this and the subsequent pregnancies, the patient could not be persuaded to submit to the induction of premature labour, which would have been the preferable practice.

In the *third* confinement, the head and cord were again found presenting. In ten hours from the commencement of labour, the membranes being still entire, turning was had recourse to, and a living daughter was extracted.

In the *fourth* confinement, the head presented alone, and the pains being very inefficient, fifteen hours elapsed before the hand could be passed to turn, when a living daughter was extracted.

When called in the *fifth* instance, the os uteri was not at all opened, and great care was taken to preserve the membranes. At length the os uteri was so far opened as to admit the hand; the side was found presenting, and the child was turned, and delivered with considerable difficulty; it was still-born, but was so far resuscitated by artificial means as to live for several hours.

In the *sixth* instance the head presented; in six hours the os uteri was so far dilated as to admit of turning, and a large living son was extracted. The forceps were used in this instance after the child's head had passed the superior strait, in order to overcome some obstacle which prevented the further progress of the head at the outlet.

In all these cases the recovery of the mother was speedy, and as after natural deliveries, with the exception of the last, where pleuritis set in, but the recovery was complete in three weeks. Nor, in any of the cases where the child had been delivered by turning, did their heads present any very marked degree of compression; it did not amount to depression of the sides of the head.

With respect to these six cases I may remark, that the two first children were lost in consequence of the membranes giving way early, and the cords being subjected to fatal pressure. The fifth child died, or was still-born, in consequence of being above the average size, and from the great difficulty experienced in its delivery. But the other three were saved (and are still alive), in my opinion, by turning alone.

Since writing the above I have been called to a case of narrow pelvis which ended fatally. It was a first pregnancy, and the woman had been eighteen hours in labour,—during the last twelve the pains had been frequent and extremely severe, without producing any change on the position of the head, which rested above the brim of the pelvis, and thrown considerably forward over the pelvis by

the great projection of the promontory of the sacrum. This projection was so great as to render, so far as I could judge, the inlet of the pelvis in its conjugate diameter not more than $2\frac{3}{4}$ inches. The os uteri was fully opened, and very lax and soft, as I have generally found it after rupture of the uterus has taken place; but the cause of this condition was afterwards ascertained. The patient was extremely restless, and the pulse very quick and small.

Turning was very well effected by Dr Stewart of Candleriggs, but with considerable difficulty. The head, however, was brought down with more ease than could have been anticipated from the contracted state of the pelvis. The child—fully the average size—was dead. On the upper part of each parietal bone was observed a deep cuplike depression, corresponding with the points which had been so long and so severely pressed against the promontory of the sacrum and the symphysis pubis. I measured from the centre of one of these depressions to the centre of the other, and ascertained the distance to be $2\frac{3}{4}$ inches; and the measurement with the hand in the pelvis, after the delivery of the placenta, gave the same limited space.

After delivery, the patient continued restless as before, and the pulse was equally quick and feeble. She complained of no abdominal uneasiness, but sunk exhausted twenty hours after delivery.

On inspecting the body, twenty-four hours after death, the abdomen was very much distended; the intestines distended with flatus, and a great quantity of dark bloody fluid was found in the abdomen. The uterus was not ruptured, as had been feared, but the posterior part of the cervix, which had been so long subjected to the severe pressure of the child's head against the promontory of the sacrum, was reduced to a perfect jelly; showing the source from which the blood had escaped. The anterior portion of the cervix was not injured, and the uterus, with the exception of the part referred to, in a healthy state. The promontory of the sacrum projected to a great extent. The space between it and the symphysis pubis measured exactly $2\frac{3}{4}$ inches, and from side to side the distance was $4\frac{1}{2}$ inches.

The only thing we had to regret in this case was, that the labour should have been permitted to continue so long. Had delivery been effected twelve hours earlier, there is hardly room to doubt that both mother and child would have been saved.

It may be said that opening the head in this case would have been preferable to turning, as neither mother nor child were saved; but I do not think so. Turning was effected with the view of giving the child the chance of life, and the mother did not incur any additional risk. The fatal injury was inflicted by the pressure of the head before the turning, as was manifest from the loose and flabby state in which we found the os and under parts of uterus before delivery, and also from Dr Stewart having experienced no resistance from the lower parts of the uterus, but from the upper in passing the hand to reach the child's feet. It may also be said, that by bringing the child's head through a pelvis so narrow, we

inflicted the injury on the cervix uteri; but that is disproved by the reasons just given, and also by the comparative ease with which the head was extracted.

In giving a preference to turning (and I think I am warranted from the preceding statements to do so), rather than at once resorting to the perforator and crotchet, in these cases, I do not mean to assert that these instruments should never be used; but I say that their use involves very serious consequences, and that they ought not to be resorted to without the most mature and deliberate consideration.

The rules laid down in books sanctioning the use of the perforator are conflicting and indefinite, and all apply to the head presenting—not to the head when turned. Dr Burns says, “It has been stated by high authorities, that if the dimensions of the pelvis were certainly under three inches, a living child could not be born. This opinion,” he adds, “is decidedly true.”

Dr Osborn states, “I have endeavoured to prove that a child at full maturity cannot be born alive, by any means of nature or art, when the dimensions of the pelvis are not $2\frac{3}{4}$ inches from pubis to sacrum.”—(P. 251.)

Dr Ashwell says, “For when at the brim the pelvis instead of $4\frac{1}{4}$ from pubis to sacrum it measures no more than $1\frac{1}{2}$, $1\frac{1}{4}$, 2 or $2\frac{1}{4}$ inches, the use of instruments becomes absolutely requisite, and very frequently in those of $2\frac{1}{2}$ and 3 inches.”—(P. 398.)

Dr Davis says, “In the case, for instance, of a pubis having for its conjugate diameter any measure short of $2\frac{1}{2}$ inches, and that fact being assumed to have been ascertained positively, he may then be permitted to introduce his perforator at almost any period of the labour.”—(P. 1159.)

Dr Hamilton says, “That the practitioner should constantly, whenever the pelvis falls under the ordinary standard, provided its dimensions be not below 2 inches in the short diameter, to wait patiently till he be absolutely convinced that the child cannot be expelled alive by the efforts neither of nature nor of art.”—*Cases in Midwifery*, p. 67.

Thus we see, from these discordant statements, that no rule can be laid down to guide us in practice from the mere measurement of the pelvis. Before a safe rule could be laid down it would be necessary to ascertain the exact size, the form, and the state of ossification of the child's head,—which is beyond our power. Such is the difference in these points, that of two heads at full time, one will pass through a pelvis half an inch short of the space which another will require. Besides, the measurement from pubis to sacrum gives no sure indication of the pelvic space—the projecting promontory may be thrown very much to one side, leaving so much space at the other that a head of small dimensions, and of compressible conformation, may pass easily through it.

Two days ago I saw a case where the head, notwithstanding

powerful and long-continued uterine action, could not enter the conjugate diameter. I pushed the head from this narrow passage, which did not measure more than two inches, towards the right side, where there appeared to be more space, when two or three pains expelled a dead child: the base of the skull measured $2\frac{1}{2}$ inches.

Without, therefore, trusting to measurement alone to guide us in our mode of acting in these cases, perhaps the best practice is when we find that, after the uterus has been acting powerfully, and as we think properly, for a due period of time, and the head does not appear capable of being forced into the pelvis, to decide upon the steps to be taken. What are we to do? Are we at once to perforate the head, or permit the labour to go on, as some advise, till the child is dead, and probably also the soft parts of the mother so much injured as to prevent her recovery? or may we rather not resort to the less repulsive expedient of turning, with some chance of saving the child, and without adding to the risk of the mother?

Dr Dewees objects to turning in such cases; but, in my opinion, his admissions amount to a recommendation of turning, at least so far as regards the mother. He says, "It will therefore follow that turning is, and must always be, of doubtful efficacy as regards the child—as one calculated to relieve the mother, it may in many instances be successful—or if the practitioner has been debating within himself the comparative merits of the crotchet or turning, the latter will unquestionably merit the preference, as it gives a chance, though a forlorn one, to the child."—(P. 570.)

An objection urged against turning, is the difficulty connected with its performance. I grant that it is much more difficult than perforating the head; but surely this is no good reason for destroying the child. Although turning is a difficult operation, a perfect knowledge of it, and dexterity in its performance, may be acquired without any extraordinary efforts. But it is said we do not always save the child by turning,—true; but we never can save it by the crotchet. It is further objected, that we cannot always extract the head entire after turning,—true also; but in such cases, when the body is born, we can perforate the head behind the ear and evacuate the brain.

The use of the long forceps has been recommended by some as preferable to turning; but experience has led me to discover that the forceps are altogether inapplicable when the parietal bone overlies the pubis to any degree, as they cannot be placed over the sides of the head, which are the points we wish to compress. The only way in which the forceps can be passed in these cases is by the sides of the pelvis; consequently one blade of the instrument is applied over the face or forehead, and the other over the occiput. Now, compression by forceps so placed, will shorten the long axis of the head, but it will extend the transverse, and thus increase the disproportion which exists between the head and the conjugate diameter of the pelvis, and augment the difficulty we were solicitous to overcome.

There is not the smallest doubt that the crotchet has been frequently used when it ought not to have been. I believe there is no practitioner of experience who has not been sent for to sanction the use of the perforator after turning or the use of forceps had failed, where by another trial, and better directed use of these expedients, a living child has been delivered.

From these, and other facts to which I might refer, it is to be feared that embryotomy is too lightly thought of; and, in conclusion, permit me to state, that our practice in these matters contrasts very strongly and unfavourably with that of criminal courts in cases involving life or death. With us, the destruction of the life of a child is often determined upon even without the formality of a regular consultation; whereas with them such is the importance attached to life—even when that life is loaded with imputed crime—that the greatest talent in the land, and the most respectable of the whole community, are summoned to judge and bear witness in the cause, so that life be not sacrificed without the most undoubted proof of guilt.¹

ARTICLE III.—*On the Medicinal Effects of the Persesquintrate of Iron.* By WILLIAM KERR, Surgeon, Corresponding Member of the Medical and Physical Society of Calcutta.

IN 1832, in consequence of cholera having reached Britain, I published three papers, pointing out the great power of the persesquintrate of iron in curing chronic diarrhoea.² That persons affected with this complaint are more liable to cholera than those whose bowels are firm, cannot, I suppose, be controverted. I therefore was encouraged to hope, that I had put into the hands of medical men a medicine which would diminish the liability of many to cholera, placing them, in this respect, on an equality with healthy people. The dreaded scourge is again approaching, and, should it return, I shall be much gratified if the publication of this paper, by increas-

¹ I consider it proper to state, that this paper was written in answer to certain queries proposed to me by Professor Simpson of Edinburgh, in September last, respecting the propriety of turning in these cases. It was never sent to Dr Simpson, because I continued engaged in testing the compressibility of the heads of new-born children. Dr Simpson kindly sent me his papers on this subject last month. They had been some time published, but I avoided seeing them while I was making my experiments, lest his opinions should influence my conclusions; and it gives me great satisfaction to find, from the perusal of these very excellent papers, that our views and illustrations are so similar. Whatever may be the value of the practice recommended, Dr Simpson has the merit of first giving it publicity.

² Edinburgh Medical and Surgical Journal, Vol. XXXVII., and Glasgow Medical Journal, Vol. V.

ing the means of prevention, be instrumental in diminishing its victims. After an experience of eighteen years, I can confidently recommend the persesquinitrate of iron as capable of curing most cases of chronic diarrhœa. The instances which do not yield to it, are those where there is reason to believe ulceration of the mucous membrane exists. It is therefore generally inapplicable in chronic dysentery, or the diarrhœa of phthisis, which, instead of checking, it aggravates. There are besides some other cases not reducible under any general rule, which are not improved by the persesquinitrate of iron; but I am satisfied that the great majority of cases unaccompanied by ulceration, are curable by this medicine. If my readers take the trouble of examining the original papers, they will find a considerable number of cases cured by it, and, if they prescribe it to their patients, I have no doubt that they will soon have many proofs in their own experience.

Dr Graves states¹ that he has lately used the persesquinitrate of iron with very considerable success. He proceeds to say, that "you will be consulted by females of a delicate and weakly habit, who frequently exhibit symptoms of nervous derangement, such as palpitations, sleeplessness, and headach, who are easily excited or alarmed, have a tendency to emaciation and paleness, and have little or no appetite. Combined with these general symptoms, you find that they have been labouring under diarrhœa for weeks, and even months, and that this, with other causes of debility, has rendered their condition extremely uncomfortable. You will also be informed by the patient, that she has tried many remedies without benefit, and that she is extremely anxious to have something done to give relief; and hence it is a matter of importance to be acquainted with any remedy which may be likely to prove serviceable. This form of diarrhœa is of an unmanageable character, and very seldom amenable to the ordinary modes of treatment. The common astringent remedies totally fail; chalk mixture, kino, rhatany root, and catechu are useless, and in such cases it has been observed, that opium is generally injurious. If you prescribe opium, it certainly checks the disease for a time; but this temporary relief is accompanied by debility, malaise, restlessness, and many uneasy symptoms, and the diarrhœa soon returns and is as bad as ever. The medicine which I have found most effectual in such cases is the persesquinitrate of iron. With it I have succeeded in curing many cases which had been exceedingly obstinate, and of very considerable duration, the disease having, in one instance, resisted all the efforts of medical skill for seven months, and in the other for two years. Seven or eight drops of the *liq. ferri persesquinitratis*, increased gradually to twelve or fifteen in the course of the day, was the quantity prescribed in both cases. In the course of four days a slight diminution of the diarrhœa was per-

¹ System of Clinical Medicine, 1843.

ceived, in a fortnight the patient felt much better, and in a month or five weeks it had disappeared altogether. This took place without being followed by any bad effects; there was no swelling of the stomach, no tympanitis, no tormina, no restlessness or nervous derangement; the patients recovered their health and strength, and the cure was at once safe and permanent."

Cases have occurred to myself where the disease has existed many years, yet were perfectly cured in a surprisingly short time from the use of the persesquinitrate of iron. A great deal of ill health frequently proceeds from chronic diarrhœa. Epilepsy and hysteria, for instance, often exist together with a long-continued relaxed state of the bowels, and do not yield till a remedy is obtained for the diarrhœa. I have seen these and other diseases cured chiefly in consequence of the persesquinitrate of iron checking the accompanying diarrhœa.

Dr Neligan says, that "the persesquinitrate of iron is an admirable astringent, possessing also tonic properties. It will be found particularly useful in chronic cases of mucous diarrhœa, when there is much emaciation and loss of appetite. In such cases, I have derived much benefit from its employment after many other remedies had failed."¹

In some cases, where the ordinary doses (fifteen to thirty drops) thrice a-day aggravated the diarrhœa, I have succeeded by giving for some time smaller doses, such as five drops twice or thrice a-day, gradually increasing the dose as the patient was able to bear it. In a few cases of the same kind, the persesquinitrate has been given in pretty large doses by enema. I have already said, that it is generally inapplicable in phthisis. In my paper in the *Edinburgh Medical Journal*, there is a striking instance of a lady in the last stage of phthisis having diarrhœa stopped by enemata containing this medicine. Intestinal ulcerations, if they existed, were probably beyond the reach of the enemata. Mr Tindal, of Glasgow, informs me of a case of phthisis, where, after the failure of many remedies, the diarrhœa uniformly ceased on the patient using the persesquinitrate of iron. These, however, are exceptions, and the rule is as I have stated.

I have seldom employed the persesquinitrate of iron in recent attacks of diarrhœa, chiefly because these cases are readily controlled by other medicines. When used in such cases, it succeeded best in large doses, such as half an ounce or an ounce given by enema. With the exception of a few cases of children, its administration in cholera in my hands was a failure. As one cause of this possibly arose from its speedy rejection by vomiting or purging, it has occurred to me, since the departure of cholera from this country, that benefit might perhaps be derived from injecting it, largely

¹ Medicines; their Uses and Modes of Administration, 1847.

diluted with water, into the veins, in the same manner as saline injections.

I have lately given the persesquinitrate of iron internally with much benefit in several cases of urticaria. A lady, twenty years of age, who had been afflicted with it since her childhood, and had tried various remedies without success, was cured by the ordinary doses in little more than a month. She is now able to take any article of food, though before using the persesquinitrate she required to be very cautious, and even with the utmost care was seldom a day free from the eruption, and accompanying uneasiness at the stomach.

A lady, twenty-one years of age, had an itchy eruption on the face and hands for at least twenty years. When I saw her, these parts were swollen, tender looking, and readily exuded a watery lymph. The disease was generally aggravated after dinner, more especially if the dinner was tempting; at this period of the day she retired to her own room, conscious that her appearance was repulsive even to her nearest relations. It is easy to conceive the unhappiness to which this must have given rise; and though medical men, both in Glasgow and Edinburgh, were consulted at different periods, no plan of treatment was of any avail. After commencing the persesquinitrate of iron an amendment was soon perceptible; the swelling of the face and hands began to abate, the skin became less tender and less affected by meals, and in the course of some months a cure was gradually accomplished. General bodily uneasiness was of daily occurrence, and aggravated very considerably the depressing effect of the complaint; no symptom was more immediately under the control of the persesquinitrate, which always made her in a short time more comfortable. I believe that the cure of cutaneous diseases by this medicine is chiefly attributable to its efficacy in improving digestion.

For external ulceration I have used the persesquinitrate of iron internally in a few cases, and apparently with benefit. Mr Tindal writes me, that he gave it in a case of ulcer of the leg of eleven years' duration, and effected a cure as he believes owing to its use. If these observations are correct, they do not contradict the experience respecting the aggravation of diarrhœa where ulceration of the mucous membrane of the intestine exists; because in the one case the persesquinitrate does not touch the ulcer, in the other it does.

I have made some trials in scarlatina, with a view to prevent or mitigate ulceration of the fauces, but hitherto without any decided result.

In hiccup it is often a very potent remedy, and great benefit has been derived from it in tic douloureux.

Dr Neligan mentions, that Dr Montgomery has used it very extensively in mucous discharges from the vagina, and in such cases considers it the best of the ferruginous preparations.

My friend Dr Paton of Paisley, who has had great experience of this medicine for many years, tells me, that he has found it very

useful in profuse mercurial salivation, both as a lotion and taken internally.

Chronic diarrhœa, however, is the disease in which the persesquinitrate of iron has been longest and most extensively used. All my medical friends who have prescribed it, concur in saying, that in this it is very efficacious, and that many cases which had resisted all other treatment have been cured by it.

The testimonies now adduced from different quarters in support of the efficacy of the persesquinitrate of iron in curing chronic diarrhœa, ought to give this medicine a strong claim on the attention of practitioners as a preventive of cholera, should that dire scourge again return. Every person whose bowels are weak will then be in danger, and every such person ought immediately to place himself under treatment. All remedies which have yet been recommended for cholera have disappointed the expectations formed of them, and this circumstance ought to raise more highly the value of preventives. The contagion of cholera seems to differ from that of smallpox and scarlatina—in this respect, that these diseases seize individuals in perfect health who have not been subjected to any cause injurious to health besides contagion. Whereas, judging at least from my own observation, a person might live unharmed in a cholera hospital if his digestion be good, and his bowels firm; if he paid strict attention to avoid fatigue, to enjoy sufficient sleep; if he kept himself comfortably warm both by day and by night; and if he took care to avoid surfeits and intemperance. No instructions, no admonitions, will ever cause the unthinking part of the population to attend to their health, and when cholera arrives it will cut down its thousands among them. The thinking and virtuous portion will undoubtedly, from contiguity, be placed in danger, though, by strict attention to health, they may escape with comparative impunity. But individuals are placed in perilous circumstances if their digestion be easily deranged, and their bowels usually relaxed. According to the evidence already given, the ordinary remedies are in very many instances inadequate. It is here, therefore, that the persesquinitrate of iron is valuable by improving digestion and curing diarrhœa. Before the last invasion of cholera, I paid strict attention to the health of my different patients; and, wherever relaxation of the bowels existed, cases which are more numerous than would readily be suspected, the persesquinitrate of iron was prescribed. I found numerous instances of chronic diarrhœa of such lengthened continuance, that the patients, having frequently tested the inefficacy of the usual remedies, had long abandoned all hope of cure. The persesquinitrate of iron was generally successful, and I do not recollect of one of the individuals thus cured being afterwards seized with cholera. I trust that the importance of this subject is apparent. During the last epidemic, remedies for the disease were eagerly sought after; in the coming, unless some

brilliant discovery be made, I apprehend that more attention will be paid to preventives.

In making the solution of the persesquinitrate of iron, I now employ the following formula, which differs in a few respects from the original in the *Edinburgh Medical and Surgical Journal*.

Take of Iron Wire (that sold under the name of No. 17), one ounce.

Nitric Acid, three ounces by measure.

Water, fifty-seven ounces.

Muriatic Acid, one drachm.

Mix the nitric acid with fifteen ounces of water (in very warm weather the quantity of water may be somewhat greater, and in cold weather somewhat less) in an earthenware vessel capable of holding three or four times this quantity. Put into this dilute acid the iron wire broken into a number of pieces, and so twisted as to extend into every portion of the liquid. Cover the vessel lightly, and set it aside. In eight to twelve hours the process is completed, when the solution is to be poured off the undissolved wire, and the remainder of the water, together with the muriatic acid, added, to make up the whole to sixty ounces (thirty in the original formula).

In this process there must be a slight excess of wire (say thirty grains) to ensure the combination of the whole of the acid. A great excess, if allowed to remain long in the liquid, would convert it into the protonitrate. When properly prepared, the solution of the persesquinitrate of iron has a dark red colour, like that of dark brandy; and carbonate of soda produces a red precipitate, unmixed with any tinge of green. The taste is very astringent. The large quantity of water, and the free muriatic acid, are for the purpose of keeping the solution long transparent. In cold weather, two or three months will elapse before it becomes muddy.

The readers of Dr Graves' work will probably recollect that he attributes the introduction of the persesquinitrate of iron to Dr Christison; a mistake which he has promised to correct in the next edition of his work. Dr Christison writes me, "I do not know any notice prior to yours of 1832, of the employment of the persesquinitrate of iron for chronic diarrhœa. I have constantly mentioned the preparation in my lectures on materia medica, referring it to you as the first person to suggest it, and, I suppose, some Edinburgh student from Ireland must have mentioned my observations on it to Dr Graves, which will account for his having supposed that I suggested it myself."

HAMILTON, CANADA WEST.

ARTICLE IV.—*Case of Large Parotid Tumour extirpated.* By M. N. PARKER, M.D., Halifax, Nova Scotia.

HISTORY.—George Blakely, between ten and eleven years of age, residing at Ship Harbour, sixty miles from Halifax, Nova Scotia, was observed in the be-

ginning of May 1847 to have a swelling in the centre of his right cheek, about the size of a Windsor bean; no attention was paid it at the time, and it continued to increase until, early in July, it resembled in shape, and was as large as a man's thumb. In August he lost about a wine-glassful of blood, from what his mother calls "a blood blister," which she then, for the first time, noticed on the mucous membrane of the affected cheek. After this, the tumour increased very rapidly; so much so, that by the middle of September the right eye was nearly closed. To allay the swelling, the mother says she "poulticed it one night," and the following morning detected, for the first time, another enlargement near the articulation of the lower jaw.

She continued the poultices for a week, and then desisted, from observing the rapid growth of these morbid masses, which soon produced great disfigurement of the neck, and distortion of the features. Hitherto there had been no pain; but now, whilst perfectly exempt from it in the day, no sooner was his head on the pillow at night, than he suffered greatly, its chief seat being about the region of the parotid. The mother (a widow), seeing that further procrastination was out of the question, resolved to proceed to the capital for the purpose of having it removed.

Appearance at the time I was consulted, October 13.—The face of the right side was enlarged to a great extent by an oval tumour, commencing within a few lines of the angle of the mouth, which had been displaced to the first incisor tooth of the lower jaw, and extending obliquely backwards over the masseter muscle to the inferior maxillary articulation, where, superficially, it appeared to be separated by a vertical band of fibres from a large mass situated over the parotid. It reached from about one-fourth of an inch below the orbit to the lower margin of the inferior maxilla, apparently implicating the submaxillary gland. The mouth was partially opened, the tumour having extended inwards, like a wedge, between the two sets of teeth, which, during mastication, could not be brought into actual contact; and here its internal or mucous covering was abraded by the action of the teeth, and in its place was a ragged fungous-like growth, from which there was an occasional escape of a little venous blood.

It had no osseous attachments, moved freely when manipulated, and appeared more dense in texture than the other portions of the morbid growth. Although the pressure against the teeth was constant and considerable, yet he suffered not the slightest pain from it.

The second part of the mass commenced superiorly and anteriorly at the zygoma, and posteriorly at the upper part of the mastoid process, extending superficially over the entire insertion of the sterno-mastoid muscle, compressing by its centre the cartilaginous walls of the meatus auditorius externus, so as to bring them into close contact, and thus produce partial deafness. From this it extended downwards, resting upon the side of the larynx and trachea, to within an inch of the clavicle, following the course of, and dipping deep beneath the sterno-mastoid.

In the cervical portion there was also an apparent division of the mass into two, the inferior being the smallest; but, as in the facial, the division was only superficial, being caused by a dense band of transverse fibres constricting it in such a manner as to render it difficult to decide whether or not the lower portion was a distinct tumour. *The inferior* was more moveable than the superior; the latter appearing quite fixed, as if attached to the sheath of the parotid, and other deep-seated and important parts. The skin over the whole surface of the tumour, although tense, was healthy, unattached, and in no degree vascular. The entire mass was of a density between the fatty and fibrous tumour, partaking more of the characters of the former than the latter; and there could be no hesitation in pronouncing the whole as encysted. The case being of one of great interest, I invited the principal professional men of the city, with Surgeon M'Laren, H.M.S. Belleisle, and Assistant-Surgeon Wells, H.M.S. Vindictive, to examine it, who all agreed, that although the tumour was of a doubt-

ful character, the immediate use of the knife was essentially necessary, and that, unless speedily removed, death would be the result of its rapid growth, from pressure on the larynx and other important parts. The rapidity of its increase was such, after the beginning of October, that the lad's mother said, "she fancied she could see it growing." Although this was "forcing the imagination" to a considerable extent, still each day displayed, to myself and others, a visible increase in its dimensions, which caused me to name an early day for the attempt to save the life, or protract, for a time, the existence of a fellow being; therefore, after administering a purgative, its removal was undertaken on the morning of the 17th October.

OPERATION.—Ether, had it been effectual, would have greatly facilitated the operation. I had previously used it with the most happy results in the larger amputations, &c., but refrained from giving it in this case, partly from the inconvenience attending its frequent administration, which would have been required in a tedious operation like that under consideration; and partly from fear of the admission of blood into the trachea whilst under its influence, in which case the boy would probably have died under the knife. The patient was placed on his left side on a table, his limbs being encased in the folds of a sheet, the extremities of which were held by two assistants; his neck was then supported by a narrow pillow, and retained in position by another assistant, who held the head. Then, standing behind him, I commenced my first incision above the zygoma, a little anterior to the ear, and carried it downwards over the tumour, nearly to the sterno-clavicular articulation, dividing the skin and platysma myoides, and exposing the cyst containing the abnormal growth. In order to remove all obstructions to ligature of the carotid, in case that step should have been demanded of me, the *inferior lobe* was first dissected out, which part of the operation was protracted and tedious, from the constant struggling of the patient, who, in spite of all the efforts made to restrain him, was constantly forcing his shoulder upwards, much to my annoyance. This, with the increased flow of blood consequent on the muscular exertion, rendered the dissection very difficult. The cyst was firmly adherent to the external part of the sheath of the carotid artery for the distance of more than an inch; consequently it had to be removed for that extent, and with it the *descendens noni* nerve. This was done by forcibly elevating the tumour with the left hand, so as to raise the implicated part of the sheath, from the vessels beneath it, and then *scratching*, rather than cutting, with the edge of the knife turned carefully towards the under surface of the tumour. From this to the end of the operation, those vessels (carotid artery and jugular vein) were constantly exposed, and I now, for the first time in my life, had ocular demonstration of their *living actions*. The transverse band of fibres, before alluded to as separating the inferior from the superior lobe, having been previously divided, the dissection of the latter, or parotid portion, was now commenced. A transverse incision, beginning on the posterior aspect of the mastoid process, was carried close beneath the lobe of the ear, to the anterior margin of the masseter muscle. This incision, being afterwards extended onwards to the angle of the mouth, served for the extirpation of the facial portion.

The removal of the parotid tumour required a slow and careful dissection, as it extended deep behind the ramus of the jaw, to the periosteum of which it adhered by its cyst so firmly, that a small portion of it was removed, leaving the bone exposed. It was closely connected to the sheath of the parotid; so much so, that it was thought advisable, for safety's sake, to remove a considerable portion of that gland, during which, of course, the *pes anserinus* was, for the most part, destroyed.

I have before stated that the submaxillary gland was apparently implicated,—its removal was the next step. On reflecting the integument, a small fatty tumour was exposed, which, more deeply, was connected by its cyst with the sheath of the gland; the substance of the latter appearing healthy, its ex-

cision was by some considered unnecessary ; but there was a doubt, and consequently it was removed, and with it the small external tumour.

The transverse incision, already alluded to as terminating at the anterior margin of the masseter, was now continued anteriorly to within about three lines of the angle of the mouth, after which the integuments were dissected upwards nearly to the margin of the orbit, and downwards to the lower border of the body of the inferior maxilla. The facial portion of the tumour was then carefully dissected out, and the whole muscular, cellular, and mucous substance of the cheek removed, leaving the bones of the face covered only by periosteum, excepting that portion occupied by the masseter, which, not being implicated, was left entire.

The operation being now concluded, and the whole tumour, to all appearance, removed, the sponges which had been placed at the lower part of the wound, and behind the ramus and articulation of the jaw, to stop the venous hemorrhage, which had been profuse, were withdrawn ; and, there being no further escape of blood, the entire wound was cleansed, and the edges of the integuments accurately approximated, and held in position by sutures and adhesive plaster. The hemorrhage, principally venous, acting in conjunction with the direct shock to the nervous system from so extensive an operation, was sufficient to render a child of this tender age pulseless, and indeed, had it not been for the alcoholic stimulus, which was administered him in large doses, I doubt not but that the proceedings would have been concluded on a lifeless body.

The arterial hemorrhage was but trifling, only two arteries requiring ligatures from the beginning to the end of the operation.

This lobular and encysted mass was about 12 oz. in weight, of a greyish white colour, and more dense than fatty tumours usually are ; besides, it was not in cellular divisions, and appeared more homogeneous when exposed by section, than any growths of adipose composition that I had previously seen.

The facial and parotid portions decreased in density from the periphery to the centre, wherein both a soft and almost liquid mass was contained, having around it small blood red vascular points, about as large as pins' heads. In short, it was an *adipose tumour* in which incipient degeneration marked the progress to future malignancy.

At two o'clock P.M., I left the patient comfortably placed in bed, on his left side, with an inclination to sleep ; pulse 140, and feeble. At 4 P.M.—Visited him again ; pulse more full, 135 ; warmth of surface, which, during the latter part of the operation was quite cold, had returned. At 7 P.M.—Much the same as at last visit. At 11 P.M.—Pulse 140 ; skin hot ; was sleeping calmly ; had taken a little thin gruel through the spout of a tin teapot.

18th.—Had slept comfortably for several hours ; pulse 130, and feeble ; skin hot ; his bowels not having been moved through the day, in the evening a saline purgative was ordered, which, not acting, an enema was given with the desired effect.

19th.—He had slept well ; pulse 130, and more full than yesterday ; skin cooler. *Evening*.—Pulse as in the morning ; skin hot, has a short dry cough ; the tongue cannot be examined, but he complains of its being sore ; bowels have been moved ; gave him small doses of tartarized antimony, with syrup of squills, and ordered the body to be sponged with tepid water.

20th.—Pulse 125, temperature of surface decreased ; cough still continues, but is less troublesome ; some discharge of bad odour from the wound ; dressed it in part, and found that adhesion, by the first intention, had taken place over nearly the whole cheek, and even as far as the mastoid process (in the line of the transverse incision), also in front of the ear for about an inch and a half (the superior part of the vertical incision) ; below this, from the motion produced whilst coughing, some of the sutures had cut their way through the integuments, which, with others, were removed.

Continued antim. tart. and tepid sponging of surface, also the cold water

dressing to the wound (this had been used from the first); complains of bad taste in mouth; syringed it with warm water; diet still gruel.

21st.—Pulse 120; skin comfortable; coughs less; complains of hunger; ordered him beef tea, which he *sucks* through his teeth from the spout of a tin vessel, not being able as yet to separate his jaws; syringed mouth.

22d.—Symptoms much as yesterday; speaks more distinctly; syringed mouth, and dressed wound, the inferior part of which is discharging healthy pus, and granulating well; removed remaining sutures; gave him permission to change his position somewhat, he having, prior to this, continued on his left side, in order that the liquid food and saliva might not interfere with the healing of the right cheek.

27th.—The patient has gone on gradually improving since last report; the wound looks well, and is granulating nicely along the inner margin of the sterno-mastoid; dressed it. Has no bad taste in mouth; can open it now far enough to take in soaked bread. His tongue looks clean; cannot as yet examine the interior of the mouth satisfactorily. The bowels act spontaneously and with the natural frequency. Pulse 112; skin comfortably cool. Has had no pain since the operation, and has slept well without the aid of opiates; coughs but seldom.

November 8.—The lad has been doing well since last report. The wound has altogether healed, with the exception of a small fistulous opening, close to the lobe of the ear, and over the parotid, through which the saliva, secreted by the remaining portion of the gland, constantly trickles; this is absorbed by a piece of sponge, which is kept in contact with the orifice. The mouth can now be opened sufficiently to examine the interior, which looks and feels much the same on the affected as on the healthy side. Indeed, it would be difficult to distinguish by sight and touch between the original and the recent formation. The expression of countenance is peculiar. There is a want of sensibility in the integument of the left cheek, which has a contracted appearance, and rests against the teeth, producing what is often termed "a hollow cheek." The angle of the mouth is drawn towards the opposite (right) side, for want of the destroyed muscles. Notwithstanding these deformities, there is a wonderful improvement in his appearance. The incision across the face has left but a very slight mark, and one that would not be noticed at any distance. Pulse 112, and weak; appetite not good; can walk about the room, but complains of muscular debility. The bowels act regularly. There is no pain, and he rests well at night. The mother, being anxious to return to her family, and the case having thus far recovered, I consented to his removal, and he consequently left the city the following day (ninth).

Iodide of potassium was prescribed, and its use continued for some time after his return to Ship Harbour. This, together with tonics, and occasional aperients, constituted the after treatment of the case.

Remarks.—The foregoing case presents many features of interest; but, having already entered somewhat minutely into its details, the concluding observations will necessarily be brief. The first question suggesting itself to the reader, will doubtless be—Was this a case for operation? It has previously been stated, and from its history must be evident, that the tumour was one of a very doubtful character. The pain (a prominent symptom of malignant growths), which was altogether absent when in the erect, or semi-erect position, and present only at night, when reclining horizontally, might be accounted for on mechanical principles; viz. as the result of pressure (arising from increased tension of the integument and neighbouring muscles). Independent of this, however, the *pulpy* feeling, short

duration, and rapid growth of the tumour, together with the comparatively fixed state of the parotid portion (in itself an unfortunate complication), were symptoms opposed to an operation. On the other hand, the boy's general health appeared uninjured, his appetite was good, tongue, pulse, and bowels natural. These indications, together with the unimpaired state of the integuments, and the somewhat defined and encysted character of the abnormal growth, led me to *hope*, that the removal of the entire mass might possibly be the means of effecting a cure. However, I conceived that the case demanded immediate surgical intervention on other grounds; for, situated as the tumour was, and increasing with such rapidity, death must speedily have resulted from pressure on the larynx. The operation was, therefore, performed as much with the intention of protracting life as with the hope of curing the disease.

I have already described, in few words, the appearance presented by the excised mass, in which a degenerate action had commenced its baneful attack; but whether the system was materially affected remains yet to be proved. The most recent accounts from the lad are to the effect,—“that the fistulous opening still continues to discharge freely; his usual strength has not been regained, but he is able to walk about; pulse frequent, and weak.” Taking into consideration these symptoms, with the pathological appearance of the diseased growth, I am inclined to believe that the result will in the end be unfavourable, and the operation protractive, not curative.¹

This morbid enlargement cannot be traced to external injury, and it would appear that it was in no degree connected with hereditary predisposition. Similar cases at such an early age, are, I presume, extremely rare.

The small amount of arterial hemorrhage during the operation, may be attributed to the manner of conducting it in the neighbourhood of vascular parts, where as much was done by the fingers and handle of the knife as with its cutting surface.

So profuse was the venous hemorrhage at one time, that it was feared the internal jugular vein, which lay exposed, enveloping, as it were, the carotid artery, had been opened whilst the patient was struggling violently; however, such was not the case, but it required the utmost caution to avoid it, until, from loss of blood, he became so weak, that his efforts could be, for the most part, restrained by the assistants, after which the dissection progressed with more rapidity and safety.

The formation of a membrane, lining the cheek, analagous in feeling and appearance to the mucous, was effected in a very brief period, and forcibly displays the restorative powers of nature, in constructing a copy to supply the place of an original structure, when art or disease shall have removed or impaired it.

¹ This opinion was well founded. We have just received a letter to inform us that the disease reappeared, and that the case terminated fatally.—ED.

Had the patient continued under my charge long enough, I had intended to have formed a *duct* to the parotid, for the purpose of conveying *its* secretion into the mouth, by which means the external fistulous opening over that gland could have been closed.

ARTICLE V.—*Contributions to the Pathology of the Kidney.* By
WILLIAM T. GAIRDNER, M.D.

PART I.—ON THE PATHOLOGICAL ANATOMY OF THE KIDNEY.

IN the following observations, I have endeavoured to keep constantly in view the connexion between the pathological elements and the normal structures; and also to reconcile, in as far as possible, the microscopic appearances with those visible by the unaided eye. In describing the latter, I have availed myself extensively of the works of Bright and Rayer; and the reader, to whom those works are accessible, will find in the present memoir numerous references to their excellent plates, which will serve to prevent misapprehension as to the appearances referred to.

I.—EXUDATION.

The exudations from the blood-vessels of the kidney, being at once the simplest and the most common of its structural changes, and being connected, either as cause or effect, with most of the other more complex conditions, require to be considered first of all.

Exudations into the substance of the kidney give rise to a great variety of external appearances. These have of late years, especially since the observations of Dr Bright upon their connexion with some of the most severe and fatal diseases, been objects of much interest to the pathologist; and have been represented and described in the works of Bright and Rayer, with a completeness and accuracy which has scarcely a parallel in the anatomy of any other organ in the body. Nevertheless, the cultivators of a more minute pathological anatomy, have found the lesions of the kidney involved in greater mystery and confusion than those of any other organ whose structure and functions are equally well understood; and although some of the best modern observers have endeavoured to bridge over the chasm between our physiological and pathological knowledge, much remains still to be done.

Exudation may take place from the blood-vessels into all the tissues of the kidney. Its most common seat is the interior of the tubes; but it also occurs frequently within and around the Malpighian bodies, and in the inter-tubular tissue, the tubes being quite clear. I have also seen it infiltrated through all the tissues in the form of a homogeneous mass, which contained within it the whole of the anatomical elements of the kidney.

1. *Exudation within the Tubes.*—The process of secretion in the kidney being in fact a normal process of exudation from the blood-vessels into the tubes, and one which, from its extreme complexity, is liable to very frequent derangement, it is not surprising that, of all the lesions of the kidney, the presence of foreign matters in the tubes should be the most frequent. The greater part of such exudations, however, are either soluble in the urine, or readily carried away by it, and only come under the notice of the pathological inquirer in connexion with the alterations in that fluid. Of this kind are the albumen in Bright's disease, and the sugar in diabetes, besides a number of soluble substances, which, although connected with morbid processes in the kidney or elsewhere, find a ready exit from the system, and do not remain to block up the tissue of the organ.

When, from any cause, the secreting cells of the kidney receive from the blood substances which do not remain in solution, obstruction of the tubes very readily takes place; much more readily indeed than in most other glands, owing to the remarkably tortuous course and narrow caliber of these tubes, and their complete occupation by the normal epithelium. If the abnormal character of the secretion continues, the obstruction is progressively increased; while, at the same time, there accumulates within the obstructed tubes a quantity of insoluble exudation, which modifies the appearance of the gland, and interferes materially with its function.

The appearance of the kidney, as altered by the presence of exudation in the tubes, is subject to variations depending on the amount of engorgement, and its partial or general character. I have already stated (April No. p. 718), that the almost invariable effect of the repletion of the tubes throughout the kidney, is a corresponding diminution in the fulness of the vessels of the cortical substance; particularly of the Malpighian vessels and the capillaries surrounding the tubes. Indeed, when the accumulation is considerable, the Malpighian vessels are rarely to be traced with the naked eye, as they generally are in a healthy kidney. I have shown above (*loc. cit.*) the anatomical reason of this phenomenon.¹

¹ I take this opportunity of stating, that on this point I am compelled to differ decidedly from the views of Dr Johnson of London. It appears to me, that he has fallen into an error both of observation and theory, in ascribing the albuminous urine of Bright's disease to secondary congestion or rupture of the Malpighian bodies, caused by the distension of the tubes from accumulated fat. Not to insist further on the anatomical argument, his view is opposed by every one of Bright's own plates, which show the cortical substance uniformly pale and bloodless, with the exception of Plate V., which would probably not be admitted by Dr Johnson to be a case of Bright's disease at all, inasmuch as it presents none of the appearances of a fatty kidney. In Rayer's work, also, every plate representing the section of a granular kidney (See Plate VIII. Figs. 3 and 5—Plate IX. Fig. 8) presents a pale uninjected cortical substance; and this concurs with the descriptions of Rayer, Bright, Rokitansky, Christison, &c. It will be seen hereafter, that my views of the relation of albuminous urine to the fatty and other degenerations of the kidney, are different from the above.

However characteristic of morbid exudations into the tubes, this paleness of the kidney on section is by no means peculiar to such diseased states. It sometimes occurs as a consequence of general anemia, and much more frequently in organs turgid from retained secretion, arising from accidental causes. Such kidneys are also frequently very soft and easily torn, often œdematous, and present a remarkable similarity in their general appearance to some of the earlier stages of exudations. In these cases the microscope is of the greatest service in enabling us to form a positive opinion; and I have so frequently been enabled to correct my own first impressions, as well as those of others, by this means, that I have ceased to repose confidence in the judgment of the unaided eye on kidneys of this description.

The consistence of kidneys containing exudation in the tubes is very various, depending chiefly on the amount and character of the morbid deposit. The colour also varies considerably according to the kind of exudation. When this is very white and opaque, it presents itself in marked contrast to the intervening tissues, giving to the kidney on section a minutely and irregularly speckled appearance, which extends through the cortical substance, and sometimes affects also the tubular cones. It is also seen very distinctly in the intervals of the superficial venous polygons, when these have not been obliterated by pressure. This form is admirably seen in some cases where the exudation consists of salts deposited from the urine. On the other hand, when the exudation approaches nearly in colour to the kidney itself, it is frequently distinguished with great difficulty, the organ presenting a uniform paleness, without any further apparent change.

The volume and weight of kidneys containing exudation in the tubes, are frequently much increased; but this circumstance is so much under the influence of accidentally coexisting diseased conditions, that I prefer to leave it to be treated of afterwards in a separate section.

The above remarks indicate the appearances produced by exudation uniformly diffused through the tubes of the kidney; but this, though common in the slighter forms of the affection, seldom persists when the abnormal deposit has become such as to crowd any portion of the organ. It then tends to accumulate in certain sets of the convolutions in which the urinary current is least active. These, becoming partially blocked up, and ceasing entirely to secrete, are thrown aside from the general outward current of secretion, and become a centre of attraction for further deposit, just as the eddies and still water at the sides of a rapid stream receive from it the foam and floating bodies brought down from above. In this way more and more of the adjacent loops of tubuli are filled with the abnormal deposit, and become added to the former nucleus, until the masses of exudation, thus imprisoned within tubules through which no secretion passes, form irregularly rounded bodies in the cortical substance, visible to the naked eye, more or less prominent

on the surface of the organ, and usually of an opaque yellowish colour. These are the granulations first described by Dr Bright, and figured in his 1st and 3d plates, and in Rayer's work (Plate VIII. Figs. 1, 2, 5, 6, and Plate IX. Figs. 1, 5, 8). The admirable descriptions of these bodies by the last-mentioned pathologist are now well known in this country, and supersede the necessity of further detail in this place.

In 1842, Mr Goodsir described the granulations of Bright as formed by the accumulation of secreted matter within the tubes. In Germany, Gluge,¹ Hecht,² Eichholtz,³ and other observers, have given descriptions of the structural relations and composition of the granulations, which, though differing in detail, concur in representing them as formed within the tubes; and in the excellent work of Lebert on microscopic pathology, a description of them will be found, which, although short, is not surpassed by any of the others in accuracy. In this country several recent observers have taken up the same view. In a paper by Mr Toynbee in the *Medico-Chirurgical Transactions*, Vol. XXIX., there are excellent plates of the anastomoses and convolutions of the tubes, and the granulations in Bright's disease, which afford valuable aid in the understanding of this subject.

The peculiar seat of the renal granulations is the cortical substance; the flow of urine through the pyramids being too constant to permit of the accumulation there of exudation in large quantity. The tendency to form granulations is generally first displayed in the neighbourhood of the surface, and also in the deep-lying convolutions between the pyramids; in both of which situations the tubes are remote from their orifices, and the pressure from behind is consequently small.

It may easily be understood, that the tubes involved in a granulation are in general permanently lost to the kidney as secreting structures; for, having ceased to perform their function, and the stream of secretion having been diverted into new channels, the re-establishment of the former ones is in the majority of cases impossible, and the useless granulations become absorbed and obliterated. The mode in which this occurs will be hereafter described.

The special characters of intra-tubular exudations next fall to be considered. Excluding tubercular and cancerous deposits, which are rare, and in regard to which I have no new observations to offer, these may be considered under three heads, viz. *a.* Crystalline or saline matters deposited from the urine; *b.* Oleo-albuminous or granular exudations from the blood-plasma; *c.* Exudations forming pus.

a. Exudations consisting of Crystalline or Saline matters deposited

¹ Atlas der Pathologischen Anatomie. ² Op. cit. ³ Müller's Archiv. 1845.

from the Urine after secretion.—I have already alluded to the fact, that the urate of ammonia, which so frequently occurs as a sediment in urine out of the body, is no less frequently deposited from the urine contained within the tubes of the kidney. This occurs in most cases simply as a *post-mortem* appearance, consequent upon the cooling of the body; and, when it is small in amount, it is only appreciable by the microscope. Occasionally, however, it is found in such quantity as to present to the naked eye the appearance of a distinct deposit. In such cases the cortical substance, which, when otherwise healthy, generally retains its normal vascularity, appears occupied by a white or yellowish-white opaque deposit, which presents itself also in a very marked form between the vascular striæ of the pyramids, particularly in the half nearest the cortical substance, where the vessels are more abundant than towards the apex. Such a deposit, which in reality is consistent with a perfectly healthy state of the organ, might easily be mistaken for a diseased condition. The following observation will illustrate this.

OBSERVATION I.—A man, æt. thirty-five, was admitted into the Royal Infirmary, March 9th, labouring under symptoms of concussion. He had fallen from a window while in a state of intoxication, and had fractured his left tibia. There was a severe lacerated wound of the scalp. He died next day.

The body was examined March 13th. It was that of a tall and unusually robust man, perfectly well formed, and presented every appearance of perfect health. Every organ in the head, thorax, and abdomen, presented its usual appearance, except the kidneys. These were of normal size, the capsule was easily stripped, the vascular arrangement perfectly normal; but in every part of the cortical substance were seen irregular opaque white specks, contrasting strongly with the vascular redness around, and giving the organ a minutely mottled appearance. This white deposit penetrated between the striæ of the pyramidal substance. I was led to suspect its true nature by the normal character of the vascularity, and also by its penetrating so freely into the converging ducts, which are comparatively rarely the seat of other deposits. On examination by the microscope, the tubes were seen clouded and obscured by a molecular deposit, which was likewise scattered over the field, but which was completely removed by a drop of dilute acetic acid. The structure of the kidney was perfectly normal.

The distinguishing character of this deposit is its ready solubility in dilute acids, such as the acetic or nitric. Under the microscope it presents the appearance, when within the tubes, of a fine molecular shading, which entirely obscures the nuclei. That part of it which floats free on the field of the microscope, may be observed to be composed of fine molecules and granules, which, when large enough to have a defined edge, may be observed to be amorphous or angular, sometimes approaching the circular form, but never accurately rounded. Sometimes these granules cohere together in the form of opaque masses, dark by transmitted light, and of irregular form. The addition of a drop of acetic acid produces instantaneous clearness, unless the deposit be very abundant, in which case more must be added.

The following case, in which a deposit similar to the above took place to a much greater extent, and produced a distinct morbid con-

dition of the kidney, presents many features of interest, and is one of by no means frequent occurrence. It is the only case of the kind which I have had an opportunity of observing.

OBSERVATION II.—*White Deposit in Tubes of Kidney—Cavities in Right Kidney filled with Deposit—Dysentery.*—Alexander Crichton, æt. seven, admitted into the Royal Infirmary, March 1, 1848, under the care of Dr Andrew.¹ He was excessively emaciated, and had been for ten weeks affected with constant purging and pain of the abdomen. He had been in a state of great destitution. He died March 3d, two days after admission.

The thoracic organs were healthy. The mucous membrane of the colon was thickened and ulcerated, and there were patches of lymph on the surface. At the lower end the caliber was much diminished, so as barely to admit a finger. The kidneys were much enlarged; the right weighed $4\frac{1}{2}$ oz., the left $6\frac{1}{2}$ oz.; surface smooth; venous injection unequal. In the left kidney cortical substance voluminous (four lines broad), mostly developed between the pyramids, protruding towards the pelvis, and closely packing in the apices of the cones. Cortical substance infiltrated throughout with small white opaque granules. On the surface these were also visible in the intervals of the venous polygons, but without disturbing the smoothness of the surface. Tubular substance compressed; but in some parts the white infiltration was seen at the base of the pyramids, at others extending nearly to the mammella. The right kidney was broken up superiorly into several anfractuous cavities, from the size of a hazelnut to that of a walnut; these cavities were filled with a diffuent white substance, which had much of the appearance of softened brain. The cavities were lined by a false membrane, which contained numerous gritty particles, and varied from two to four lines in thickness. It appeared to be composed of the condensed tissue of the gland, lined by a layer of concrete matter from the deposit contained within the cavities. In other respects, the right kidney presented the same appearance as the left. Both kidneys were tolerably firm in texture; the venous network of the surface was well injected, and the veins of the pyramids were in some parts full of blood. The cortical substance contained little blood; the vascular striæ and the Malpighian bodies were obscure.

Microscopic Examination.—The tubes were seen to be completely filled with an opaque matter, which obscured the nuclei within. Diffused in water, this matter was observed to be composed of molecules and amorphous granules (Figs. 3, 4), and of a nebulous obscuration, which under a power of 350 diameters was not wholly resolved into distinct parts. The cells, many of which were well formed and entire, were filled with a similar obscuration, which prevented the nuclei from being distinctly visible—(Fig. 4). The white opaque fluid from the cavities was composed of similar granules and molecules, in addition to which were seen some dark opaque amorphous bodies, evidently composed of aggregated granules and molecules—(Fig. 3); and in the midst of the deposit there were numerous nuclei, which were not surrounded by a cell-wall. The whole of this amorphous deposit, and all the molecules, were dissolved on adding a drop or two of dilute acetic or nitric acid; the tubes, with their cells and nuclei, then became clearly visible; nevertheless, after the addition of the acid, some of them were found to contain a few fatty granules and globules, some of which were also scattered over the field. The Malpighian bodies were mostly destitute of blood; a few were slightly injected.

¹ I take this opportunity of expressing my thanks to the physicians of the Royal Infirmary, who have kindly permitted me to make use of every source of information as to the cases under their charge. It is right, however, to state, that I am alone responsible for the selection of facts in relation to the histories of disease, and for the whole account of the pathological investigations.

Fig. 3.

Fig. 4.

Fig 5.

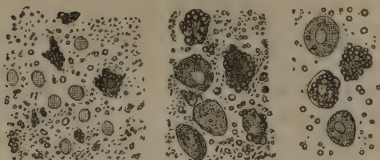


Fig. 3. Some of the white semi-fluid matter from the cavity in the kidney. It is seen to be composed of molecules and granules, interspersed with free nuclei, the debris of the epithelium cells. Some of the granules are aggregated into masses of irregular form. The perfectly spherical granules are composed of fatty exudation, which is present in limited quantity. The nuclei are slightly and uniformly shaded in their interior. (250 diameters.)

Fig. 4. Fluid scraped from the surface of the cortical substance in the same kidney. It differs from the last in containing entire epithelium cells, which are obscured and filled with granules and amorphous exudation. Part of this, as in Fig. 1, is composed of perfectly spherical fatty granules.

Fig. 5. The same. The urate of ammonia has been removed by acetic acid, leaving only the fatty granules and epithelium cells.

In a case in the *Medico-Chirurgical Transactions of London*, Vol. XXIX. p. 272, in which one kidney was deficient and another very extensively diseased, Mr Busk describes a deposit in the tubes of "a semi-opaque white granular material, soluble or rendered transparent by acetic acid, and presenting none of the characters of oil." In this respect, and also in the small opaque specks and white striæ described as existing in the cortical and pyramidal substances of the kidney, the case has many points of resemblance to those which I have given above; and, in as far as appears from the description, I should incline to consider this a deposit of the same kind, occurring in a kidney otherwise diseased and atrophied. Mr Busk seems to have considered it albuminous in its nature; but albuminous deposits, when amorphous or granular, are not generally found to present the ready solubility in acetic acid which is described in this case, and is always found with urate of ammonia.

Although the deposit of urate of ammonia in the tubes is of little or no pathological importance in the majority of cases, yet it occurs so frequently, and presents an appearance so much like other deposits to the unaided eye, and in some cases readily mistaken even in a microscopic examination, that I am satisfied it must occasionally have been a source of erroneous impressions to pathologists. At least I am conscious, in my own case, that I must have been frequently misled, before I was aware of the importance of applying the test of acetic acid to every deposit occurring in the kidney. This test is particularly necessary, when, as not unfrequently happens, the urate of ammonia deposit is mixed with a certain quantity of fatty granules; in which case, an idea of the relative amount of the two forms of exudation can only be obtained by the removal of one of them in the way described. (See Figs. 4, 5.)

Crystalline deposits within the tubes are of much greater rarity than the amorphous urate of ammonia. I have repeatedly seen in

the tubes of diseased kidneys, small groups of perfectly circular bodies, with a clear distinct edge, of a yellowish colour, and varying in size from 1-200th to 1-80th of a millimetre. As they did not present the peculiar glistening refraction which distinguishes fatty granules and globules, I was disposed to consider them as crystals, more especially as they were very similar to those described by Dr Golding Bird (*Urinary Deposits*, Fig. 8, p. 72) as crystalline urate of ammonia. I have lately seen reason to alter this opinion, having found them to resist the action of acetic acid, and to present characters by no means compatible with the supposition of their crystalline nature. I have now ascertained, from observing their generation in urine, that they are in all probability formed out of the body as a product of decomposition; but as my observations have not yet led to any precise knowledge of the mode or circumstances of their development, I shall for the present do no more than record their occurrence.

A less questionable form of crystalline deposit has been observed by Gluge in the kidney of a dog, whose bladder contained a sanguinolent urine. The kidney presented marked capillary injection, and was considered by Gluge as being inflamed. The tubes were in some places crowded with semi-transparent crystals of very irregular form (see *Atlas d. Path. Anat. Lieferung*, 10. P. II. Figs. 5-7.) Gluge has not stated the probable nature of these crystals.

In the following case I had an opportunity of observing a deposit of crystals very like those mentioned by Gluge; and, from their appearance and colour, I have little doubt that they were uric acid, although from their minute quantity they could not be submitted to chemical examination.

OBSERVATION III. *Nephritis? Pneumonia. Crystalline Deposit in the Tubes of the Kidney.*—Edward Graham, æt. twenty, labourer. Admitted into the Royal Infirmary, February 29th, under the care of Dr Douglas, on account of epigastric pain and tenderness, nausea and vomiting, accompanied by obstinate constipation, scanty high-coloured urine, and a peculiar typhoid oppression of aspect, less marked on admission than it became afterwards. Two months before admission he had a gonorrhœal discharge, for which he appeared to have taken mercury; the gums and mucous membrane of the mouth were extensively ulcerated. Three days after admission the urine was examined, and found to contain a considerable quantity of albumen, with blood corpuscles; its density was 1.017. The urine continued scanty (10-15 oz. daily), and had to be drawn off by the catheter, on account of its retention in the bladder; the typhoid depression increased; and the day before his death the physical signs of extensive pulmonary affection, without any marked symptoms, were observed. The pulse gradually became weaker, and he died on the 9th March.

On dissection, the heart and great vessels were loaded with very dark blood. Both lungs were much engorged, and a considerable portion of the left lung had passed into the state of red hepatization. The kidneys weighed 7 oz. each, and were of large size, the increase being chiefly apparent in the cortical substance, which was of a much deeper colour than natural. The radiated vascular striæ of the cortical substance were also much injected, and the points

indicating the Malpighian bodies were turgid and dark-coloured. The surface showed the venous network in a state of congestion, but no other change.

Microscopic Examination.—On making a section of the cortical substance with Valentin's double-bladed knife, the gorged capillaries were seen surrounding the tubes, and apparently completely filling up the inter-tubular spaces. The Malpighian bodies were large, and injected throughout, instead of merely at their edges, as is commonly the case. In one or two places the tubes appeared full of blood; but this was not at all general. Here and there were scattered among the sections of the tubes well-defined angular bodies of different sizes, from 1-100th to 1-30th of a millimetre. Their shape was by no means regular, being sometimes imperfectly rounded, at others distinctly angular, and when so, inclining to the rhomboidal form. When isolated, their thickness appeared to be considerable, and their colour was a moderately deep amber yellow. The larger ones presented the appearance of cracks or fissures in their interior, the cleavage being generally more or less accurately parallel with the sides. On adding acetic acid, which did not affect in any way the bodies above described, many of the tubes were seen isolated, their nuclei plainly visible, and their cells free from deposit; the crystalline bodies could be seen in a few; and one, which is figured below (Fig. 6), was completely crowded by them. Comparatively few, however, were in this condition; the greater number being apparently healthy.

Fig. 6.

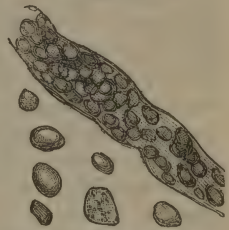


Fig 6. Irregularly crystalline deposit, probably of uric acid, in a tube from kidney of Graham. Some of the crystals are seen loose. (250 diameters.)

In this case we have partial suppression of the urinary secretion, which was of low specific gravity, and albuminous (partly, no doubt, from blood): in connexion with which circumstances there were symptoms of gastric irritation, such as frequently accompany disorders of the kidney, and a marked typhoid depression and stupor. It seems every way probable that this last condition was owing to the retention and accumulation of urea in the blood. These circumstances entitle us to look to the kidney as having a share in the disease, notwithstanding the absence of pain on pressure in the lumbar region, which is indeed a circumstance of little moment, when we consider that all uneasy sensation seemed to be masked, owing to the typhoid condition. Notwithstanding these functional alterations, however, there is no apparent exudation within the substance of the gland, except this minute sabulous matter in the tubes. May we not regard this last as one of the exciting causes of the disease, taken in connexion with the urethral irritation, and the state of the system induced by the action of mercury, to which he appears to have been subjected before admission?

b. Oleo-Albuminous Exudations from the Blood-plasma.—I employ this term as including, in one extended series, the whole of those exudations recognised both by German and English pathologists as fatty in their nature (the fatty granules, globules, and corpuscles of authors), together with many of those which have been distinguished as more properly inflammatory, such as the inflammation globules, granular corpuscles, or exudation granules and corpuscles of different writers. The necessity of a classification founded on the element-

any structure and chemical composition, rather than on the accidental structural varieties of such exudations, is every day becoming more apparent. On the one hand, the use of the term "fatty" by pathological anatomists, though in itself most descriptive and apposite, has frequently been the means of vitiating their conclusions, and even their descriptions, when employed, as it has very frequently been, with the preconceived view that it is applicable only to chronic changes. On the other hand, the observations contained in the present memoir will be found to add new links to the chain of evidence which has been accumulating for several years past, that the so-called "inflammation" or "exudation" corpuscles, masses, and granules, are by no means characteristic of acute inflammatory processes. This conviction, which from the first induced many of the most eminent and well-informed pathological writers to withdraw the original term "inflammation globules" employed by Gluge, and to substitute for it those of granular cells, or exudation corpuscles (see the works of Henle, Vogel, and Hughes Bennett), has received a most complete confirmation from the valuable researches, lately published by Reinhardt,¹ on the nature of the granular corpuscle; in which it is proved by numerous and well-founded observations, that the corpuscle in question arises, in many instances, from the deposition of granules, consisting of fatty and protein elements, in the natural epithelium cells of different organs; and that its origin is not only in some cases independent of the inflammatory process, but frequently a purely physiological change, as in the *membrana granulosa* of the Graafian vesicle of the ovary. The occurrence of the granular corpuscles in almost every species of pathological product, is mentioned in the systematic works of Lebert and Vogel; and the readers of the *Monthly Journal* need scarcely be referred to the observations of Dr Bennett on cancerous structures, for numerous proofs of their formation in connexion with this form of chronic disease.

The fatty nature of the granules occurring in inflammatory products is known to most histologists, and is easily proved by observing the reaction of ether. Vogel² states, that they are composed partly of fat and in part of protein, and carbonate or phosphate of lime (the mineral elements are, however, very variable in amount). The relations of the oil to the albumen or protein, in these and other structures, has been minutely studied by Ascherson and Hughes Bennett,³ who have shown that an albuminous membrane surrounds the oil granules, and prevents them from coalescing, as they would otherwise necessarily do; and that the formation of emul-

¹ Archiv. für Phys. Pathologie, by Virchow and Reinhardt, No. I. 1847; analyzed in *Monthly Journal*, February 1848,—Retrospect, p. 6.

² Path. Anatomy, Dr Day's translation, p. 157.

³ See the paper of the latter "On the Structural Relation of Oil and Albumen;" in the *Monthly Journal* for September 1847.

sions, where oil exists in a minute state of division, depends on a similar arrangement.

I have myself had repeated opportunities of observing the formation of granular corpuscles in diseased structures. In pneumonic exudation, in which they can be traced very frequently from their earliest stages, I have invariably found them to be formed from epithelial cells, according to the law laid down by Reinhardt. I have likewise satisfied myself, by repeated observations, that in some of the forms of so-called pneumonia, the quantity and size of the fat globules and granules is such as to constitute a true fatty degeneration of the lung, in the same sense in which the term has hitherto been more familiarly applied to the corresponding lesions of the liver and kidney; and that these lesions present no structural difference from the more ordinary forms, except the greater number and the larger size of the globules which accumulate in the tissue. Finally, in the case of the lung these views have been fully borne out by chemical analysis. In a series of researches by Guillot (*Gazette Médicale*, No. XXIX. 1847), it is shown, that in all diseases of the lung giving rise to obstruction of its tissue by exudation, there is an increase in the relative quantity of fatty matter, which, in the adult healthy organ, is about six per cent., but in the diseased states (such as pneumonic or tubercular infiltration) rises frequently to fifteen, and sometimes to fifty per cent. No similar analysis has yet, so far as I know, been applied to the kidney; but considerations, deduced from histological observations, give the strongest reason to suppose that in this respect, as in others, an analogy would be found between the pathological conditions of the two organs.¹

I have entered thus far into the general pathological anatomy of this form of exudation, with the view of reconciling my own observations with those of previous writers on this subject; as also to explain my adoption of a classification which annihilates, or, at least, very much modifies, distinctions which many conceive to have a pathological significance corresponding to their practical importance. However satisfactory it might be to point out the inflammatory and non-inflammatory lesions of the kidney as presenting strongly marked pathological distinctions, I am satisfied that such an attempt would fail, from not being founded in nature or truth; and I am confirmed in this view, by the free admission, on the part of the most skilful pathological anatomists, of the extreme difficulty of making the distinction in question in the case of the kidney. Indeed, in considering the terms inflammatory and non-inflammatory as being more applicable to the modes of invasion of diseases of the kidney than to differences in their pathological anatomy, I am only following out the ideas of Rayer, who included most of them under one patho-

¹ Rokitsansky enumerates inflammatory exudations as among the circumstances under which fat is deposited pathologically.—*Handbuch d. Path. Anat.*, and *British and Foreign Med. Chir. Review*, Jan. 7, 1848, p. 287.

logical name (Nephrite), and then distinguished them into acute and chronic.

The application of these principles to the explanation of various well-known diseases of the kidney, will be treated of in the sequel (Part II).

Oleo-albuminous exudations are distinguished by their being partially soluble in ether, which leaves an amorphous residue insoluble in cold mineral acids. The amount of this residue relatively to the whole mass, differs much in different cases, and indicates the relative amount of the protein element; it is sometimes in minute quantity, but is never entirely absent. The form assumed by such exudations is that of granules or globules, which are perfectly spherical, and present a dark distinct edge. These spherical bodies vary in size, being sometimes exceedingly minute, at other times as large as 1-60th of a millimetre, or even much larger; the variation is generally considerable in a single portion submitted to examination. Owing to their powerful refraction of light, they present a brilliant white centre and a dark circular rim, which is darkest towards the external edge.

The composition of these granules and globules has been already adverted to. The oil which forms their central portion is probably derived from the serolin, and the protein envelope from the fibrin or albumen of the blood-plasma. When the albuminous element is in large quantity relatively to the oil, the granules found are small, verging into the minutely molecular appearance; when the reverse of this is the case, they occur mostly in the form of large globules, of which the investing membrane is thin and readily ruptured by pressure. The prolonged action of acetic acid also frequently dissolves the membrane, and allows the contained fatty particles to coalesce. Caustic potass dissolves, after a time, both the albuminous and the oily element.

The mode in which the fatty granules or globules are disposed within the tubes of the kidney, next demands attention. The existence of fat in this situation was first distinctly recognised and described by Gluge as a diseased condition of the kidney, to which he gave the name of cirrhosis¹ or steatorosis, with the view of distinguishing it from the states which he has described as inflammatory. In Germany, although many authors have written upon this subject, none appears to have in any way added to Gluge's later researches in his *Atlas der Pathologischen Anatomie*. In this country, the memoir of Dr Johnson of London is the only one, I believe, yet published on this subject.

¹ Gluge uses the term cirrhosis, both in the liver and kidney, to denote the pale and yellow rather than the granulated state of the organs.



Fig. 7. Fatty granules and globules as seen in fluid scraped from the cortical substance of a very pale and soft kidney (slightly granulated). The existence of a large number of free nuclei of epithelium cells, as at b, b, indicates the imperfect formation or disruption of these cells. Some of them are, however, seen entire, as at a, and contain fatty granules in different proportion. In this exudation the oil is relatively in large, the albumen in small quantity. (250 diameters.)

Fig. 8. Fluid scraped from cortical substance in kidney of Biggie (see p. 811). The entire epithelium cells (a a) are in much larger proportion than in the last case. Some of them are nearly clear, others filled in different proportions with granules, constituting the forms of cell described by authors as inflammation globules, exudation cells, fatty corpuscles, granular cells, &c.. Free nuclei (b) and free fat granules are also seen in considerable numbers. The proportion of oil is in this case also relatively large. (250 diameters.)

According to my observations, fatty exudations from the tubes present themselves under two different aspects: *First*, free molecules, granules, and globules, intermingled with the cells and nuclei of the secreting structures; *Second*, similar granules, &c., enclosed within the cell-wall, between it and the nucleus. In regard to the first of these forms it is not necessary to repeat what has been already said; but the second demands a few words of explanation.

Dr Johnson is certainly the first who has stated distinctly, and kept constantly in view, the fact of the accumulation of fatty granules in the glandular epithelium of the kidney, having been guided in doing so by the facts previously stated by Bowman with respect to the liver. In regard to these facts there is no doubt; nor is there any doubt that their disregard by the continental writers on the kidney, has introduced much needless complexity into their descriptions.

Fatty deposit may exist in the cells of the kidney in large or small granules. It sometimes takes the form of a nearly molecular deposit; in this case it forms a mere shading, obscuring the nucleus, and rendering the cell more or less opaque. At other times, cells may be seen in different states of fulness, their contents being granules of nearly equal size, and not larger than from 1-500th to 1-300th of a millimetre. This is the granular corpuscle or inflammation globule of German writers. Again, the contained granules may be very unequal in size, the cell being irregularly distended or partially filled with granules and globules, from 1-500th up to 1-100th of a millimetre. It is rare in the kidney to find the contained granules so large as they are observed in the liver, where they not unfrequently fill nearly the entire cell. These appearances are illustrated in Figs. 7 and 8.

Fig. 9.



Fig. 10.

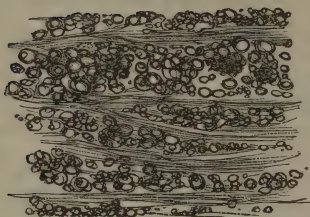


Fig. 11.

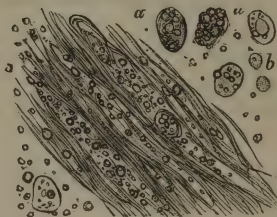


Fig. 9. Section of the cortical substance in a granulated kidney; the tubes are filled with granules and molecules of fatty or oleo-albuminous exudation. A Malpighian body is also occupied by exudation. The granules are mostly of small size. The tubes appear somewhat shrunk, the fibrous tissue having contracted around the deposit. (250 diameters.)

Fig. 10. Exudations, consisting of fatty globules of considerable size, in tubes of kidney of Biggie (see p. 811). (250 diameters.)

Fig. 11. Scattered granules in tubes of the pyramidal substance, in a kidney in which many of these were obliterated; a, cells from cortical substance of the same kidney; b, nuclei from do. (250 diameters.)

In the tubes the fatty granules may appear to occupy the whole cavity (Figs. 9, 10), or to be sparingly disseminated (Fig. 11). Here, too, they may present every variety of size—from the smallest perceptible points or molecules, up to twice the size of the nuclei, in which case they are generally free. The tubes containing the fatty granules sometimes appear distended, at other times smaller than natural, as if they had contracted around the fat (Fig. 9). All these appearances are best seen in sections by Valentin's knife; and, when the tubes are displaced or roughly handled, the fatty deposit is very apt to be squeezed out of them.

It never happens that the whole of the tubes and cells of a kidney are equally the seat of fatty deposit. Even in the most diseased kidneys some healthy cells and tubes can generally be found; and, on the other hand, it is not unusual in kidneys apparently quite normal, to meet with a few cells containing fatty granules in greater or less number. This is, however, a departure, though a trivial one, from the strictly normal state of the cell.

The general description of the appearances produced in the kidney by the occurrence of deposits in the tubes (pp. 796-8), is applicable with peculiar force to the oleo-albuminous exudations, which are of

all others the most persistent, and the most frequently disorganizing in their effects.

The following cases are selected with a view to exhibiting the different stages, and most characteristic appearances presented by this form of exudation into the tubes.

OBSERVATION IV.—*Mottled Smooth Kidney—Partial and slight Exudation of Fatty Granules in the Epithelium Cells—Deposit of Urate of Ammonia—Phthisis—Pericarditis.*—John Young, æt. thirty-eight, printer, admitted into the Royal Infirmary, October 26, 1847, under the care of Dr Robertson: had for three years suffered from cough and dyspnœa, latterly attended with night sweats, diarrhœa, and rapid emaciation. He had observed slight hæmoptysis on one occasion two years before admission. He had never had pain in the chest. On admission there was aphonia, but no urgent symptom. The normal sounds of respiration were obscure, and supplanted by mucous and subcrepitant râles, particularly in the left lung, where there was comparative dulness on percussion at the upper part as well as at the lower dorsum. He died on the 12th November, having suffered from increased cough and dyspnœa, constipation and nausea.

On examination the lungs were found extensively affected with miliary tubercle. There was dense fibrous induration, of an iron-gray colour, at the upper part of both lungs, affecting most extensively the left; and a few small dry caverns existed in the upper lobes. The pericardium contained a turbid serum, with a few shreds of lymph. The liver weighed above 4 lbs., and presented on section a pale waxy appearance; it contained rather small fatty granules in nearly all the cells. There were a few tubercular ulcers in the ileum. The kidneys weighed each above 5 oz. They presented a smooth surface, and were of firm consistence. The cortical substance was of uniform density, but slightly mottled throughout from the greater vascularity of some parts than others; the gradations of colour, however, passed insensibly into one another. The vascular injection was nowhere absent, but was on the whole diminished.

On microscopical examination, the tubes and their contained nuclei were very obscure till after the addition of acetic acid, which had the effect of rendering them perfectly distinct, apparently by the removal of a fine molecular haze. The cells were then seen to be well formed and mostly entire, but they contained in many instances fatty granules from 1-300th to 1-120th of a millimetre in diameter, which were also seen to fill some of the tubes. These could be removed entirely by ether. (When a thin section of a kidney is to be treated with ether, it should be placed, carefully spread out by needles, on a plate of glass, which should be gently inclined, and ether should then be poured drop by drop over the whole surface of the section; this will generally remove much of the oil. The section should then be taken up with the forceps, spread on a clean plate of glass, and treated with water or acetic acid in the ordinary manner. The process may require to be repeated.)

This case presents a good example of the slightest form of the fatty exudation. The organ is mottled from the unequal distribution of the vascular redness; it is slightly increased in volume; its consistence is not altered; its vessels are nowhere obliterated. The secreting cells are well formed, and the presence of an oily deposit in them is the only abnormal appearance.

The fatty exudation was in this case, as very frequently occurs, common to the liver and kidney, and was probably indicative of no special morbid tendency in either organ, but rather of a cachectic

state of the system, in which the respiratory, secreting, and nutrient functions suffer a common decay. There was no symptom calculated to lead to an examination of the urine.

OBSERVATION V.—*Pale Kidney, with general Fatty Deposit in the Tubes—No Granulations. Fever? Delirium Tremens*—Robert Kirkwood, æt. fifty-six, was admitted into Dr Paterson's ward in the Royal Infirmary, on December 7, 1847, with symptoms of delirium tremens. He was ascertained to have been of intemperate habits. He died three days after admission, December 10th.

On examining the body (December 12th), there was considerable serous effusion into the subarachnoid spaces and the ventricles of the brain. The spleen weighed fifteen ounces and was softened, as were most of the organs. The kidneys weighed each four and a half ounces; they were exceedingly pale; the surface was smooth and pale, presenting only a few stellar veins; the Malpighian corpuscles were much obscured, and under the microscope appeared destitute of blood. The tubes obviously contained, throughout the cortical substance, a number of oil granules and globules of different sizes, up to the 1-100th of a millimetre. On examining the contents of the tubes separately, the oil granules were found to be mostly floating loose; the cells imperfectly formed; their membrane thin and delicate, and many of the nuclei free. (See similar appearances in Fig. 7.)

This case differs from the last chiefly in the imperfect development of epithelium, and in the more general diffusion of the exudation, which gave rise to greater paleness of the organ.

OBSERVATION VI.—*Very general Deposition of Fatty Granules in the Tubes of the Cortical and Pyramidal Substances—No Granulations—Incompetency of Aortic Valves—Hypertrophy of Heart*—Alexander Durham, æt. twenty-nine, plumber, admitted into Edinburgh Royal Infirmary under Dr Douglas, on March 5, 1848. He had been affected from the beginning of the year with cough, dyspnoea, and palpitation, and had on one occasion expectorated a small quantity of blood. There were on admission accelerated respiration and orthopnoea, sibilant râles in the chest, and the physical signs of aortic regurgitation, with hypertrophy of the heart in a very marked form. On the 11th March there was observed slight swelling of the limbs; the urine was tested and found to be albuminous, though faintly so; it was of good specific gravity, and 18 oz. (had been generally not more than 10 oz.). The œdema increased till his death, which happened rather suddenly on the 15th. On the 13th he had slight hæmoptysis, with temporary relief to the symptoms; but the orthopnoea continued to the last.

The heart weighed 22 oz., through hypertrophy chiefly of the left ventricle; the aortic valves were incompetent; the lungs were extensively studded with hemorrhagic extravasations. All the abdominal organs were normal, except the kidneys; these were above the normal weight, and dense. They were, however, very friable, so that in removing the capsule small portions of the cortical substance adhered to it. The vascular striæ opposite the bases of the pyramids were distinct; the vascularity of the capillaries and the Malpighian bodies not appreciable. The cortical substance was slightly and minutely mottled; the striæ of the pyramids, from their bases through two-thirds of their length, were marked by the presence of a white opaque deposit.

Microscopic Examination.—The greater part of the deposit proved to be granules and globules of fat, which appeared to fill the tubes in every part of the organ; it was mostly unconnected with cells. The epithelium was very imperfectly developed, many of the nuclei being free. The tubes themselves seemed in no way displaced or deformed.

In this case there was albuminous urine, constantly deficient in

quantity, and generally loaded with urate of ammonia, which formed a portion of the deposit in the tubes of the kidney. There was no marked permanent disorganization of the organ; but the deposit in the tubes was as general as I remember to have seen it without such disorganization taking place.

It is to be remarked, that in the urine there was no apparent sediment except that of amorphous urate of ammonia, which disappeared on heating, leaving the urine clear. It is therefore probable that it did not contain oil in any appreciable quantity. It is to be regretted, however, that a chemical examination of it was not made.

OBSERVATION VII.—*Fatty Granules and Globules in the Tubes, extending into the Pyramidal Portion—No Granulations—Fever?*—John Biggie, æt. twenty-six, Irish labourer, admitted July 26, 1847, into Ward 3, Edinburgh Royal Infirmary, in a state of extreme prostration, with impaired intelligence, weak pulse, brown and dry tongue. He was stated to have been eight days ill of fever. His abdomen presented slight swelling, with indistinct fluctuation. On the 28th, abdominal swelling and fluctuation increased; prostration greater; some yellowness of the conjunctivæ and surface. Died. The urine could not be procured.

On dissection, the cellular tissue, which contained within its substance a good deal of fat, was infiltrated with serum. The serous cavities also contained more or less of fluid. The liver weighed 4 lbs.; it was of a nut-brown colour; its surface presented numerous mammillary projections, varying from the size of a mustard-seed to that of a pea; a similar granulated disposition pervaded the whole organ. The gall-bladder contained a small quantity of bile. The spleen weighed nearly 3 lbs.; its consistence was diminished, and its colour dark. The kidneys were of normal size, flabby, and soft; the capsule brought away with it small portions of their tissue. Both cortical and pyramidal substances were very pale in colour. (Condensed from the report of Dr Waters, then pathological clerk.)

The microscopical appearances of the kidneys in this case are delineated in Figs. 8 and 10. The tubes were for the most part full of oil globules of considerable size, mixed with smaller fatty granules. Some of them were, however, nearly healthy. The deposition of fatty matter extended to the pyramids, from a section of which Fig. 10 was drawn. A section of the cortical substance presented an appearance under the microscope very like Fig. 9, but with occasionally larger globules. The epithelium cells were mostly entire and well formed, but in their interior presented various degrees of granular exudation.

In the two last observations, the deposit was connected with extreme diminution of the activity of the renal function, and this without any obvious destruction or disorganization of the substance of the organ. So far as can be judged from the pathological appearances alone, there seems to be no adequate reason why a kidney in the state above indicated should not return to health, the deposit in the tubes being removed or reabsorbed. If, on the contrary, the tendency to this deposition should continue, it seems to be inevitable that the consequence must be a complete breaking up of the structure of the organ, and such obstruction to its functions as must lead to speedy death. I have several times seen kidneys which appeared to have undergone this form of degeneration to a greater or less extent; but none so

marked as in the following case, where both the liver and the kidneys seemed converted into a species of atheroma.

OBSERVATION VIII.—*Complete Atheromatous Degeneration of Liver and Kidneys—Pus in Hepatic Vein.*—Mary M'Gonagil, æt. thirty, admitted Jan. 26, 1848, into Edinburgh Royal Infirmary, under the care of Dr Douglas, with marked enlargement of the liver, apparently of five years' standing. Before this she had nine still-born and premature children. She had been subject to obstinate costiveness, and pains of the abdomen and loins. The constipation and tympanitic distension of the abdomen were overcome by remedies, and she continued pretty well till the 3d February, when marked tenderness of the epigastrium and right chest became developed, with pain on motion of the corresponding arm. On the 7th there was great and indefinite distress; the right front of the chest was the seat of a firm, diffused, and painful swelling, seated apparently in the cellular tissue. Tenderness extended from this to the axilla. The same day she died. (The urine was always very pale and scanty.)

On dissection, the surface was very pale; the thoracic organs were healthy; the liver was of large size, and exceedingly irregular form; its surface was every where uneven, at some places puckered and depressed; its substance was soft, friable, in some parts almost diffuent; it presented no appearance of structure or vascularity; scattered throughout were dense semi-transparent masses, which had the appearance on section of pieces of fibro-cartilage. The hepatic vein contained a good deal of pus. The kidneys were of the normal size, soft, and flabby; on stripping the capsule, the smoothness of the surface was seen to be diminished; this appearance, however, was found not to be the result of the development of granulations of Bright, but of a peculiar condition of the cortical substance, which had lost its characteristic appearance, and became converted into a homogeneous friable mass; this, however, possessed greater consistence than that of the liver above described.

Microscopic Examination.—The texture of both liver and kidneys seemed converted into fatty granules and globules of different sizes, amid which very few secreting cells could be seen. In the kidney, however, by diluting the mass with water, fragments of tubes could be observed greatly attenuated, sometimes consisting of basement membrane alone, sometimes with a few nuclei attached. Many of these fragments were empty, others contained deposit in large quantity, and were very irregularly distended. A few loose Malpighian bodies, empty and shrivelled, were also seen. The cartilaginous-looking matter in the liver was composed of peculiar thin and delicate transparent laminae, wholly unlike any of the normal tissues of the organ; they were insoluble in hot ether and in strong nitric acid, but I did not pursue the investigation into their nature further.

The preceding observations afford sufficiently characteristic examples of the fatty deposit in that form in which it is infiltrated in various degrees through the whole of the tubes. Very frequently, however, especially when the disease is very chronic in its character, the exudation tends to occupy particular points of the organ, leaving many of the tubes free; the points so occupied are the granulations of Bright, the formation and progress of which I have already explained. The observation which follows will illustrate this.

OBSERVATION IX. *Irregular Granulations (early stage) throughout the Cortical substance of Kidney—Exudation consisting of small Fatty Granules—General Dropsy.*—George Whitnall, a weaver, æt. thirty-seven, admitted January 14, 1848, into the Royal Infirmary, under the care of Dr Paterson. He had laboured under general dropsy for about twelve months, which became ex-

cessive shortly before admission. He was much prostrated, and died next day (January 15th). The urine was not procured.

The lungs were highly emphysematous; the heart slightly hypertrophied. The abdominal viscera were healthy, with the exception of the kidneys. One kidney weighed 8 oz., the other 9½ oz. The capsule was easily stripped. The surface, which was pale, approaching to a flesh-colour, with a few stellar veins, was interspersed with yellowish opaque granulations, irregular in form, and very slightly elevated. They did not exceed the size of a small pin's head, and were pretty uniformly scattered throughout the cortical substance, which was completely anemic, and sharply divided from the highly injected pyramidal substance. The latter contained a few points of yellow deposit, near the bases of the pyramids. Both cortical and tubular substance were hypertrophied.

Microscopic Examination.—The organ was so friable as not to admit of a moderately thin section being made with Valentin's knife. Owing to this circumstance the structure was very indistinctly discovered; but there was seen, disposed in irregular masses throughout the cortical substance, an exudation composed of very fine and small granules, having the refraction of oil, and not affected by acetic acid. The nuclei were abundant, the complete cells few.

This case presents an instructive instance of the earliest form of the granulations of Bright. It is that figured by Rayer (Atlas, Plate VIII. Figs. 1 and 2), and except in the slight prominence of the granulations on the surface, and their somewhat yellowish colour, corresponds exactly to his fourth form of Bright's disease, in which he describes the granulations as "*petites taches d'un blanc laiteux,*" and as appearing "*sous la forme de lignes irrégulières, comme floconneuses, qui semblent se continuer avec les stries divergentes des cônes tubuleux.*" The small size of the granules, forming the exudation in this case, is probably owing to the absorption of part of the oily matter, in consequence of which there is an altered relation between the proportions of the constituent parts of the exudation—(See ante, p. 806). The irregular form of the granulations, their small size, their copious diffusion through the cortical substance, and their existence even at the bases of the pyramids are all accounted for, if we consider this kidney to have passed through a stage of general fatty infiltration, similar to that of Durham or Biggie (Obs. VI. and VII). If, in either of these kidneys, the deposit had been absorbed from some of the tubes, and persisted in others, and had then become consolidated and opaque from the removal of part of its fluid constituents, they would have presented exactly the appearances here indicated.

May, then, the early stage of the granulations of Bright be considered as, in some instances, the retrograde movement of a still more threatening condition,—viz. the universal fatty infiltration?

The progress of the granulations of Bright is connected with other changes of a different character, such as atrophy of the surrounding tubes, obliteration of vessels, &c.; and will therefore come under consideration in the other sections of this memoir. Enough has been said for the present to show their connexion with the fatty exudations in the tubules.—(*To be continued.*)

Part Second.

REVIEWS.

Practical Observations on Midwifery, and the Diseases incidental to the Puerperal State. By ALFRED H. M'CLINTOCK, M.D., &c., and SAMUEL L. HARDY, M.D., &c., Dublin: 1848. 8vo. Pp. 368.

THIS is an excellent and valuable specimen of a class of works we like to see; and we are only sorry that our Dublin brethren have, with their unequalled opportunities, been so niggardly in supplying the profession with the data on obstetric subjects which their hospital affords. With the exception of Dr Collins, and our authors in the work before us, we may say they have done absolutely nothing; Drs Clarke and Breen's reports being rather memorials of the literary apathy of these fortunate men, than contributions worthy of being presented by them to the scientific world. With even ordinary industry on the part of the physicians of that institution, what a mass of facts might we not have had with which to compare and qualify the various observations and opinions which have been promulgated on this important branch of medicine!

Besides a short introduction, the contents of Drs Hardy and M'Clintock's work are arranged under the following heads:—Natural, tedious, and difficult preternatural and complex labours, convulsions, rupture of the uterus, twin births, funis presentations, opthalmia neonatorum, and the management of still-born children.

Their statistics extend over a period of three years from January 1, 1842, to January 1, 1845, during which 6634 women were delivered in the hospital of 6702 children. Among the whole there were 65 maternal deaths, and 6235 of the children were born alive.

Of the 6634—5852 were cases of natural labour; 259 were tedious; 101 were breech cases (exclusive of 39 which occurred among the twins); 38 were footling cases (exclusive of 23 twins); and 20 were cases of presentation of the shoulder or arm (exclusive of 6 twins). They enumerate the twin cases among the shoulder and arm presentations, while they exclude them from their list of breech and footling cases; and thus they make a total number of 227 preternatural labours, whereas there were actually either 221 or 289. Among the complex cases there were 37 of hemorrhage before delivery, including 8 cases of placenta prævia; and 56 of hemorrhage after delivery. The placenta was retained in 28 cases. There were 13 cases of convulsions, 9 of rupture of the uterus, and 37 of prolapse of the cord; 95 women were delivered of twins, and one had three at a birth. One case of spontaneous evolution occurred in an eight month child.

Among the 5852 cases of natural labour, 16 of the mothers

died; among the tedious and difficult labours, 22. There were 3 maternal deaths in breech cases, and one in foot and one in shoulder and arm presentations. Three of the cases of placenta prævia, and four of the others in which hemorrhage occurred before delivery, proved fatal to the mother. In 7 of the cases of hemorrhage after delivery, the mother died, and 10 of those in which the placenta was retained were fatal; but 5 of these are also included among the fatal cases of hemorrhage after delivery. The mothers died in all the cases of laceration of the womb; in 3 of the convulsion cases; in 2 of the twin cases; and in one case of prolapse of the cord.

The number of children born alive was, in each of these varieties of cases, as follows:—In the tedious and difficult labours, 139; in the breech cases, 64, exclusive of twins; in presentations of the lower extremities, 20, exclusive of twins; in cross births, 9, exclusive of twins; in placental presentations, 5; in other cases of hemorrhage before delivery, 18; in convulsion cases, 7; in cases of laceration of the uterus, 1; in prolapse of the cord, 12; 171 of the twins, and all the three in the triple birth, were born alive.

We extract the following list of the causes of death in the 65 maternal fatal cases:—

Phlebitis and Arthritis,	-	-	-	-	-	24
Ruptured Uterus,	-	-	-	-	-	9
Peritonitis,	-	-	-	-	-	7
Sloughing of Uterus and Vagina,	-	-	-	-	-	6
Effects of Hemorrhage,	-	-	-	-	-	5
Convulsions,	-	-	-	-	-	2
Mania,	-	-	-	-	-	1
Bronchitis or Pneumonia,	-	-	-	-	-	3
Phthisis,	-	-	-	-	-	2
Anasarca,	-	-	-	-	-	1
Scarlatina,	-	-	-	-	-	1
Jaundice,	-	-	-	-	-	1
Tumour on Bronchus,	-	-	-	-	-	1
Anomalous diseases,	-	-	-	-	-	2

From this list the authors think it would be fair in estimating their mortality to deduct the last 11 cases, as in these they assert “the cause of death was a strictly non-puerperal disease,” and also four others, in which we are told the patients were upon admission in a dying state; but we beg to differ from them on this point. We have looked into the circumstances of these cases, and can find no good reason for subtracting any of them. Neither can we allow the exclusion of puerperal fever on the ground of its being an accidental occurrence. In this way every one of the unfortunate cases might be set aside.

Drs Hardy and M‘Clintock have taken as a model the practical treatise of Dr Collins, and we believe they could not have fixed upon a better. If they had only followed it more closely, their work would have been greatly increased in value. The omission of the chapter on patients dying in the hospital, on still-births, and on children dying in the hospital, and more particularly the want of such gene-

ral tables as are to be found at pages 22 and 27 of Dr Collins' reports, are all serious defects. But what, in our opinion, detracts chiefly from the statistical value of their book, is the circumstance that it does not include the *whole period* of their assistantship.

The importance, in a scientific point of view, of such evidence as that which they have recorded, cannot be overrated, and on this account it is incumbent on us to scrutinize it narrowly. For there is no denying that there is a spirit of rivalry between men, and institutions, and countries, that is prejudicial to the advancement of science; and it is of the greatest consequence that every circumstance affecting such evidence should be made known.

We are far from imputing any intention to deceive to the authors of the work before us. On the contrary, we believe them to be men of the strictest honour. But we cannot avoid stating our surprise, that Drs Hardy and M'Clintock should have omitted to report a very considerable portion of the period during which they filled the situation of assistants. If we mistake not, too, one of them was assistant during only part of the time included in the report. But, however this may be, we know at least that they were both assistants during the greater part of the year following the date of their report.

We have made some inquiries regarding the period omitted, and have discovered one fact regarding it of great importance; namely, that the mortality must have far exceeded that of the three years reported. We cannot at present state the exact number of deliveries, nor that of the deaths, during the whole of this period; but we know, that within two weeks (from the 21st of February to the 6th of March, inclusive of both) in the spring of 1845, out of 77 cases 10 died of puerperal fever alone; and again, that there were 10 fatal cases among 265 deliveries which occurred during eight weeks (from the 16th of August to the 10th of October inclusive) in the autumn of the same year,—a mortality of 1 in about 14 for these ten weeks; and, supposing there was not another death in the remaining 4-5ths of the year, and that there were not above 1200 deliveries, and we do not believe there were, we should have a mortality of 1 in 60 for that year.

That the mortality in the Dublin Lying-in Hospital should be so large is not at all wonderful to us, when we consider that, notwithstanding the great and steadily progressing improvement which has taken place in the practice of midwifery elsewhere, in that hospital little or no alteration has been permitted since its foundation, more than ninety years ago. They still think it a duty to allow the poor patients to suffer in extreme agony for days, nay, till death threaten to interfere, before they give any instrumental assistance to expedite the labours. And, when they do step in, they persist in their preference of instruments which the results of experience have proved to be most dangerous in their application; and to confide in a routine of treatment which their ill success in the management of the accidents and diseases of puerperal women, most unequivocally

condemn. It was all very well, nearly a century ago, to make a decided stand against the forceps, which at that time appears to have been much abused; but the necessity for an exaggerated opposition to that instrument has long since gone by, and, on the contrary, the risk now is, that the operation which requires the least professional skill (as the one preferred in the Dublin hospital does) will be most readily resorted to.

That the simple protraction of labours has a serious effect on the issue of each case (which the Dublin Hospital physicians have always denied), has been strikingly illustrated in a recent analysis of Dr Collins' cases by Professor Simpson,¹ in which it is demonstrated that the proportion of maternal deaths, and of morbid complications, increases with the duration of labour, and that the maternal mortality attendant upon forceps and crotchet operations is also regulated by the same circumstance, as the two following tables, constructed from Dr Simpson's, show plainly enough.

TABLE I.—*Showing the Proportions of Maternal Deaths, of Infantile Deaths, and of Morbid Complications in Labours of different Durations, reported by Dr Collins.*

Duration of Labour.	Proportion of maternal deaths.	Proportion of complicated cases.	Proportion of infants still-born
Within 1 hour,.....	1 in 322 died.	1 in 114	} 1 in 20 lost. ... 18 11 6 3 2
1 to 2 " 90	
2 to 3 " 231 69	
4 to 6 " 134 58	
7 to 12 " 80 32	
13 to 24 " 26 12	
25 to 36 " 17 9	
Above 36 " 6 5	... 2

TABLE II.—*Showing the Maternal Mortality attendant upon Dr Collins' Forceps and Crotchet Cases in Instrumental Labours of different Durations.*

Duration of Labours.	RESULT TO MOTHERS.	
	In Forceps Cases.	In Crotchet Cases.
In labour under 24 hours,.....	1 in 13 died.	1 in 19 died.
From 25 to 36 " 6 ...	} ... 8 3 ...
" 37 to 48 " 4 ...	
Above 48 " 2 ...	

And to show that these results are not peculiar to Dr Collins'

¹ Provincial Medical Journal for February 9, 1848.

report, we add two tables which we have constructed from the reports of two other physicians of the same hospital.

TABLE III.—*Showing the proportion of Maternal and Infantile Deaths, in relation to the duration of Labour, in Dr Joseph Clarke's*¹ 9748 uniparous Cases of Delivery in which the Children presented with the head.

Duration of Labour	PROPORTION LOST.	
	Of Mothers.	Of Children.
Within 24 hours,.....	1 in 137	1 in 28
Above 24 „ 5	... 2

TABLE IV.—*Showing the proportion of Maternal and Infantile Deaths, in relation to the duration of Labour, in Dr Breen's*² 11,695 Cases.

Duration of Labour.	PROPORTION LOST.	
	Of Mothers.	Of Children.
Within 30 hours,.....	1 in 101	Not stated. ³
Above 30 hours,.....	... 7½	1 in 3½ ⁴

The frequency with which they resort to craniotomy in that hospital, cannot be too strongly condemned. For it must be borne in mind, that it is not because an appreciable disproportion between the parts of the foetus and of the maternal passages is a common occurrence there; on the contrary, monster foetuses and distortion of the pelvis are very seldom indeed observed within its walls. We venture to say, that they cannot give the excuse of disproportion for one tenth of their crotchet cases. Their reason for resorting to it so often is, that they consider it perfectly safe; while they conceive that all other operations are necessarily attended with the most fearful consequences. They must, however, allow, that with the crotchet the child cannot be saved; and, on the other hand, that by means of turning, and the forceps, and lever, it gets a chance of life. And if they would but analyse with candour their own statistics, they could scarcely avoid admitting, that even these do not justify the high

¹ *Vide* Transactions of Physicians in Ireland, 1817.

² *Vide* Dublin Medical and Physical Essays, Vol. I.; and Edinburgh Medical and Surgical Journal, Vol. XV.

³ The average proportion of still-born births in the Dublin Hospital, including labours of every kind, is about 1 in 17.

⁴ This calculation does not include instrumental cases.

encomiums with which they have been accustomed to exalt the crotchet over all other methods of assistance, with regard to the safety of the mothers; seeing that, exclusive of rupture of the uterus, the reports of Collins, and of Hardy and M'Clintock, show that about one in every four mothers die in the cases in which the crotchet is used, while the mortality in forceps and lever cases is about one in six, and in turning one in sixteen.

There might also be some excuse for their preference of such a fearful operation, if they were more successful than others in performing it; but it would be easy to show that the very reverse is the case. For example, Drs Clarke, Collins, Murphy, Hardy, and M'Clintock of the Dublin Hospital, report 261 crotchet cases, of which one in every three and a half died; while of the 209 cases recorded by Drs Smellie, Ramsbotham, and Lee of London, one in six one-third were fatal, that is, a mortality of not much more than one-half of what occurred in the Dublin Hospital practice.

It is worthy of notice, too, that in the London practice, judging from the reports, which give sufficient data, crotchet operations are less frequent as well as less fatal. The Ramsbothams perforated in only one in every 940 of their 35,745; while in 39,134 reported by Clarke, Collins, Murphy, Hardy, and M'Clintock, the proportion was one crotchet operation to every 149 cases. And the Germans use the perforation less frequently still. The following table shows not only the proportion of crotchet cases to deliveries, but the proportion of lives lost in crotchet cases in relation to deliveries:—

TABLE V.

Reporters.	Total Numbers.	Proportion of Perforations to Deliveries.	Proportion of cases to whole Deliveries
Riecke,.....	221,923	1 in 2264	1 in 1668
Ramsbotham, ¹	35,745	1 in 940	1 in 812
Dublin Hospital, ² ..	39,134	1 in 149	1 in 116

But it is not only with regard to the use of the crotchet that the Dublin Hospital practice is inferior to that of the metropolis; they are still more unfortunate in the results of their forceps operations. Clarke, Collins, Murphy, and our authors, lost 13 in 72 mothers delivered by the forceps, or 1 in $5\frac{1}{2}$; Smellie and Ramsbotham only lost 5 in 101, or one in 20. And even this operation is less frequently resorted to in London. For we find that the Ramsbothams had recourse to it 49 times only in 35,745 labours, or once in 729; while Clarke, Collins, Murphy, Hardy, and M'Clintock,

¹ Smellie's mortality in crotchet cases is less still than Ramsbotham's.

² Including Clarke, Collins, Murphy, Hardy, and M'Clintock.

used it 72 times in 39,134 labours, or once in 543. And although the Germans do use the forceps in so large a proportion of cases as about 1 in 80, it is important to bear in mind that their practice, so far as regards operations, is on the whole far more successful than that of the Dublin Hospital, as is shown in the following table:—

TABLE VI.—*Giving a comparative view of the London, German, and Dublin Hospital practice.*

Reporters.	No. of cases.	No. of operations in 1000 cases.	Proportion of deaths to 1000 operations.	Proportion of deaths to every 1000 cases.
Ramsbotham—London,	35,745	2 $\frac{1}{2}$	666 $\frac{2}{3}$	1 $\frac{2}{3}$
Riecke—Wurtemberg,	221,923	12 $\frac{3}{4}$	316 $\frac{1}{7}$	4 $\frac{1}{26}$
Clarke, Collins, and } Murphy—Dublin, }	32,500	8 $\frac{2}{3}$	985	8 $\frac{1}{4}$

It was our intention to have given an analysis of the cases that are detailed in Drs Hardy and M'Clintock's report, to show the effects of mercurial treatment, in which there appears to be such confidence placed in the Dublin Lying-In Hospital; but this we must at all events defer. Suffice it to say, that we do not believe there is on record a more unfortunate set of cases. The very small proportion of recoveries from any puerperal affection, and the frequency of mercurial diarrhoea and dysentery, and of ulceration of the bowels, not to mention any after effects of the drug, might stagger the uneducated empiric. We trust that the physicians of the Lying-in Hospital will not continue to blind their eyes to the light which their own observations have unwittingly cast on this subject, and that they may at length be induced to look more to experience, and less to custom, as their guide in this, as well as every other point in the practice of midwifery.

Whilst pointing out these defects, we cannot take leave of this treatise without commending the great ability and care which its authors have evidently bestowed upon it. To all who are interested in obstetric medicine, we highly recommend this work as a very important record of valuable cases and observations.

Des Vegetaux qui croissent sur l'Homme et sur les Animaux Vivants.

Par M. CH. ROBIN, M.D., &c. Accompagné de trois planches gravées. Paris: 1847. 8vo. Pp. 120.

The Vegetations which grow on Man and Living Animals. By M. CH. ROBIN, M.D., &c. With three plates.

SINCE the discovery by Bassi, in 1835, of the vegetable nature of the muscardine in silkworms, the parasitic fungi which grow on living animals have been studied by numerous naturalists, physiologists,

and pathologists. Of late years, the subject has derived considerable extension from the researches of Remak, Gruby, Hughes Bennett, Hannover, Goodsir, Berg, and others, and their united labours are condensed in the work before us. Although it contains little original matter, the compilation is so judicious, and for the most part so correct, as to constitute a very valuable publication. Indeed we consider it a model of what such *resumés* ought to be. We nowhere find the author claiming for himself the labours of others; opinions, or, as they are called by some modern writers, "views," are nowhere egotistically put forward in opposition to facts; the whole consists in the condensation of a large number of the isolated memoirs and notices scattered among different publications, systematized and conveniently arranged, with a short summing up by way of conclusion. We have also a bibliographical account of nearly all the memoirs or notices (111 in number) which have been written on the subject; and lastly, three plates, in which are copied all the figures ever published of these peculiar growths.

The author gives a very good description of the fungus constituting *Tinea favosa*. He seems to be unacquainted with the *second* memoir published on this subject by Dr Hughes Bennett, in the *Monthly Journal* for June 1842, and with the confirmation by that observer of Remak's experiment, proving its contagious nature, recorded in the *Northern Journal* for September 1845. Neither does he notice the extraordinary assertion put forth by some London writers, that this peculiar growth is *not* vegetable in its nature,—a circumstance which, for the credit of British science, we ought perhaps to congratulate ourselves upon.

The author also gives an excellent description of the fungus of Muguet, first discovered by Eschricht, and afterwards studied by Gruby and Berg. He adds some original observations on a filiform vegetation, of which the following is a condensed account:—

1. On the surface of the tongue, on the accumulated matter in the interstices of the teeth, or in the cavity of carious teeth, in certain vomited liquids, or matters passed by individuals with diarrhoea, and in the liquid contained in the stomach after death, there are found a considerable quantity of such peculiar filaments.

2. These filaments are straight, slightly curved or bent suddenly at variable angles, the edges distinct, the extremities not slender, in size 0·001 or more, in length, which varies from 0·020 to 0·100 of a millimetre.

3. These filaments are free and floating when studied in the liquids above indicated, and are very short. They are equally very short if sought for in the matter detached by friction from the surface of the tongue; but there they are often found fixed in great numbers by one of their extremities on a finely granular mass which serves as a soil, and on the surface of which they form tufts, or bushy turf. Those which float are only the truncated filaments separated from their soil. Lastly, in the substance accumulated between the teeth for two or three days, they attain a length of about 0·100 of a millimetre which is that of their perfect development. They are here disposed more or less parallel in straight or waved bunches, closely compressed.

4. There are always found with them vibriones of several species, epithelial cells, pus or mucus globules, and molecular granules.

5. With a power of seven or eight hundred, there may be seen in the filaments small corpuscles more or less separated, round, and very difficult to study well. They are probably reproductive corpuscles.—P. 46.

Having described all the parasitic vegetations found in man, including the *Sarcina ventriculi* of Goodsir, he speaks of those which have been observed in the lower classes of animals. In his concluding observations (p. 95) he states, that the mycodermata have as yet only been found in man,—a statement opposed to an observation he elsewhere notices, that Hughes Bennett discovered them on the face of a mouse. His opinion, that fungi grow on the surface of mucous membranes in the same manner as they grow on azotised substances long exposed to the air, is undoubtedly correct.

Among all the facts observed, there is only one which favours the idea of spontaneous generation, and that is the circumstance of an *aspergillus* developed in the yolk of an egg intact. In all other cases the author holds the same opinion advanced by Dr Bennett, namely, that germs are deposited in a matter, the result of a morbid process, which constitutes their soil.

Although it has been established in science for the last six years, that parasitic fungi are a cause of disease, new facts are every day discovered in connexion with it; and we can recommend the work of M. Robin as the most complete monograph yet published on this interesting subject.

Treatise on Fractures in the Vicinity of Joints, and on certain Forms of Accidental and Congenital Dislocations. By ROBERT WILLIAM SMITH, M.D., M.R.I.A., &c. &c. Dublin: 1847. 8vo. Pp. 314.

THERE are few subjects which give rise to more anxiety and annoyance to surgeons than injuries in the neighbourhood of joints; the thickness of the superimposed parts and the great swelling which so rapidly follows the injury, all render examination and diagnosis difficult; and though a well-informed surgeon will scarcely fail to detect dislocation when present, he may often suffer great anxiety in cases of other injuries, where he cannot readily make out the exact nature of the accident. The more ordinary forms of fracture occurring near joints may, for the most part, be readily diagnosed; but every now and then the surgeon meets with cases which do not present the ordinary appearances, where, in fact, the injury is so complicated as to render exact diagnosis, and consequently prognosis and treatment, exceedingly difficult. It is evident, therefore, that all contributions which tend to throw light on this truly important department of practical surgery, must be highly desirable. The work before us seems fitted to be eminently useful in this point of view, and especially recommends itself to us from the internal evidence it bears of being the result of personal experience and careful observation of the class of injuries we have referred to, combined

with a thorough knowledge of what has been already done thine same field by others.

The author's style is graphic and perspicuous, and his descriptions of the appearances resulting from the various injuries, the causes of these appearances, and their value as diagnostics, are so well arranged as to be at once succinct and full, easily understood and remembered.

Considerable space is devoted to the consideration of the subject of fracture at the neck of the femur. Mr Smith points out the importance of the surgeon being able to decide in his diagnosis as to whether the fracture be extra or intra capsular, as influencing the prognosis and treatment of the case, and he considers that this decision may generally be attained by attention to the degree of shortening *immediately* following the injury: He holds the opinion, that the amount of shortening of the limb in extra-capsular is generally greater than intra capsular fracture; inasmuch as in the latter the retraction of the lower broken portion of the femur must be limited by its ligamentous connexions, whilst in the former, there is nothing to prevent the full retraction of the shaft of the femur by the force of muscular action. This is contrary to the high authority of Sir A. Cooper; but, nevertheless, we think that the anatomy of the parts, and the manner in which the injury generally takes place, fully bear out our author in his conclusions. It seems to us, however, that he perhaps places undue value on the retentive power of the fibrous investment of the neck of the femur, which he terms the cervical ligament, in preventing shortening. We think the strong capsular ligament, and the accessory or ilio-femoral portion of it especially, more likely to offer the principal resistance to the action of the displacing forces.

The causes of inversion of the foot in some cases of this fracture, and the "*questio vexata*" of union by bone or otherwise, are also fully and we think satisfactorily, discussed.

The very common and troublesome accident, fracture of the lower end of the radius, has received a careful investigation; and though perhaps little could be added as regards the diagnosis and nature of the injury to the able and graphic description of Colles, which our author quotes, merely correcting some comparatively trifling errors, yet he has done much, by extended investigations of the subject, to give greater exactness to our knowledge of the various forms of the accident. In speaking of Sir A. Cooper's views of this fracture he quotes the following passage:—"There is an evident projection of the radius and ulna on the dorsal surface, and of the carpus on the palmar surface of the forearm." This quotation is at variance with the generally received opinion, and, what is more to the purpose, it is at variance with what is found on dissection; and therefore we are as much disposed as our author to question the accuracy even of such high authority, only we cannot help thinking that some error must have crept into the last edition

of Sir Astley Cooper's work on fractures; for though we find the passage as quoted, at page 494 of that work as edited by Mr Bransby Cooper, we cannot find it in the large edition published during Sir Astley's lifetime. On the contrary, the description and the illustration there (8th edition, page 355, and Plate XXVIII. Fig. 1) nearly agree with Mr Coles' description, except that, owing to the projection of the ulna, Sir Astley Cooper terms the injury simple fracture of the radius and dislocation of the ulna; whilst his description of simple fracture of the radius, without dislocation of the ulna, evidently refers to fracture immediately above the attachment of the pronator quadratus. With his vast experience and powers of observation, Sir Astley could scarcely have fallen into the mistake of describing the exceptional case as the general rule. In common with most surgeons we have seen many cases of this injury, and we cannot recollect one where the radius and ulna projected backwards; whilst, as regards examination of anatomical specimens, we can state that neither in the museum of the University, nor in that of the Royal College of Surgeons of this city, have we been able to find a single specimen of fracture of the radius immediately above its carpal articulation where the lower or carpal fragment projects forward, and this is only what might be expected *a priori*, from consideration of the attachments of the supinator longus and long extensors of the thumb, and of their action in displacing the carpus and lower fragment of the radius.

Our limits prevent us noticing so fully as we could wish the other important subjects treated of in the work; but all are deserving of the same praise, as evincing great accuracy of observation, and no ordinary power of deducing useful practical rules as to diagnosis and treatment from the facts observed. Indeed, the work as a whole will, we doubt not, be welcomed by the profession as advancing our knowledge regarding the important injuries of which it treats; and the only fault that we have to find with it is, that it omits the consideration of fractures near the elbow, knee, and ankle joints; but we trust that, at no distant period, these may form the subjects of another such publication by an author who seems so well fitted for the task.

On Indigestion: its Pathology; and Treatment by the Local application of uniform and continuous Heat and Moisture. With an Account of an Improved Mode of applying Heat or Cold in irritative and inflammatory Diseases. By JAMES ARNOTT, M.D., &c., London: 1847. 8vo. Pp. 107.

ALTHOUGH heat and cold are universally acknowledged to be therapeutic agents of great power in the treatment of disease, there are none which are so inefficiently made use of in general practice. Any one who has laboured under fever, and experienced (as we ourselves have done) the inexpressible relief afforded by cold applied

to the head, will be convinced that, properly employed, it must in this, as well as many congestive, inflammatory, and irritative diseases, be one of the greatest value. In private practice we have ever found it most beneficial; but in hospitals, where its advantages ought to be most experienced, it has been impossible for us to secure its application in a useful manner. Indeed, there can be little doubt that the order, "cold to the head," in these institutions, is productive of harm rather than good, followed as it is by the application of a few folds of linen or lint, saturated with water, which, though for a moment cold, are immediately rendered warm, and for hours afterwards annoy the patient. In all such cases, the simple and ingenious application of Dr Arnott will prove of incalculable service. A current of water, of the appropriate temperature, is made to flow through a thin waterproof cushion or bladder in close contact with the body. The water runs into the cushion from a fountain reservoir raised above it, through a long flexible tube; and, again escaping from the cushion, it passes through another tube into the waste vessel. The cushion is of a size and form suitable to the part of the body on which the water is to act; and, by a particular contrivance, any pressure from its weight is prevented. The part in contact with the cushion is kept moist either by previously wetting the cushion, or by interposing a piece of wet lint, flannel, or other bibulous substance.

Dr Arnott points out the advantages of this apparatus in numerous diseases; but dwells more at length on its use as a means of applying continuous heat to the stomach in indigestion. It can be made to serve several purposes; for not only can steady warmth or cold be kept up over a part much more efficiently than by means of poultices, fomentations, lotions, affusion, &c. &c., but pressure, an agent of great value to the surgeon, can be accurately employed at the same time. Now, cold and pressure are two of the most powerful agents we possess in checking growth, and, had Dr Arnott been acquainted with the more modern treatises on inflammation, he would have derived from them far stronger and more correct arguments than any he has brought forward in support of his mode of using cold and heat. For instance, by considering inflammation, cancer, and some other organic diseases, as the results of anormal nutrition, it follows that the same circumstances which modify growth in plants and animals will influence the progress of these diseases. Hence, says Dr Hughes Bennett (*On Inflammation, as a Process of Anormal Nutrition*), "for the same reason as the horticulturist who wishes to bring forward a plant places it in a hot-house, so the surgeon who desires to bring on suppuration applies hot poultices and fomentations." Cold also, as it checks growth, is opposed to the formation of inflammatory products.

It has occurred to us while reading Dr Arnott's work, that his apparatus might be employed with the utmost advantage in all our

fever hospitals and sheds. In Edinburgh its application would be very easy. All that is necessary is to convey a water-pipe along the wall in each ward, a little above the patient's head, and then screw one of the tubes to a stop-cock placed here and there for the purpose. We trust that this suggestion will not be lost. In the mean time, we warmly recommend the adoption of Dr Arnott's apparatus, and the perusal of his work as introductory to its proper application. The profession and the public are deeply indebted to him.

Report of the Proceedings of the Pathological Society of London.
First Session, 1846-7. London. 8vo. Pp. 128.

WE consider that the pathological societies which have sprung up in different parts of the country, are destined to be of the utmost service to the cause of rational medicine. The ideas engendered by the examination of morbid lesions, and the structural causes of symptoms, must, sooner or later, lead to a modification of that routine system of giving drugs, which still prevails to so great an extent in the southern part of the kingdom. To assist in advancing science, however, much more requires to be done than usually takes place at the meetings of these institutions, at least such is the opinion we are led to form from the report now before us. Certainly, nothing can be more meagre or unsatisfactory, in a scientific point of view, than the majority of the observations herein brought before us. Thus, in the first twelve observations, which refer to several very important lesions of the brain and spinal cord, the structural alterations in these parts are not even alluded to. In the thirteenth observation by Dr Williams, of melanotic cancer in the spinal canal, &c., we are told that, under the microscope, it presented "numerous remarkably caudate and branched cells filled with dark granular matter, and a few fat globules." We humbly suggest that such descriptions can be of very little service in advancing pathology.

The same fault is to be noticed in all the reports, with a few honourable exceptions, among which we must place two observations of Dr Handfield Jones, one, on a variety of nutmeg liver, and the other, on certain concretions of frequent occurrence in the prostate gland; some examinations of the kidney by Dr George Johnson, and the descriptions of some sections of osseous tissue by Dr Ogier Ward, and Mr William Adams.

Among the more interesting and careful communications brought before the Society, may be noticed a case of premature occlusion of the foramen ovale, by Mr Ebenezer Smith; cases of rupture of the heart and large vessels, by Mr Prescott, G. Hewett, and Dr Peacock; ulcer of the stomach, forming a communication with the cavity of the colon, by Dr H. Davies; fibrous polypus of the intestines, with strangulation, by Dr Peacock; on deposits of cystine, by Dr Garrod; on a rare form of uterine disease, by Dr Ramsbotham; and certain obser-

vations (any thing, however, but complete) on some forms of malignant disease, by Mr Simon.

On the whole, notwithstanding the want of detail, and absence in many instances of accurate description, this first report of the Society is highly creditable to the industry and knowledge of its members. We would advise, that in all cases connected with cancer, tubercle, morbid growths, &c., a careful microscopic examination should be made by some one well qualified for the task. This should be considered a *sine qua non*; for it is discreditable to the London school of medicine to see mere assumptions so frequently brought forward as the basis of speculative opinions, when the facts and true structure of a morbid production can be arrived at by taking a little pains. It should be continually remembered that the naked eye is quite insufficient to enable any one, even the most experienced morbid anatomist, to pronounce with certainty on the real nature of a foreign growth. The annual report also should be enlarged, and figures of the minute structure described should be given. In the actual state of histology, mere description is insufficient. Are, for instance, the bodies described by Dr H. Jones, p. 102 of the Report, the same which Dr Spencer Thomson has figured in the last Number of this *Journal*? By following our recommendations, which apply to all Pathological Societies as well as to the one in London, we conceive that morbid anatomy and pathology would receive a great impulse, and a body of facts gradually be accumulated, that would ultimately lead to generalizations of the greatest utility. Whereas, the mere exhibition of morbid parts, in conjunction with a history of the case, although in a few instances interesting to the practitioner, fails to establish any doctrine of the slightest benefit to medicine, either in advancing the science or improving the art. Of the truth of this statement the Pathological Society of Dublin, which has been established so many years, offers us a convincing proof.

British Cholera; its Nature and Causes considered in connexion with Sanitary Improvement, and in comparison with Asiatic Cholera. By SPENCER THOMSON, M.D., &c. London: 1848. Small 8vo. Pp. 110.

It is perfectly delightful to see rational medicine extending to the provinces, and the light of pathology and judicious reasoning emanating from a country town, to clear up the mists of empiricism and routine practice. Perhaps there is no disease, the causes and treatment of which are considered so well understood as British Cholera. Derangement of the alimentary canal by indigestible substances, especially fruit, are among the chief of the first; while the treatment consists of a purgative to clear away the offending matter, followed by chalk mixture, astringents, and opium. In this little work, however, Dr Thomson satisfactorily shows, that the bilious cholera of this country can never take its origin from

such a cause, and that the provision of wholesome, watery, sub-acid fruits, during hot weather and in hot climates, is rather a providential arrangement for the prevention of disease, than a source of its production. From a consideration of the history and progress of the epidemic bilious cholera of the summer and autumn of 1846, he considers that high atmospheric temperature, and the disease British or bilious cholera, stand related as cause and effect. Heat produces an accumulation of carbon in the system, the effects of which are those hepatic and nervous disorders symptomatic of the disease. A consideration of the influence of intense cold; of those cases where heat was directly introduced into the body; and of those where means for its removal are deficient, lead to the further conclusion, that heat acts by inducing in the system an accumulation of a noxious or poisonous agent, carbon. It follows that although a sudden exposure to heat will often exert some injurious effect upon many unacclimated constitutions, it is certain that most of its hurtful influences in the production of disease, depend upon those numerous subordinate agents which are almost entirely under the control of man to remove altogether, or greatly modify.

This theory Dr Thomson has not only very ably worked out, but he shows how important is the rational treatment of the disease to which it leads. Further, as the Asiatic cholera is always preceded or accompanied by an initiatory bilious diarrhoea, it is upon checking this that our hopes of averting the more dreaded affection must principally depend. In conclusion, we have only to state, that this little work deserves the most careful perusal and consideration of every British practitioner.

Portraits of Diseases of the Skin. By ERASMUS WILSON, F.R.S., Fasciculi 1 and 2. London: 1848. Folio.

IT is impossible for us to speak too highly of the beauty of these portraits, which, as finished and exact representations of diseases of the skin, should find their way into every medical library in the kingdom. They are all good, but more especially the representations of *Tinea favosa*, *Lupus*, and so called chronic psoriasis of the hand. They constitute an undoubted proof of the superiority of British art in similar publications, as will at once be acknowledged by comparing these plates with those of Casenave, now publishing in Paris.

Whilst the artistic part of the work merits our warmest praise, the accompanying observations of Mr Wilson are very far from satisfying us. They are, in short, liable to all those objections we have formerly noticed in this writer (see *Monthly Journal*, March 1847); unpathological, unpractical, and altogether behind the present state of our knowledge in affections of the skin. Mr Wilson has now a magnificent opportunity of producing a work that, if properly conducted, will hand down his name to posterity. We conjure him, for

the honour of British medical science, and out of regard for his own reputation, to lay aside those miserable, empirical, and falsely called "practical" notions which vitiate so many medical publications of the day, and at once determine to adopt only what is true and positively known. We beg him to pay more attention to the labours of his contemporaries, and avoid insulting the understandings of his readers by asserting, that *Tinea favosa* is not of vegetable origin, and not contagious, when scientific Europe knows it to be the one, and direct inoculation has proved it to be the other. We advise him to cancel the letter-press as yet published, and begin *de novo* with the classification of Willan and Bateman as modified by Biett, or in some other way, and entirely disown the unpathological and very faulty arrangement he has again favoured us with. Lastly, it is incumbent on every pathological writer of the day to give representations of the minute structure of morbid growths, and, by conjoining them with the present splendid portraits, much would be done to rendering this work really valuable. Unless these suggestions be attended to, we fear that the present publication will only add to the costly representations of well known objects, without in any way advancing our knowledge, or being serviceable to the profession.

The Pocket Formulary and Synopsis of the British and Foreign Pharmacopœias, comprising Standard and Approved Formulæ for the Preparations and Compounds employed in Medical Practice. By HENRY BEASLEY. Fourth Edition. London: 1848. 18mo. Pp. 455.

Pocket Dispensatory and Therapeutical Remembrancer, comprising the entire Lists of Materia Medica, Preparations and Compounds, with a Full and Distinct Version of every Practical Formula, as authorized by the London, Edinburgh, and Dublin Colleges of Physicians, in the latest Editions of their several Pharmacopœias; to which are subjoined Copious Relative Tables, exemplifying Approved Forms under which Compatible Medicines, &c., may be extemporaneously combined, &c. &c. &c. By JOHN MAYNE, M.D. L.R.C.S. Edinburgh: London, 12mo. Pp. 271.

THE lengthy titles of these two works sufficiently set forth their nature and objects, and to all who stand in need of such pocket helps, either one or the other will answer the purpose. Dr Mayne's therapeutical classification is the most scientific, although the alphabetical arrangement of Dr Beasley seems to be most convenient.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVII.

MEETING VIII.—*Wednesday, April 5, 1848.*—Sir W. NEWBIGGING in the Chair.

1. ON THE CHARACTER AND WRITINGS OF SYDENHAM. BY J. BROWN, M.D.

THE author remarked, that in a composite subject like medicine, comprehending, as it does, a science and an art, each distinct from but dependent upon the other, it was a most interesting and important, but, at the same time, a difficult inquiry, what has been the condition of the art of healing, as an art, at different periods of its history?—an inquiry quite separate from that which has reference to its progress towards a systematic science, strictly so called. This inquiry into practice rather than into principles, he observed, would be made to most purpose, by singling out the great practitioner and practical writer of his time; Hippocrates and his epoch; Sydenham and his; John Hunter, Lennec, or Abercrombie, and theirs. These great men, though differing much in circumstances and internal character, all agreed in their possessing, in large measure, and in rare quality, that native vigour of mind, that practical sagacity, that gift of insight, that principle and habit, of serious, patient, continuous, honest observation, and that knack in the application of knowledge, so as to make it go forth at once, and safely, to the diagnosis and treatment of the disease before them,—a combination of gifts and acquirements, which, as long as human nature and its wants continue unchanged, must constitute the cardinal virtues of the good and great physician.

After adverting to the many reasons why the writings of Sydenham had fallen into comparative disregard, and, in particular, to their being in many respects scientifically obsolete, and buried in a language now all but dead, he observed that it was the lot of all the most successful practitioners of the art of healing, to be invaluable while alive, and to be soon forgotten after they were dead, much of what was best in their practice, and its reasons, being, by the very nature of things, incommunicable, and dying with them. He contrasted the fate, in this respect, of Sydenham, with that of the great Harvey, whose fame every day brightened, and the value of whose discovery every other discovery in physiology enhanced the more. He instanced as examples of the same thing, in our own day, Sir Charles Bell and Dr Abercrombie. The one, inventive, speculative, originative, scientific; the other, sagacious, observant, practical. He then took occasion to call attention to a very remarkable passage by Plato, in his *Theætetus*,—"That particulars (or, as we call them, instances or facts) are infinite (and therefore, in their unreduced condition, impossible and unmanageable), and the higher generalities (scientific truths demonstrable and referrible to exact rules) give no sufficient direction in medicine; but that the pith of all sciences or knowledge, that which makes the artsman differ from the inexpert, is in the middle propositions, which in every particular knowledge are to be taken from tradition and experience." He illustrated this at some length, and said it contained the essence of all that was true as to the distinction between philosophy and common sense—speculation and practice—science and art—remarking that Sydenham was eminently a *craftsman*, and great in the "middle propositions;" at the same time stating, that all great practical men, as Franklin, Watt, John Hunter, &c., while they preferred working among practicals, among the middle propositions, were far from destitute of the speculative faculty, though they used it rather actually, than formally, and indeed that they could not have been what they were, or done what they did, without possessing the capacity of philosophizing. He directed attention to the higher qualities of Sydenham's character, as exhibited in that regard for the welfare of his fellow-men and the glory of his Maker, which

distinguished him in his life, and in his writings, and he quoted a passage from the preface to the first edition of his *Observationes Medicæ*, in which he says,—All men giving themselves to medicine should lay to heart these four things; 1st, That they must one day render an account to the supreme Judge of the manner in which they have dealt with the lives of those committed to their care; 2d, That they should disdain to make their high and honourable calling subserve the mean uses of avarice and ambition; 3dly, that they should realize the dignity of human nature, that material in which their work lay, by remembering that the only begotten Son of God, in becoming man, ennobled by his own majesty the nature he assumed; and finally, that the sense of their own frailty and mortality should prevail with them to use all diligence and the most tender affection towards their fellow-sufferers.

He then read various extracts from Sydenham's treatises illustrative of his enlightened, manly, and thoroughly English understanding—of his sagacity, and of the value he put upon, and the rich reward he got from, studying the external phenomena of disease, what he called its natural history, apart entirely from all hypothetical notions; and reminded them that, though in many most important respects, these works are now of little immediate value, as being imperfect and erroneous, they still remained a monument of the genius, and good principle, and sense of their great author, and a most valuable study for all practical and reflecting men, who regarded professional knowledge as a means rather than an end, and who desired chiefly to know a thing in order by its means to do some other thing. He then concluded by quoting and illustrating the following remark of Hartley Coleridge in his life of Dr Fothergill: "There are certain inward gifts, more akin to genius than talent, which make the physician prosper, and deserve to prosper; for medicine is not, like practical geometry or the doctrine of projectiles, an application of an abstract, demonstrable science, in which a certain result may be drawn from certain data, or in which the disturbing forces may be calculated with an approximation to exactness. It is a *tentative art*, to succeed in which demands a quickness of eye, thought, tact, and invention, which are not to be learned by study, nor, unless by connatural aptitude, to be acquired by experience: and it is the possession of this *sense*, exercised by patient observation, and fortified by a just reliance on the *vis medicatrix*, the self-adjusting tendency of nature, that constitutes the physician, as imagination constitutes the poet, and brings it to pass that sometimes an old apothecary, not far removed from an old woman, and whose ordinary conversation savours, it may be, largely of twaddle, who can seldom give a rational account of a case or its treatment, acquires, and justly, a reputation for infallibility, while men of talent and erudition are admired and neglected—the truth being, that there is a great deal that is *mysterious* in whatever is *practical*."

In conclusion, while he hoped he was not unobservant of, or ungrateful for the many new methods of investigating disease, and while he admitted the prodigious improvement in the art, as well as in the science of medicine in later years, he hoped he might be permitted to say, that all new good things were apt to bring with them some new evil things, or at least to displace or disparage some ancient good; and that thus the danger now-a-days lay in our forgetting, that, with the wonderful access of purely scientific truths which has signalized our age, and with all the advantages we enjoy of being taught *ab extra*—this would be of little avail in practice, unless we met it by a hearty *vis ab intra*, and exercised on every occasion that nicety and keenness of observation, that independence and honesty of judgment, and that intelligent self-reliance, which made Sydenham the able and useful member of his profession and of society which we know he was. That while information was good, was essential, *formation*, internal assimilation, appropriation, and application of outward knowledge, was not less so—and that we must never forget that our chief duty is with a craft rather than

a philosophy, with knowing what to do rather than being able to discourse learnedly about final causes, or to remember every thing every one else has known or said. And that in the words of Sydenham—“*Cognitio nostra in rerum cortice omnes ferme versatur, ac ad το 'οτι (sive quod res hoc modo se habeat) fere tantum assurgit, το διοτι (sive rerum causas) nullatenus attingit.*”

2. *Dr Skae* read a Memoir giving the Medical History and Statistics of the Royal Edinburgh Asylum during the past year; with notices of the more interesting cases which had presented themselves.

3. *Dr Hughes Bennett* exhibited the ileum, cœcum, and mesenteric glands of an individual who had died labouring under typhoid fever. The ileum contained numerous typhoid ulcerations, and the mesenteric glands were loaded with typhoid deposit. This was the first case of intestinal complication with fever which had occurred in Edinburgh during the present year, and he thought it worthy of observation, as it was about the same season that the disease first manifested itself last year.

MEETING IX.—*Wednesday April 19, 1848.*—DR COLDSTREAM in the Chair.

1. CASE OF OBLIQUE FRACTURE OF THE FEMUR IMMEDIATELY ABOVE THE CONDYLES.
BY JAMES SPENCE, ESQ.

Mr Spence was sent for on the night of the 9th July 1845 to visit Mr M—, who had been found lying insensible at the foot of a very steep stair, down which it was supposed he had fallen. On arriving at his house, he was found lying on a couch; the right thigh was fractured close above the knee-joint, and as the limb was imperfectly supported, the sharp point of the broken shaft of the femur was seen projecting, merely covered by the skin. There was great effusion around, and into the knee-joint, and the patella was found, after some manipulation, lying deeply seated towards the inner side of the projecting end of the shaft of the femur, and apparently locked between it and the condyles. By extension, the projecting portion was considerably reduced, and then the appearance of the limb when viewed laterally was very similar to dislocation of the tibia backwards. On further examination, I found there was fracture of the neck of the humerus and of the ribs on the same side, and several contused wounds of the head and face. After putting up the fracture of the humerus and bandaging the chest, the patient was placed in bed, and it was attempted to complete the reduction and co-aptation of the fractured femur. Extension was made on the leg by two gentlemen, whilst counter-extension was kept up by means of a folded shawl applied as a perineal band by another party; but this attempt was ineffectual, as the projecting shaft seemed locked by the position of the patella. As the projection was to the outer side of that bone, extension was next made in an oblique direction in the axis of the broken shaft of the femur, and it was endeavoured to disengage the bone from the muscles through which it had protruded, by bending the leg and thigh towards the pelvis; but these attempts were attended with but little further benefit to the position of the bone. The long splint was therefore applied to keep up what advantage had been gained by extension, and a large opiate, with occasional doses of camphor mixture, was ordered.

Next day, with Mr Syme in consultation, further reduction was again attempted by extension, but ineffectually; the splint was therefore again applied. The patient, who had been very excitable before the accident, became gradually worse, delirium set in, antimonials combined with opiates were given, but could not be continued owing to the state of the pulse: the camphor mixture and opiates somewhat relieved him and procured rest; but he gradually became worse and died on the 13th July, the fourth day after the injury.

With some difficulty permission was obtained to examine the broken thigh and take a cast of it; but no further examination was permitted. On dissecting off the skin, the sharp point of the broken shaft of the femur was seen to have perforated the cruræus and vastus externus muscles, appearing immediately on the outer edge of the tendon of the rectus, which it had also perforated;

it overlapped and pressed firmly upon the upper and outer edge of the patella, locking it against the condyles of the femur. The condyles and the lower fragment of the femur, together with the leg, were drawn backwards, upwards, and rather inwards; the sartorius and inner-hamstring muscles were very much relaxed. After taking the cast of the dissected limb, efforts were made to reduce the bone, but still in vain; the wound was then enlarged upwards in the axis of the bone, but without success, until the vastus was divided, together with a small part of the tendon of the rectus, in a transverse direction, when reduction was accomplished with the greatest ease. On this being done, the condyles separated, showing that they had been split up by the injury; and, on carrying the dissection further, the bone was found comminuted into numerous fragments.

Remarks.—The kind of fracture of which an example has been given is not of very frequent occurrence, and opportunities of examining the exact state of parts by dissection are very rare. Sir Astley Cooper in his work on fractures, has given the history of two cases, and states, that he considers it “a most formidable injury from its consequence on the future form and use of the limb; for it is liable to terminate most unfortunately by producing deformity, and by preventing the flexion of the knee-joint.” He also gives an account of the dissection of a case of this kind of fracture which he had an opportunity of seeing in a body brought into the dissecting-room, and of which a plate is given in the large edition of his work. All the cases mentioned by Sir Astley seem to have terminated unfavourably as regards the usefulness of the limb; and though he does not mention that there was any difficulty in reducing and replacing the bones, and seems rather to impute the bad result to the difficulty of keeping up extension, Mr Spence was of opinion that the ordinary long splint would keep up the extension sufficiently if the broken ends of the bone were fairly brought in apposition. From the state of parts found on dissection in the case related, as well as in the dissection given in Sir Astley’s work, there must, in most of these cases, be great difficulty in freeing the end of the shaft from the muscular fibres through which it has protruded. Although the projection may be considerably diminished by extension, there will, in cases where the bone has passed through the cruræus and vastus muscles, be a risk of muscular fibres intervening between the broken surfaces, and so preventing firm union. The position of the patella, also, firmly fixed to the inner side of the projecting end of the shaft, and locked between it and the condyloid portion of the femur, serves to check extension and prevent the lower broken portion being brought into a line with the shaft. From the ease with which the bone was reduced after death, when the fibres of the muscles surrounding it were divided transversely, although it had previously resisted strong efforts at extension, and free longitudinal incision; the author, in similar circumstances, taking into account the usual unfortunate termination of such cases, considered himself warranted in dividing freely the muscular tissues surrounding the projecting bone by means of subcutaneous incisions, as any additional risk from such a proceeding would be more than compensated for by the advantage obtained from its facilitating the reduction and coaptation of the broken bone. It will be noticed that in this case, when extension was effected, the condyloid portion of the femur was found split longitudinally. This must be of very frequent occurrence in such accidents when the bone is broken so low down as in the case just related, and when we consider the anatomical formation of the femur near the knee-joint. But it is a lesion which cannot be fairly ascertained so long as the other fracture is non-reduced, because the lower broken portion of the shaft presses the outer condyle against the inner, and so keeps them firmly locked together.

2.—ON THE PATHOLOGICAL NATURE OF BRIGHT’S DISEASE.

BY DR W. T. GAIRDNER.

The object of this paper (the details of which are comprised in the memoir
NEW SERIES.—NO. XXIII. MAY 1848.

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to be published by Dr Gairdner in this and the succeeding Numbers of the *Monthly Journal*) was to show, that the symptoms usually considered as indicating Bright's disease, are not found in connexion with one pathological lesion exclusively, but may be the result of several disorganising processes. The author described the pathological lesions of the kidney as being essentially the same as those of other organs, the differences being the result of the anatomical disposition of the gland. The deposition of fatty granules might occur in connexion either with acute or chronic forms of disease. Instances of its acute deposition are to be found in pneumonia, and other parenchymatous inflammations, where the so-called exudation corpuscles have exactly the same mode of origin from the epithelium of the lung, as the fatty cells in the parenchyma of the kidney. Neither could the granular or fatty exudation be considered as the proximate cause of albuminuria or dropsy; as it might occur to a considerable extent without either of these consequences. Albuminuria was generally connected with imperfect formation of the secreting cells, and very frequently with desquamation, as described by Dr Johnson, Mr Simon, and others. Dr Gairdner pointed out the analogy of this morbid process with the ordinary phenomena of mucous inflammations, as described by Henle. This he was disposed to consider as the initiatory process of Bright's disease; it might be succeeded by exudation, or by other pathological lesions, according to circumstances. The granulated kidney, the non-granulated (fatty) kidney, and a peculiar waxy degeneration, corresponding with the "marbled" kidney of Rayer, were described by the author as frequent in connexion with Bright's disease. The result of most of these forms of disease was atrophy and contraction, which occurred by the absorption of the exudation, and collapse and obliteration of the tubes.

Dr Hughes Bennett stated, that he had on several occasions examined the demonstrations of Dr Gairdner, and could vouch for the accuracy of the facts he had described. Some of those, more especially such as were explanatory of the changes occurring in the tubes after the evacuation of their fatty contents, were new, and constituted a valuable contribution to our knowledge of the morbid anatomy and pathology of renal disease. The observations of Dr Gairdner, as well as those of several German pathologists, and of Dr Johnson of London, as regards fatty degeneration of the organ, were illustrative of a series of changes which occurred in numerous glands, and exhibited the great importance of paying attention to accumulation of the fatty element as a cause of disease. There could be little doubt that exudation into the tubes was one of the primary changes in the kidney, whereby, as the author had explained, secondary effects were produced on the Malpighian bodies and the capillaries. It was in recent cases, more especially such as occurred during or subsequent to scarlatina, that fibrinous moulds of the tubes were most frequently found in the urine, which could only be regarded as the natural excretion of the morbid product. They were analogous to the fibrinous moulds of the bronchi, expectorated in pneumonia. Dr Bennett considered that the good effects of diuretics in certain cases of kidney disease, might be explained by the increased secretion of fluid favouring the expulsion of these moulds. In other cases, again (which further observations could only more particularly indicate), diaphoretics were of great service, owing apparently to the well-known sympathetic connexion existing between the skin and all mucous membranes, but especially those of the kidney.

Professor Simpson stated, that he was much interested in this subject from the frequency of albuminuria in pregnant cases. Disease of the kidney led to the absorption of urea, and a train of nervous symptoms of the utmost importance. He related several interesting cases of these, and especially one in which the urine remained albuminous for four months, and, after every thing had failed, was cured by diaphoretics. The discovery of the fatty nature of the disease was very important, and its frequent occurrence in cases of pregnancy might be explained by the circumstance, that milk was often secreted and circulated in the blood long before parturition.

Dr Paterson asked, whether the albumen found in the urine was secreted in the tubes, or was poured out from the blood, and how the hæmaturia, which was so common a symptom in Bright's disease, was occasioned?

Dr Gairdner said, in reference to the remark of *Dr Bennett*, that he had thought it right to be very cautious in drawing therapeutical inferences from a pathology which was still in its infancy. It had been attempted by some writers on this subject, to deter the profession from the use of diuretics in the desquamative forms of the lesion; whereas these remedies were undoubtedly found useful in some cases, in which he believed their action was precisely similar to that of expectorants in catarrh and bronchitis. He had seen comparatively few cases of the temporary albuminuria described by *Dr Simpson*, owing to his observations having been made in hospital practice. He considered the albuminuria undoubtedly a morbid secretion, and thought that the hæmaturia was generally seen where there was little of the fatty exudation.

EDINBURGH OBSTETRIC SOCIETY.

SESSION VII.

MEETING III.—*February 9, 1848.* *DR SIMPSON*, President, in the Chair.

A FEW EXPERIMENTS WITH CHLOROFORM. BY *DR T. M. LEE*.

THE author began by alluding to the never-failing powers of annulling pain which this agent possesses, and to its complete safety when administered by those who understood its action and are skilled in the human frame; and afterwards related a case of labour in which he had recently given it with very gratifying effects.

But the object of his paper was to draw the attention of the Society to its property of securing rest, a point of the utmost importance in the treatment of almost all diseases. And he gave a detailed account of six cases in which he had administered it with this object in view, in every one of which its application was followed by the most satisfactory results. To wit, a case of acute rheumatism complicated with bronchitis; one of acute mania; one of urgent dyspnoea, depending on a combination of asthma with permanent contraction of both sides of the chest from pleurisy; one of aggravated hysteria; one of infantile remittent or hectic fever; and one of typhus. The ages of the patients varied from one to forty years; and the sleep which each dose of the chloroform induced was natural and refreshing, and of several hours' duration.

He had also witnessed its effects in many other cases, both in healthy individuals, and when given to prevent or remove pain; and stated that, while he had never known it to fail in producing almost immediate insensibility, he had not seen any disagreeable consequences from its use; except in a case of tooth-drawing, in which the power of expelling a few drops of blood that had trickled down into the wind-pipe was lost for a few seconds, causing not a little alarm among the bystanders. In two other cases also vomiting had taken place while the patients were under its influence.

Dr Lee concluded by describing a *post-mortem* examination he had made of a young rabbit intentionally killed by chloroform inhalation. The animal was apparently in perfect health at the time, but died in a few seconds after beginning to breathe the vapour. The brain, spinal cord, and windpipe, and the thoracic and abdominal viscera, were in the highest degree healthy-like; with the exception that there were a few hydatids adhering to the peritoneum, which, however, had not caused any apparent alteration in the membrane itself. The lungs were collapsed, spongy, and of a pale pink colour. The heart exhibited no appearance of disease. The right auricle and ventricle contained a small quantity of clotted blood. There was no congestion in any part of the body.

Dr Simpson believed, that when chloroform is given in order to procure sleep it should be given in a full dose at first, and that a continued action

should be kept up by subsequently giving the patient a few inhalations of it from time to time, when he usually passes from the anæsthetic sleep into a natural sleep.

Several members of the Society mentioned a variety of cases in which they had successfully employed chloroform to relieve pain. Dr Purdie had used it in two cases of severe vomiting, with cramp of the stomach. In both cases it removed the pain and vomiting instantly and permanently. One of the patients was a woman above seventy years of age.

CASES OF SECONDARY FŒTUS.

Dr Keith read a notice of two cases of secondary fœtus observed by Dr Christie, Dundee. Dr Christie was summoned to a woman in labour by the attendant midwife, who had got alarmed from the presentation being unusual. He found the breech and one foot presenting, and with a little assistance a healthy child was soon born. An arm was now found presenting—and immediately the head followed of a fœtus which proved to be very putrid, and apparently of about seven months. The woman calculated on being at her full time, and the other child was full grown. There was but one placenta; but the portion of it that belonged to the blighted child was decayed-looking and easily lacerable—the other portion of it was quite natural and healthy. A similar case happened to Dr Christie several years ago, but in that case there was a double placenta.

Dr Simpson showed some specimens of “secondary fœtuses” from the University Museum, and stated some general propositions regarding them.

OBSERVATIONS ON THE INFLUENCE OF THE DEATH OF THE FŒTUS IN RELATION TO ITS RETENTION IN, OR EXPULSION FROM, THE UTERUS.

The general law which regulated the occurrence of labour in relation to the death of the fœtus, was well known to be this—1. That usually, in from one to three weeks after the fœtus died, uterine contractions supervened, and effected its expulsion. But to this very general law there were various exceptions. For, 2. If the embryo died in early embryonic life, and the fœtal appendages continued to live and vegetate, expulsion might not supervene for months. He showed a case of hydatiginous ovum, where the embryo was not larger than an embryo of the sixth week, but the placenta, or rather the chorion, was the seat of hydatiginous hypertrophy and degeneration; and the mother (a patient of Mr Girdwood's of Falkirk) calculated that she had passed the usual term of utero-gestation, not having menstruated for eleven months previously to the expulsion of this diseased ovum. 3. When the fœtus dies from the third month onwards (in consequence either of disease in its own organization, in its umbilical cord, or in its placenta), and a second twin living fœtus exists at the same time in utero, and this second fœtus continue to grow and keep up a correspondence of development between the organ and its contents, the dead and undeveloped twin may be retained up to the full term of pregnancy, and be then born along with the other living and full-sized child. 4. When the dead fœtus is thus retained, it is preserved free from the decomposition usually following death by all access of air to it being prevented. Sometimes it retains its usually rounded appearance and form if it continues to be surrounded by a sufficient quantity of liquor amnii; but in other cases where this protecting medium of liquor amnii is defective, the fœtus becomes gradually more and more squeezed between two forces,—viz. the parietes of the uterus on one side of it, and the other living twin or its membranes on the opposite side; and at last, when born, it is found compressed and *flattened* in form. Two such flattened fœtuses are in the University Museum; and many such cases are on record. 5. The birth of such undeveloped dead twins had sometimes given rise to most groundless and erroneous ideas of the existence of superfetation. 6. Occasionally when one of twins died early in pregnancy, it was after a time expelled (when it happened to be situated near or

over the os uteri); afterwards the uterus closed, and pregnancy went on to the full time with the remaining living child. He mentioned a case of a lady aborting of a foetus about the third month, going on in pregnancy to the full time, and then being delivered of twins; having originally conceived of triplets. 7, This last circumstance evidently led to the practical deduction,—that when a dead foetus in its envelopes was expelled during the currency of pregnancy, and the uterus notwithstanding still remained large and apparently distended, its further contents should not be in any way interfered with; but rest and other means employed to avert the excitement of any additional uterine contraction, under the hope that a living twin might still be retained, and be carried onwards to the full term of utero-gestation.

INTRA-UTERINE PERITONITIS IN THE FŒTUS.

Dr Simpson showed the body of a new-born infant which had died a few days before birth of acute Peritonitis, as evidenced by quantities of coagulable lymph effused upon various parts of the surface of the peritoneum, and more particularly on the surfaces of the spleen and liver. *Dr Simpson* stated, that, 1st, Acute and fatal Peritonitis appeared to be a very common inflammatory disease in the foetus in the latter months of utero-gestation. 2d, A large number of foetuses dying in the seventh and eighth month of utero-gestation, presented, as he had found on dissection, well-marked anatomical evidence of it, in the presence of effusions of coagulable lymph, adhesions between the folds of intestines, pus, &c. 3d, The child was sometimes, though rarely, born alive, and affected with it. 4th, Far more commonly the child is born dead, and the previous history of the mother shows that it had perished from one to three weeks before its expulsion, its movements having ceased about that time. 5th, Before the child's movements entirely ceased, the mother very generally remarks that its movements are morbid and excessive for fifty or sixty hours previously—probably during the currency of the fatal disease. 6th, Peritonitis is occasionally apt to recur in successive children in the same mother, and seems in some a result and remnant of the syphilitic poison in the parents. 7th, But in most cases its occurrence is independent of syphilis, and occasionally it will not attack successive children in the same mother, or even both children in cases of twins. In an essay on the disease, published some years ago in the *Edinburgh Medical and Surgical Journal*, Vol. I. p. 392, *Dr Simpson* had described a case of twins, in which one was born living and healthy; the other was dead, and within the abdomen were found all the usual appearances following intra-uterine peritonitis. Whilst intra-uterine Peritonitis was very common, intra-uterine Pleuritis was very rare; *Dr Simpson* had only seen two well-marked cases of it in the foetus.

DEATH OF DR MAKELLAR FROM FEVER.

On two former occasions we have pointed out the injury which medical men inflict upon themselves, and upon the profession, by gratuitously attending the poor. The efforts made by our Irish brethren, and especially by the heads of the profession in that part of the country, for the purpose of obtaining proper remuneration for important public services, in our opinion does them great honour, and we sincerely hope they may be crowned with success. With us, however, so great seems to be the apathy of those concerned, and so little interest do the elevated members of the profession take in the welfare of their less favoured brethren, that the greatest abuses in public institutions, and the utmost sacrifices on the part of medical practitioners, are regarded with unconcern. Scotland wants men, who, like Graves and Stokes in Ireland, will lay aside their private interests, and nobly combat for the rights of the profession, with all the advantages that their elevated position and talents alone can give. In the mean time, events occur which are well calculated to excite the attention and serious reflection of many medical men, but especially of the Fellows of the

Royal College of Physicians in Edinburgh. During the various fever epidemics which have attacked the city, the members of that body have voluntarily and gratuitously attended the fever wards and sheds of the Royal Infirmary. In doing this, many were doubtless actuated by the hope that such services would constitute a claim upon the managers for the office of physician. Repeated acts of injustice, however, have opened the eyes of the profession to the fact, that no services or sacrifices on the part of medical men, can, without private interest, acquire for them the consideration of those in power. It happened in consequence that, during the late epidemic, the managers were under the necessity of privately requesting the assistance of certain Fellows of the College to do that duty which has hitherto been freely and cheerfully performed. Among others, Dr Makellar's services were requested, and they were given. This gentleman had practised his profession with credit, for many years, in some of the mining districts of Scotland, and had resided latterly, in ease and comfort, with his family in Edinburgh. He must be well known to the readers of the *Monthly Journal*, through the valuable series of papers he wrote on the black phthisis of colliers, inserted in our fifth volume, and, we may add, that he was highly respected by all who knew him. He commenced his duties as an extraordinary physician¹ last November, and for a long time his friends thought him not susceptible to infection; but at the commencement of last April he caught the disease, and died of fever, complicated with pneumonia, shortly after.

It should be remembered, that every one who does duty in the Edinburgh Infirmary, is almost as a matter of certainty attacked by fever. Many have narrowly escaped death; while some, and the case of Dr Makellar is not the least distressing, have thereby sacrificed their lives. And for what? No sooner are their remains cold than their services are forgotten, and the same selfish and ungrateful policy towards members of our profession continues in force. A feeling of grief, mingled perhaps with an idea of the folly of the parent, is all that is left to the orphans our deceased medical brother has left behind him.

Seeing, then, the risks to which the medical officers are exposed, it appears to us certain, that they have the first claim upon the funds. Why should the Chaplain, the Superintendent, the Clerk, and the Matron, who are exposed to little danger, receive salaries, whilst the Physician, especially the extraordinary one, derives no advantage, either directly or indirectly, from the performance of his onerous duties? We repeat, for the third time, that the *fault is with the profession*, and that the *power of making a proper arrangement lies entirely in the hands of the medical man*. Who the next candidate for disease and death may be, we do not know; but we trust that the day is not distant when the dangers, if not the interests, of medical men, may induce them to demand from the authorities the remuneration to which their services are entitled.

THE STUDENTS OF UNIVERSITY COLLEGE AND MR SYME.

A DEPUTATION of gentlemen, students of medicine in University College, lately waited upon Mr Syme, at his residence in Bruton Street, for the purpose of presenting to that gentleman a congratulatory address on the occasion of his settlement among them as Professor of Clinical Surgery. The following is a copy of the address, and also of Mr Syme's answer:—

Copy of the Congratulatory Memorial to Mr Syme.

"Sir,—We, the undersigned students of medicine in University College, embrace with pleasure an opportunity of presenting to you our heartiest con-

¹ The extraordinary physician is only permitted to do duty in the fever wards or sheds. He has no laws for general cases, wherewith to relieve the intolerable dullness of always seeing the same thing. By a recent regulation of the managers also, he is not qualified to assist the clinical professors, although his services may be especially requested by them.

gratulations on your accession to the Chair of Clinical Surgery, lately rendered vacant by the death of our beloved and revered master, Liston.

"Such an expression of our feelings towards you, sir, might at first sight be deemed somewhat premature; but this objection falls to the ground, when we state that we feel ourselves called upon to obviate any erroneous impression as to the spirit in which we receive you, which might arise in the mind of those who judge of your connexion with us through the media of certain organs of the press only.

"But apart from this, Sir, we sincerely welcome you among us; for the reputation you have so justly earned both as a surgeon and teacher of surgery, not only entitles you to our highest respect and warmest admiration, but becomes, at the same time, a subject of self-gratulation now that we can look upon you as our teacher.

"The death of Mr Liston had thrown a gloom over our studies in surgery, which threatened to be wellnigh permanent; but your appearance among us, Sir, has done much, and will do more, towards dispelling that gloom, since, while his death will be long felt by us, we have the comforting assurance of believing that you are he whom Mr Liston would have nominated as his successor.

"That you may long continue in the office you now hold, to carry out the work which Mr Liston began among us, and that you may ever have a satisfaction in that work commensurate with your abilities for the discharge of its important duties, are the desires and sincerest wishes of, Sir,—Your most respectful pupils."

[Here follow the signatures of upwards of 180 gentlemen in connexion with the Medical School of University College.]

Copy of Mr Syme's Answer to the Memorial presented by the Students of Medicine in University College.

"Gentlemen,—I receive this expression of the feeling entertained towards me by the students of medicine of University College, with great pleasure. Sentiments of mutual respect and regard between a teacher and his pupils not only increase the comfort, but greatly promote the benefit derived from their connexion. Since entering upon the discharge of my duties at the hospital, I have witnessed with extreme satisfaction the deportment of the gentlemen who pursue their studies there. The diligence and intelligence which they display in the acquisition of professional knowledge, afford me the strongest encouragement to exert every effort in my power for their instruction; and I venture to hope that the confidence they have thus early been led to place in me, will not be diminished in the course of their longer acquaintance. My old connexion and intimate friendship with Mr Liston, enable me to appreciate the loss they have sustained by his sudden and premature removal from the field of honour and usefulness which he so worthily occupied. It would be difficult, I fear impossible, to supply this loss; but, so far as it may be in my power, I shall constantly and earnestly endeavour to prevent their interests from suffering by the sad event that deprived them of so able a teacher, and the profession of so bright an ornament.

(Signed) "JAMES SYME.

"22, Bruton Street, March 1848."

THE ECLECTIC PRACTITIONERS, OR THE SO-CALLED PRACTICAL MEN.

THERE are medical men in high positions, greatly occupied with numerous patients, who, from a want of study, of intelligence, or of time, from a natural indolence, or from being too old to master recent important improvements, affect a supreme disdain for every thing that concerns doctrine or generalization, either physiological or philosophical. They call themselves PRACTICAL men, and speak ironically of *theorists—men of science or of the closet*, such who labour most for the advancement of medical science, and whose knowledge crushes and confounds them. These so-called practical men are those who have no doctrine and no general principles, who gather together ready made

formulae and isolated cases, without any kind of scientific discernment. The only medicine they study is that contained in small books of prescriptions, published in 18mo or 24mo, which they carry in their pocket, and know by heart. We have frequently had occasion to remark that a practical man, that is, a man who boasts of knowing nothing of scientific medicine, is a medical machine inferior intellectually to a master mason, a locksmith, or a cabinet-maker, for these have principles and a sort of doctrine which they apply in their business. They were appreciated in a like manner by a learned individual whose authority no one could doubt, and who said,—“The true eclectic works without conviction, without principle, without idea. He is continually enlarging his circle, in order to enclose within it facts of the most contradictory nature—they sacrifice in a sort to every god, and create a kind of scientific pantheism, not less fatal to true science than pantheism properly so-called is to true religion.”—*Professor Cruveilhier's Address to the Anatomical Society, 1845.*

ISOPATHY.

A new medical doctrine has appeared on the horizon, and it is Germany again, *alma parens rerum*, which enriches the world with this benefit. Homeopathy, magnetism, and phrenology salute their new sister under the harmonious name of Isopathy. Dr Hermann is the prophet of this doctrine, which is based on the following principle:—Every diseased organ has its remedy in the same organ—thus, if you have disease of the liver, eat liver; if a headache, eat brain; if you suffer in the bladder or kidneys, nourish yourself on bladder and kidneys; if the testicle be disordered, eat testicle. As the organs may not appear very tempting to certain squeamish persons, M. Hermann has made tinctures of them, which his patients take in spoonfuls, under the scientific names of stomachine, cystine, testiculine, unbria, &c. The work published at Augsburg contains fifty cases of radical cures. Go, young doctrine, increase and prosper—thou wilt doubtless be called to high destinies!—*Medical Gazette, March 29, 1848.*

BOOKS RECEIVED.

1. Portraits of Diseases of the Skin. By Erasmus Wilson, F.R.S. Fasciculi I. and II. London. Folio. 1848.
2. Principles of Medicine, comprising General Pathology and Therapeutics, &c. By Charles J. B. Williams, M.D., F.R.S., &c. Second Edition. London. 8vo. 1848.
3. Practical Observations on certain Diseases of the Chest, and on the Principles of Auscultation. By Peyton Blakiston, M.D., F.R.S., &c. London. 8vo. 1848.
4. The Philosophy of Animated Nature; or, the Laws and Action of the Nervous System. By Calvert Holland, M.D., &c. London. 8vo. 1848.
5. A Treatise on Diet and Regimen. By W. H. Robertson, M.D., &c. Part V.
6. A Discourse on the Asiatic Cholera and its relations to some other Epidemics, &c. By Thomas Henry Starr, M.D., &c. London. 8vo. 1848.
7. Chloroform in the Practice of Midwifery. By Edward W. Murphy, A.M., M.D., &c. London. 8vo. 1848.
8. The Advantages of Ether and Chloroform in Operative Surgery. By T. B. Curling, &c. London. 8vo. 1848.
9. On Indigestion: its Pathology; and Treatment by the Local Application of Uniform and Continuous Heat and Moisture. By James Arnott, M.D., &c. London. 8vo. 1848.
10. Medicine, an Art, and its Truths to be Attained—an Address, &c. By Thomas Shapter, M.D., &c. London. 8vo. 1848.
11. Report on the Mortality of Edinburgh and Leith for the Year 1847. By James Stark, M.D., &c. Edinburgh. 8vo. 1848.
12. Historical and Statistical Sketch of the Progress of Epidemic Fever in Glasgow during the Year 1847. By R. S. Orr, M.D., &c. 8vo.

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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Ununited Fracture treated by Subcutaneous Puncture.*

By JAMES MILLER, F.R.S.E., Professor of Surgery in the University of Edinburgh, Surgeon to the Royal Infirmary, &c. &c.

(Read to the Medico-Chirurgical Society of Edinburgh, May 17, 1848.)

EVER and anon a case of ununited fracture obtrudes itself into the practice of the surgeon, and sometimes baffles all his efforts of cure. As to the best means of treatment, there seems to be still great difference of opinion. To contribute something towards the adjusting of that question, is the object of the following observations.

To undo the apparatus of a fractured limb, and to find the solution of continuity in the bone still unrestored, at the end of four, five, six, seven, or eight weeks, is no demonstration of the expected union having altogether failed. One part of the reparatory process has proved defective, the formation of the *provisional* callus; but it may be that the other part, the formation of the *definitive* callus, is in its ordinary progress; and, if undisturbed by movement of the limb, this may be completed in the ordinary time, counting months for weeks,—at the end of four, five, or six. The provisional callus, in truth, is not essential to osseous reunion. It is a ferrule or clasp, tightly embracing the broken part, rendering it immoveable, and seeming to restore its actual continuity; just as a like binding agent may give continuity to two pieces of wood, and make them as one. But, so far as the binding agent is itself concerned, there is as little actual restoration of continuity of texture in the bone as in the birch. The provisional callus just enacts the part of a steady splint, until the process of true consolidation has been completed by elaboration of the definitive callus, whereby there is, as it were, an interweaving of texture between the broken ends. It

takes some time to construct this splint, and to apply it with due tightness; four, six, or eight weeks, as may be. During its construction, it is necessary to steady the parts by external means; and that is the province of the surgeon. After it has become firm in itself, and tightly applied to the bone, then it alone is capable of restraining motion, so as to permit true consolidation of the broken ends; and the surgeon's splints may be now taken away. In short, there are three distinct means towards the final cure;—1. Surgical splints, to steady the parts until provisional callus is formed and completed; 2. This provisional callus, or nature's splint, to secure perfect immunity from motion, until the definitive callus has been constructed; and, 3. This definitive callus, by whose gradual elaboration and modification true continuity of both texture and tissue is ultimately restored. When No. 2 is finished, No. 1 is useless, and is taken away by the surgeon's hands; when No. 3 is complete, No. 2 is removed by the busy labour of absorbents; No. 3 remains, but is ultimately much modified by interstitial absorption. The presence of No. 1 is favourable to the occurrence of No. 2; and the existence of No. 2 as strongly favours the formation of No. 3. But, of the series, the only one which is truly essential is the last. Bones knit by provisional callus, though no surgeon is by, and no splint is applied—though not so well; and they may also unite—still not so well, yet perhaps not much after the ordinary period—though the provisional callus may have proved either faulty or altogether defective. That is, union *may* take place, independently of the splints both of the surgeon and of nature. Flat bones, such as the cranium, when fractured, unite mainly, if not solely, by definitive callus; and fracture of the neck of the femur, within the capsule, if it unite at all, can do so in no other way. The process of union, no doubt, is unspeakably favoured by the presence of both splints in due succession, first the surgeon's, and then that of nature; but still it *may* be completed, independently of one or other of them, or even of both.

Supposing then, that on removal of our splints, at the end of the accustomed period of probation, we find the broken ends still moveable on each other, it is manifestly our duty to reapply the retentive apparatus with still greater care than formerly, and to keep it so applied for a very considerably greater period than was at first contemplated, it having now a new duty to perform; not to keep the parts steady till provisional callus clasp them tight; but to take the place of this callus, which has failed, and to keep the parts steady for a longer period than before, so that the definitive callus, now supposed to be in progress, may duly advance to completion. And not until a reasonable period of probation—say four, five, or six months—for the construction of this, the essential part of the uniting process, shall have passed away, does the surgeon abandon either the careful use of his simple retentive apparatus, or the hope of cure?

In regard to this form of “ununited fracture,” there need be no two opinions as to the right mode of treatment. To put up the limb

afresh, to keep the parts immoveable, and to maintain the general health and powers of system in as vigorous a condition as possible.

But when, at the end of four, five, six months, or more, we find the limb still loose and moveable at the fractured part, it is a sign that the ordinary process of reunion has failed in all its parts; the definitive as well as the provisional callus has proved abortive. And the same conclusion is forced upon us in cases of an earlier date—six or eight weeks only, it may be, after the accident—in which mobility is great, in which a space, defective in every thing like restorative means, can be felt between the ends of the bones, and in which these can be plainly felt blunt, tapering, and rounded. In such cases it is, that difference of opinion prevails as to best modes of treatment, and latitude exists as to their selection.

It naturally occurs to one, that it would be desirable to restore something like the state of matters which exists at the first, immediately after the injury has been received; so that we may start again in treatment *de novo*. Accordingly, among other plans, it has been proposed to expose the part by incision, to saw off the ends of the bone, and then, closing the wound, to re-adjust all carefully. Thus is a recent fracture re-established no doubt, but it is a compound one; and, being so, it is by no means an improvement on the original casualty. The proceeding proved unsatisfactory in practice, and may be said to be now abandoned.

When the principle of “subcutaneous incision” came into use, the idea struck me that this important addition to surgery might be made available towards the remedy of ununited fracture; and accordingly I proposed (p. 692, *Principles of Surgery*, 1844) “that a strong needle, having been passed obliquely down to the part, should have its edge freely moved about in all directions, so as to cut up the ligamentous bond of union, as well as the dense investment of the ends of the bones; the needle being then carefully withdrawn, and the puncture covered by isinglass plaster. The parts will probably be reduced to a state very similar to what attends on ordinary fracture at the first. A pouch of blood will form; the blood will be absorbed; fibrin will take its place; inflammation being absent, the plasma will become organized, and probably form an excellent imitation of the ordinary provisional callus; while, at the same time, secretion and organization may advance from the ends of the bone; and consolidation, as by definitive callus, be completed.”

The connecting materials of the “false joint” are disrupted and excited, not destroyed. They are valuable towards the formation of bone, when brought into and maintained in a state of moderate vascular excitement. “A state of active hyperæmia generally precedes the osseous transformation of the fibrous, cartilaginous, and fibro-cartilaginous tissues. M. Rayer observed, that when he excited an artificial irritation in the fibro-cartilage of a rabbit’s ear, the part was at first softened; a yellow matter was next deposited in its texture; and, finally, a calcareous deposit was formed, and a true

ossification produced. M. Cruveilhier likewise observed different portions of periosteum, ligaments, and cartilages, pass into the osseous or ossiform state, under the influence of different stimulating applications."—*Andral*.

I have not had the opportunity I could have desired of reducing the theory into practice, but few examples of ununited fracture having fallen to my professional lot. I proceed, however, to lay a brief notice of a few cases which bear upon the subject before my brethren, not to prove the efficiency of the proposal, but rather in the hope that others may be induced to lend their aid in bringing it to the test of experience :—

CASE I.—*Ruptured Tendo-Achillis—Subcutaneous Puncture—Cure.*

Catharine Miller, æt. forty, admitted into the Royal Infirmary June 11, 1845. She is found to have sustained rupture of the tendo-Achillis, about an inch and a half above its insertion into the calcaneum, and there is an interval of nearly two inches between the two portions. In walking she drags the foot after her, and has little or no power of extending the ankle-joint. Her narrative is, that about six months ago she stumbled, and, in endeavouring to recover herself, the tendon snapped, and she fell forwards. Treatment had proved ineffectual in restoring the separated parts.

June 14.—To-day, by means of subcutaneous puncture, the ends of the tendon and the intervening parts were lacerated; and the limb was secured, with extension of the ankle and flexion of the knee.

July 1.—There seems to be some effusion, but the space remains unsatisfactorily occupied.

July 4.—The puncture was repeated, and the needle used more freely.

July 29.—The space is now occupied by plasma, and the patient says that she feels the part "quite different."

August 20.—Union seems tolerably firm; and accordingly walking is to be cautiously practised, with a high-heeled shoe. The heel to be of cork, three inches high, and a slice to be removed every second day.

August 30.—The heel has been reduced by one half; and walking is effected with considerable ease.

September 8.—Dismissed cured.

In November the patient was seen on the street, walking without the smallest lameness or difficulty.

The uniting plasma was here, doubtless, due to the puncture; and this mode of obtaining it is surely preferable to the operations with knife and suture, which have been practised for the same end. The first application of the needle, on June 14, was made with much caution, and gently; its effect was nothing, or next to nothing; and it may be thrown out of consideration in regard to the ultimate result. The commencement of the cure is to be dated from the second puncture, which was practised with sufficient freedom, July 4. The parts were thereby placed in a condition very similar to that which immediately followed the original injury; and, after the lapse of the ordinary term of cure, we find the report stating :—"August 20.—Union seems tolerably firm; walking begun to be cautiously practised."

So far as the cure of ununited rupture of tendon is concerned, this case has a plain reference, and must be viewed favourably. It

has also a strong bearing, by analogy, on ununited fracture—so plain as not to require illustration.

CASE II.—*Ununited Fracture of Jaw—Subcutaneous Puncture—Amendment—Death from another cause.*

A man about thirty-five, from Orkney or Shetland, was admitted under my care in the Royal Infirmary, in the same year as the former patient. Unfortunately I am not able to lay my hand on the notes of his case. The leading points, however, are the following :—He had sustained fracture of the lower jaw at two points ; near the symphysis and at the angle. The anterior fracture had united in the ordinary time and way. The posterior was still moveable ; and on that account he sought the aid of the hospital. There was also slight overlapping of the fractured ends. I punctured the part freely from the mouth, and, having readjusted the bone as well as I could, applied a tightly-fitting pasteboard splint, retaining it by bandaging. At the end of ten days the parts were decidedly firmer ; the patient described his sensations in the jaw as much more satisfactory ; and I had become very hopeful of a successful issue. Some days later all was still well and promising. Then, however, the poor fellow was seized with virulent small-pox, and died. Unfortunately, an examination of the parts after death was not obtained.

CASE III.—*Ununited Fracture of the Tibia—Subcutaneous Puncture—Cure.*

Archibald Mcintosh, aged thirty-four, of the Preventive Service, was admitted into the Royal Infirmary October 2, 1846. About six weeks previously his left leg had been broken ; the tibia and fibula at corresponding points in the lower third of the leg, and the fibula also about two inches below its head. The limb had been duly treated with splints ; but, on examination, while the fibula seemed firm throughout its whole extent, the tibia was found moveable at the fractured part, with the upper fragment projecting slightly to the inner side.

On the day after admission, the ends of the tibia were freely stirred up with the needle, introduced at a fresh point for each fragment ; a compress was applied over the punctures ; and the limb was put up in starched bandages, with a pasteboard splint on the outer side.

On the 7th of October he was seized with a dysenteric attack, but had recovered on the 14th. On that day a second splint was applied on the inside of the limb. On the 20th he “states that he feels more strength in the limb.”

On the 28th he felt certain that the fracture was tolerably firm, and was extremely desirous to return home. He was accordingly dismissed, with directions to come back in three weeks. The original bandaging had never required to be undone.

On November 18th he again presented himself at the hospital ; and, on the bandages being removed, the fracture was found completely consolidated. As a precaution, however, light retentive apparatus was re-applied, with a recommendation that he should wear it for a fortnight or three weeks longer. This he promised to do.

From my friend, Dr Greig, I lately learned that the cure had proved quite satisfactory, and that the patient soon came to walk on the limb very stoutly. Perhaps, however, in one sense, it had been better otherwise ; for, shortly after having resumed his duty, his walking led him, on a dark night, along the brow of a precipice ; he missed his footing, fell, and was killed on the spot.

This case, I am aware, is not conclusive as to the efficacy of the puncture ; for, seeing that at the time of admission only about six weeks had elapsed since the occurrence of the fracture, union might have taken place, as formerly explained, under the use of the splints

and starched bandages alone. So far as it goes, however, the inference is in favour of the practice.

CASE IV.—Compound Fracture of the Humerus—Loss of several inches of the Bone—Non-union—Subcutaneous Puncture—Still under Treatment.

Robert Anderson, aged twenty-three, sustained a severe compound fracture of the right humerus, at its upper third, in May 1846. He was not under my care at that time. He stated that the bone was much injured and protruding, and that at least three inches of it, including the entire thickness, was sawn off at the time of reduction; no union occurred; and when he presented himself to me, on the 23d of September following, the limb was quite useless, moveable as a flail, and a large unoccupied space could be felt between the ends of the bone, these latter feeling attenuated, and rounded off at their extremities.

Here there was not only a want of union between fractured ends, but also a vast deficiency of space to fill up; a very unpromising state of matters under any treatment. However, though far from sanguine of success, I resolved to practise the subcutaneous puncture; the patient *might* be benefited thereby, and could not be made worse; the practice, by failure, would not be disgraced, and even a partial success would tell greatly to its credit. The needle was used, and splints carefully applied; after some weeks, puncturing was repeated, and the patient sent home. At long intervals he has come back, and had the operation repeated four or five times in all.

On the first use of the needle, I had a difficulty in bringing the instrument's point in contact with any thing like bone, even at the extremities of the shaft; and the intermediate space certainly did not contain a particle of earthy matter. On the next introduction, the ends of the bones were more readily and distinctly rubbed against; and, in the intermediate space, spicula of earthy matter jarred plainly against the instrument. On each successive introduction, the presence of bone, in process of formation, became more and more distinct. On the last occasion the needle had not to seek for bone, as it were; but at once came in contact with it.

He is now in the country. On his return I shall advise him to remain in the hospital, with the double view of repeating the puncturing at much shorter intervals, and securing a more thorough immunity from motion in the limb than could otherwise be obtained. I think that hitherto the stirring up has been performed too seldom, and that the chance of procuring ossification will be materially strengthened by maintaining a more constant excitement in the parts.

Some time before his death, Mr Liston wrote me that he had a case of ununited fracture under his charge in the hospital, and that he thought of employing the treatment by subcutaneous puncture. He wished for further information on the subject than was to be obtained in its first announcement. I gave him what I could; and urged him to lend his powerful aid to bring the matter to a practical bearing. His ever-to-be lamented death proved a sad interruption. But through the kindness of his colleague, Mr Quain, I have been furnished with the following report of the case, written by Mr Clover, present house-surgeon of University College Hospital. The patient was operated on by Mr Liston; he was afterwards attended by Mr Morton; and, still in hospital, he is now, I believe, under the charge of Mr Syme. The practice seems to have been entirely successful; and had it not been for the unfortunate fall, by this time, in all probability, the patient had been walking stoutly on the limb.

CASE V.—Ununited Fracture of Femur—Subcutaneous Puncture—Union—Fracture reproduced by fresh Injury.

William Duckmanton, æt. forty-eight, a bricklayer of stout conformation and full stature. Has enjoyed good health, and always had enough food. When at work, sometimes drinks six or eight pints of beer, besides some gin.

July 17, 1847.—Whilst walking on some wet boards his feet slipped, and he fell on his left side and broke the left femur. He was conveyed to St Bartholomew's Hospital, and a long splint applied (reaching from the axilla to beyond the foot). At the end of a month the limb was examined—it was not firmly united, and the foot turned inwards, so that when the splint was reapplied some force was used in order to correct this defect. At the end of nine weeks there was still found some movement at the seat of fracture. Short splints (the length of the thigh only) were now used, but the flexibility of the thigh increased.

October 8, 1847.—He was admitted at University College Hospital. The left leg two and a half inches shorter than the right. At the seat of fracture there is a projection at the outer side of the limb; and, when he attempts to raise it from the bed, an angle is formed by the upper fragment, and the heel rests on the bed.

October 24.—Fourteen weeks from the occurrence of the accident. The patient being made insensible by the inhalation of ether, a narrow knife, sharp only for one inch from the point, was passed for two inches beneath the integuments, and then made to divide the substance which connected the fragments. The knife was directed from below upwards. The limb could now be extended to the proper length. A long splint was applied, retained by a starch bandage, and over this a wooden splint was applied to the inner part of the thigh, and retained by a tourniquet kept constantly tight. The limb was examined at intervals, and the union became gradually firmer.

January 21, 1848.—(Three months after the operation). Union was thought sufficiently firm to discontinue the long splint. Short pasteboard splints were now applied instead. There was a perceptible projection of the upper fragment in front of the lower, some thickening around the fracture, and no motion was perceptible between the fragments. The opening made in October has never quite closed. Phosphate of lime was given in doses of twelve grains thrice daily in water. He was allowed to leave his bed, and moved about the ward with crutches. There was no shortening, eversion, or inversion of the foot. He could not support himself on the injured limb, but could place it evenly on the ground.

February 16.—(Four months after operation.)—Whilst moving about the ward his crutch slipped, and he fell down. The femur broke at the same point as before—the fracture was not compound at first, but in the course of a few days it became so, by the sloughing of the integuments, which were injured by the fall. A long splint was again used and retained by a sheet instead of a roller, as before,—in a few days, a pasteboard splint was applied to the outer and inner side of the limb, and retained by looped bandages, which were changed daily in order to remove the discharge, which usually amounted to nearly half an ounce. The upper fragment could be seen from the wound on the separation of the slough, and it was denuded for nearly half an inch in length over the whole circumference.

March 27.—There has been less discharge during the past week; a groove can be felt behind the necrosed portion of bone; an abscess is threatening—behind the trochanter major. There is union between the pieces of bone by thickening and consolidation of the soft parts, but motion between the fragments is quite evident.

As already stated, I am aware that these cases bring no conclusive evidence in favour of the practice in question. They have done

enough, and all that is expected of them, if they persuade any of my professional brethren that there is "something in it," and lead them to lend their aid in bringing that to the decision of practical experience.

It is surely better—though somewhat like—the practice of John Hunter; whose treatment of an ununited fracture of the humerus, Mr Samuel Cooper tells us, was as follows:—"There was an artificial joint, and he made an incision into it; and then, having introduced a *spatula*, he irritated the whole surface of the artificial joint. This brought on considerable inflammation, which ended in ankylosis, and the patient was cured." The subcutaneous puncture and the needle, if they are likely to obtain the same ultimate result, are surely preferable to the incision and the spatula. White's severe operation of cutting down, and sawing off the ends of the bones, was not only hazardous to life, but not unfrequently failed to accomplish the end in view; in some cases it proved fatal. Dr Physick's seton is less formidable than the saw; but chance of failure with it is not slight; and in fractures of the lower extremity, indeed, its success may be regarded as only the exception to the general rule. Mr Amesbury's pressure has not come into vogue; but is rather looked on as painful, irksome, and uncertain. Of Dieffenbach's pegs I have no experience; but I am quite sure that the insertion of them must in most cases prove difficult, and their presence in many dangerous. Lately Mr Burman has employed galvanism as an exciting agent, and with success, in the case of an ununited fracture of the tibia of fourteen weeks' duration. There were other means at work, however; namely, an improved diet, and constant firm pressure on the fractured ends. It may have been the galvanism alone that moved the plasma and its organization; but many perhaps will be inclined to rank that agent rather as an adjuvant than as a principal. To the method by subcutaneous puncture it might prove a powerful auxiliary. The simultaneous use of both is not incompatible; and, in these days of chloroform, the frequent repetition which either may require, cannot be considered as cruel and objectionable. From neither, conducted with ordinary prudence, can risk of untoward casualty be suspected.

Of course, in no case is local treatment exclusively to occupy our attention. Constitutional management must never be overlooked; and often it proves of the highest importance.

Further, I may add, that the foregoing observations are not intended to apply to those cases where non-union is obviously dependent on the impaction of a slip of muscle between the fractured ends, or to the presence of a piece of dead bone, or to the lodgement of a foreign body from without. In such cases, immunity from motion, with attention to the system, after removal of the cause, is usually sufficient.

ARTICLE II.—*Additional Observations on a Case of Intestinal Concretions.* By D. CARMICHAEL, M.D., Buckie.

IN consequence of having read Dr Turner's remarks on the case of intestinal concretions, published in the last January Number of the *Monthly Journal of Medical Science*, as well as those in the *Monthly Journal* of September 1841, by Drs Turner and Douglas Maclagan, and having, on both occasions, been engaged in assisting my friend, Dr Milne, in attendance on the patient Alexander Gordon, near Cairnfield, from whom the concretions were extracted, I beg to offer a few additional observations on the same.

The symptoms, on both these occasions, seemed to indicate that the concretions were formed in the *caput cæcum coli*; from which place, and during their whole passage through the colon till they sunk into the pelvis, they could be perceived through the abdominal parietes as an irregular mass, larger in size than a goose egg. During their whole progress the balls did not pass singly, but remained all in contact till they reached the rectum.

Whilst the mass continued in the cæcum, the patient's health was nearly unimpaired; but, during their whole progress along the colon, the symptoms were those arising from intestinal irritation and partial obstruction. Their course was arrested on both occasions, for several weeks, in the right side and over the middle of the transverse colon; during which time the symptoms were very distressing, and were those occasioned by pressure on the liver and pylorus, and indicated great functional derangement of the stomach. It cannot, therefore, be matter of surprise that Dr Turner, who saw the patient but incidentally, should have, as he himself allows, mistaken the complaint at first for a schirrus of the stomach.

I do not consider that the arrestment of the concretions mentioned above was occasioned by any stricture of the intestine; for, in that case, such a mass could by no means pass at all. It appears rather owing to the anterior parts of the lumbar vertebræ, with the vessels, nerves, and viscera, over which the mass had then to pass, opposing their progress to the left side. When the mass arrived at the left transverse colon its progress was steady, and with comparatively little trouble, till its arrival in the rectum. When there, the concretions seemed to fill the whole cavity of the pelvis, and then the most urgent symptoms were, total obstruction of the bladder and bowels, with violent disuria and tenesmus.

The means of treatment we placed most confidence in were emollients, oleaginous and emollient laxatives, and enemata; and, when practicable, gentle friction and exercise, in order to assist the peristaltic action of the bowels. When the concretions arrived at the rectum, mechanical assistance was required; difficulty was experienced, and considerable ingenuity was required, to effect their separation and expulsion. Some of the largest required to be divided.

In July 1840, fourteen concretions were taken away; again, in September 1846, the number was eighteen. On a former occasion, when several balls were removed by purgatives alone, as his medical attendant informs me, the number was not recorded. They were of all sizes, from half an inch to two and a half inches in diameter.

From a careful examination of the structure of the alimentary concretions by Dr Milne and myself, with the aid of good microscopes, we have come to the conclusion that they are entirely composed of the silky fibre or *caryopsis* that covers the oat grain, and the ordinary fæcal and mucus matter of the colon, arranged in layers slightly differing in colour, owing to the variable contents of the colon when the matter was accumulating.¹

Attempts have been made to account for the formation and growth of these concretions on chemical and physiological principles. To me it appears that the mechanical properties of the silky, hairy covering of the oat grain, of which almost their whole bulk is composed, account sufficiently for their formation.

This substance, when separated from the meal, is generally termed *oat dust*, and is possessed of a remarkable felting property, superior perhaps to that of wool. This may be seen at any time in any oatmeal mill, where, by the rotatory or oscillatory motion of the harp or wirecloth sieve that separates it from the grain, the dust shows a decided tendency to form into soft balls; and if any thing suitable for a nucleus—such as thread, or any other adherent matter—occurs in its way, it will gather round it in layers, and thereby form masses of considerable size. This is more particularly the case if the mass be occasionally wet; for it seems that the felting property of this, as well as that of wool, is increased by watery fluids.²

From the writings of Drs Munroe, Haller, Thompson, Wollaston, and others, it would appear that concretions formed of the fibres of

¹ These fibres, when analysed, are found to possess several of the properties of lignin.

² The felting and adhesive property of this fibrous matter is so very remarkable, that I have little doubt, by proper manipulation and machinery, it could easily be manufactured into hats and felt cloth; and, from the fineness of the fibre and its great tenacity, it is probable that a fabric could be made of it much lighter and stronger, though perhaps not so elastic, as of wool.

[We have to thank Dr Carmichael for several specimens of the concretions, as well as of the oat dust and balls formed on the wirecloth sieve of a mill. In a note accompanying these specimens, Dr C. observes,—“They were procured in September 1846. Within the same box you will find some balls, formed artificially in a mill on threads, which shows that the *dust* has the power of agglomerating without the aid of any other adhesive matter; the threads being wet with water only once. You will perceive that nothing but the hairy silky fibre enters into the *intimate* composition of the artificial balls, the chaff, portions of husk, &c., being entirely rejected; which would seem to indicate that there must be something peculiar in the structure of that matter which causes this agglomeration or felting.

“One of the intestinal concretions has a small hole in it; this is the work

the oat grain were much more frequent in former times than they are found to be now. This may be accounted for, first, by the more perfect machinery of the mills in modern times, separating more perfectly the oat *dust* from the oat meal; and, secondly, by the difference of the food of the poorer classes of people in the present day from what it was during the last century.

Besides the ordinary mills being very defective in machinery, there was in pretty general use till past the middle of the last century, in many parts of Scotland, a domestic instrument for grinding corn called a *quirn*, composed of two flat stones turned round by the hand. With this there was no contrivance of any kind for separating the hairy covering called *dust*, so that the meal must then have contained a much larger proportion of this substance than now.

Before the present century a great part of the rural population of Scotland lived almost entirely on farinaceous and vegetable food. Except at Christmas, and some other feast times, flesh meat was never seen, and milk was so scarce as to be very seldom used. The substitutes for the latter were kale, and some liquid farinaceous preparations. Now matters are quite changed; flesh meat is more generally used, and milk is of universal daily consumption. The introduction of the sown grasses and turnips, with the improved system of husbandry, has made the above articles of more easy access; and the more general use of potatoes, and other reasons influencing domestic policy, have caused oats to be much less used now than formerly.

From analogy, it is to be expected that oleaginous and fibro-albuminous food would have the effect of preventing the growth of concretions which are formed by a process of felting or fulling. It is well known that oil destroys this property in wool. To enable wool to be carded, a process which is the reverse of felting, it is necessary to smear it with oil, without which precaution it would gather in clusters. On the other hand, the assistance given by fuller's earth to the processes of felting and fulling, seems to be entirely owing to its property of depriving the wool of its greasiness.

I have been informed by those who have had good opportunities of knowing, that concretions, probably similar to that given by Dr Reid of St Andrews to Dr Douglas MacLagan, were, in former times, frequently found in horses fed with *oat dust*, and were occasionally the cause of their death. Then the imperfect machinery of the mills left a considerable quantity of nutritious matter among the dust, and tempted the owners of mills to feed horses and cattle with it. Now, by the more perfect machinery, nearly all nourishment is separated from it, and millers are not in the habit of giving it to

of an insect, a worm or moth. Several specimens in the possession of Dr Milne have been attacked with insects."

No one can examine these specimens, without agreeing with Dr Carmichael as to the great felting property of the oat dust, and the probable correctness of the explanation he has offered of the formation of the concretions.]—ED.

these animals, and from experience they are generally aware of its dangerous properties. They, however, sometimes feed swine with it. The form of the intestines of these animals, and the usual fatty nature of the accompanying food, and the short time they generally remain with the millers who breed them, may partly account for its being less hurtful to these animals than to some others which live altogether on vegetable food, and whose colon cells are particularly large.

A question arises, is there any thing apparent in the structure of these fibres to account for this property of felting? Dr Milne and I have examined them with the aid of powerful microscopes; but under them they appear only as transparent hollow quills, not very unlike those of a porcupine, some of them slightly striated longitudinally. I am of opinion, however, that a still more powerful instrument would show them in a certain degree serrated, or indicate something like scales or barbs, similar to what is said to be on wool.

The celebrated German, Kohl, has directed attention to a species of concretion formed under very different circumstances; but apparently on the same principle as those we have been treating of. His words are—"In the hermitage (near the cascade of Achern Den, south side of Loch Tay) I found a little curiosity, which I do not remember to have seen any where else, since it is, I believe, peculiar to Loch Tay. This is a natural ball, formed of the needles of the larch-tree, which, falling into the lake, are dashed about by the wind and waves till they adhere together in masses of a globular form, though it is not easy to say how. The balls were about three inches in diameter, and resembled the balls of felted hair found in the stomachs of cattle. The needles appear to be arranged in a certain order, and so firmly united, that it was not easy to separate them. In the centre we found at length a small piece of wood, and I was told there was always something, a leaf, a seed, or a little grass, seeming to form the nucleus. They are most frequently found, I was told, in November, when the lake is most agitated by wind and storms; but I must confess the mode of their formation remains a mystery to me."—*Chapman and Hall's edition of Kohl's Scotland*, p. 70.

It may be asked, has there been any thing peculiar in the mode of living, or other circumstances, of the patient Alexander Gordon, to account for the repeated formation of these concretions? It must be acknowledged that this is a question difficult to answer. He belongs to the poorer class of small crofters, is accustomed to use very little animal food at any time, and has mostly lived on oatmeal, which is almost the whole produce of his small place. But this alone will not altogether account for this repeated formation in his case, as many other individuals live in the same manner, and are entirely free from them. There must be something peculiarly predisposing to their formation, though it is not easy to say what it may be. It is well

known that the *cæcum*, from the circumstance of its being out of the reach of the ordinary current of the bowels, is the place where foreign substances of every kind are apt to accumulate. The uniform structure of the concretions shows that no part of them was formed in the small intestines. From what I have seen, I am of opinion they were all formed in the *cæcum*, and that they had arrived at their full size before leaving that place. This would seem to indicate that that part must be, in the case of Gordon, more than ordinarily capacious, and on that account peculiarly suited to the growth and accumulation of concretions.

It is evident that, when any of the balls are in process of formation, they have the property of causing the formation of new ones, which in their turn mutually assist the growth of one another. This they do by their firmness, pressure, and constant rolling, causing every suitable nucleus in the way to take on the same kind of action. It seems likely, that although the great mass of concretions were removed at the different times mentioned above, some incipient concretions had still been retained in the *cæcum*, which led to the after accumulations.

ARTICLE III.—*Remarks on the Occurrence of Convulsions during the progress of Typhus Fever.* By WILLIAM AITKEN, Jun., Esq., Resident Clerk in the Edinburgh Royal Infirmary during the Fever Epidemic of 1847–8.

(Read to the Royal Medical Society, March 10, 1848).

THE occurrence of convulsions during typhus fever is comparatively rare, and generally indicates an unfavourable termination. Little is known of their nature; for, in addition to their rare occurrence, the subjects of them seldom present any morbid appearances after death sufficient to account for their existence. The conditions under which such symptoms take place, therefore, deserve to be recorded; and as I have recently had an opportunity, in the hospital, of witnessing five cases of typhus fever terminate fatally by convulsions, occurring under conditions different from those described by authors as common to convulsions during that disease, I am induced to make them public.

CASE I.—Margaret Low, æt. twenty-four, a servant, admitted into Ward 12, January 17, 1848, suffered from smallpox about six months ago, from which she made a slow recovery. Since then her health has not been good, although she had no particular complaint. For the last three days has been confined to bed.

Present condition.—Restless and agitated. Expression of countenance anxious. Intellect confused. Bowels confined. Pulse 120.

January 18.—Is more composed, and very deaf. Bowels have been well opened. Tongue dry, and covered with a brown fur.

January 20.—Much delirium last night, with restlessness and inclination to leave her bed. A copious eruption in large red patches, mottling the skin over the chest and belly. Bowels open. Urine passed in usual quantity.

January 21.—Became more prostrate last night, with a pulse feeble and intermittent. Between eight and nine o'clock this morning, was seized with convulsions. The muscles of the face were drawn together. The eyelids were forcibly closed, contracting still more so on the least irritation from touch. Pupils dilated on exposure to light. The teeth were clenched firmly together, and foam issued from the mouth. The legs and arms were alternately rigid and relaxed. Breathing became laboured, the surface cold and pale, and the pulse scarcely perceptible at the wrist. She continued in this state about three hours, and died comatose about eleven o'clock.

Dissection.—Surface of the body somewhat livid, and deeply pitted by the cicatrices of smallpox.

Head.—Slight subarachnoid serous effusion, with some venous congestion at the posterior and dependent part of the brain.

Chest.—Lungs somewhat congested with venous blood; their texture healthy. Heart healthy. Blood fluid.

Abdomen.—No apparent lesion in any organ.

CASE II.—James Gibson, æt. twenty-two, a mechanic, admitted into Ward 7, January 17, 1848, reported to have been suffering for seven days.

Present condition.—Exanthematous eruption of typhus fever exists in abundance over all the body. Face flushed. Expression of countenance anxious. Pulse 100.

January 19.—Eruption continues. Face flushed, and the general surface of the skin has a decided yellow tinge; and there is tenderness on pressure over the region of the liver.

January 20.—Since yesterday there is no apparent change, except that the eruption is fading, and he is more restless.

January 21.—About seven o'clock in the evening he appeared somewhat more restless and stupid, constantly rolling his head from end to end of the pillow, and tossing his arms about. About half past eleven o'clock he was seized with convulsions of a clonic nature. Extremities cold, alternately rigid and relaxed. Jaws firmly clenched. No pulse perceptible at the wrist. For about five hours he continued in this condition, the convulsions maintaining their clonic character, and he died suffering from a spasm, with rigid flexion of the extremities upon the trunk of body.

Dissection.—The external aspect of the body presented considerable wasting of the muscular texture.

Head.—Slight subarachnoid serous effusion. No vascularity nor congestion of vessels in the brain.

Chest.—Considerable congestion of both lungs. Pulmonary texture otherwise healthy. Heart normal. Blood fluid.

Abdomen.—Spleen of large size, very soft and almost diffuent. Condition of the bile in the gall-bladder somewhat inspissated. Other organs healthy.

CASE III.—Archibald Christie, æt. thirty-eight, a carver, admitted into Ward 9, January 21, 1848, having been five or six days ill previous to admission. The following day a copious eruption of small rosy blotches appeared over all the body. Face became flushed. Eyes injected. Otherwise no particular complaint, nor local complication. Bowels being confined, he was well purged with castor-oil.

January 27.—Since admission he appeared to be going on favourably, but to-night his pulse was observed to be extremely irregular, intermittent, and weak. Still he made no complaint. Eyes still appeared more injected than usual, and face more flushed. Examination of the heart failed to detect any murmur, or any abnormal dulness over its region. Ordered carbonate of ammonia and wine.

January 28.—About four o'clock this morning, after having been up at stool, he became convulsed, and the repetition of the convulsive attacks caused

the medical attendant to be summoned, who found him lying on his side, rolling his eyes under the upper eyelids, so that the lower part only of the cornea was visible. The eyes were still intensely injected. His arms frequently gave convulsive twitches, but he had no regular fit after half-past five o'clock. He could not answer questions. Was ordered a turpentine enema, and mustard poultices to the calves of the legs and precordial region.

Eleven o'clock forenoon.—Apparently getting worse; stupor increasing; face flushed. The eyes, intensely injected, were incessantly rolling under the upper lids. The pulse, however, had regained its strength, and had lost its irregular and intermittent character. The stimulants were withdrawn. Leeches were applied to the temples, and a blister to the head. Mercurial ointment was also rubbed in beneath the armpits, and large doses of calomel administered. The mustard poultices were also continued, but all to no purpose; he became perfectly comatose, and died at four o'clock P.M.

No dissection of the body could be obtained.

CASE IV.—Thomas M'Kenzie, æt. twenty-one, a confectioner; admitted to Ward 7, February 5, 1848; reported to have been ten days ill. Of sober habits, and previously in the enjoyment of good health. On admission his pulse was extremely feeble, and extremities cold; but on resting in bed for some time his pulse increased in strength, and the heat of skin returned. Intellect clear. Typhus eruption abundant.

February 7.—Heart's action feeble; sounds normal. Considerable depression of spirits and prostration of strength. Intellect confused. Hair ordered to be shaven off the head. About eleven o'clock in the evening he was seized with convulsions, which, judging from the description given by the nurse, were of an epileptic character. When I saw him about one o'clock in the morning, his eyes were fixed, pupils dilated, and insensible to light or to external objects. Conjunctiva injected; skin warm; pulse 96, soft. The convulsive fits recurred at intervals; were of a clonic nature, and accompanied with moaning, as if from suffering pain. He involuntarily passed in bed a copious evacuation of urine; fæces of a very fetid character.

February 8.—Appears more sensible and less agitated. The fits continued to recur for about six hours. He is now able to protrude his tongue, which is free from tremor, moist, and clean. Is unable to articulate distinctly. Ordered the acetate of potass in ten grain doses.

February 9.—Towards evening last night the fits returned, disappearing again towards morning. About eleven o'clock he died comatose. From the nature of the fits, as well as from the general aspect of the patient, it was suspected that the convulsions might merely be an attack of epilepsy, to which he might have been accustomed. Our suspicions accordingly led to further inquiries, from which we have evidence that they were unfounded. It was stated confidently by his friends, that he had never been subject to convulsions—a statement more particularly confirmed by a young man who slept in the same bed with the patient for many years, and who positively asserts that he never knew of his companion suffering from fits.

Dissection.—External aspect of body presented appearances of much emaciation.

Head.—No subarachnoid effusion, nor congestion of the brain, other than hypostatic. The veins over posterior part of cerebrum and cerebellum were full of blood; anterior part of cerebrum quite free from venous turgidity. No vascularity nor injection of vessels existed, nor could softening be detected in any part of the organ.

Chest.—Old and firm adhesions between the pulmonary and costal pleuræ of the right side. Right lung collapsed and shrunk, with slight congestion and condensation at the apex. No tubercular deposit existed in either lung. Heart healthy. Blood fluid.

Abdomen.—Liver irregularly tuberculated, firm, small, and of a globular

form, presenting the condition of cirrhosis. Both kidneys congested. Spleen weighed thirteen ounces. No deposit existed in it or in any organ.

CASE V.—Anne Home, æt. twenty-six ; married ; admitted into the Fever House, February 4, 1848 ; reported to have been eleven days ill. No eruption on admission. Febrile symptoms of moderate severity, of a typhoid type.

February 12.—Since admission she seems to be going on favourably ; but, although typhoid symptoms were predominant, no eruption became visible. For the last three days has been more restless. Intellect confused, with a tendency to stupor. About eleven o'clock to-night she was seized with a convulsive fit, and in about half an hour afterwards she appeared breathing with apparent ease, at intervals accompanied by a deep and prolonged inspiration. Cheeks somewhat flushed, but felt cold, as did the surface of the body generally. The pupils were contracted, but there was no vascularity nor injection of the eyes. For some minutes she would continue quiet, and then gradually fall into a kind of tetanic paroxysm, accompanied by a cataleptic state of the hands and arms, writhing of the body and grinding of the teeth. When first seen the pulse was about 90. During the continuance of the attack it wavered much, became slow, laboured, and irregular, until it came so low as 56, about an hour after the commencement of the convulsions. To have ten grains of calomel, followed by a turpentine enema, and the acetate of potass with the decoction of broom tops as a diuretic.

February 13.—Face flushed, but cold ; is restless in bed. Arms are constantly being rolled over the head, and the body writhes violently about ; pulse about fifty, full and laboured ; pupils dilated, and will not contract on exposure to light ; no urine passed since yesterday forenoon ; towards evening became completely comatose, and died about ten o'clock.

No dissection obtained.

CASE VI.—On the 4th of February an epileptic patient, in Ward 7, was seized with convulsions during the progress of typhus fever. He ultimately recovered, although he had fits frequently, and informed us that he was a patient in one of the general wards about two years ago suffering from epileptic attacks. To the existence, therefore, of the epileptic constitution, we may ascribe the occurrence of those convulsions, whose presence during the progress of typhus fever has caused his case to find a record here.

In these cases there are some points deserving special consideration, namely,—the conditions under which the convulsions occurred—the rapidity with which they proved fatal—the almost universal absence of any morbid appearance sufficient to account for death—and, lastly, the occurrence of six cases in different wards within twenty-one days, five of which terminated fatally.

All the cases (with one exception where the eruption was not visible) were cases of undoubted typhus fever. In the first case death took place on the seventh day, about two hours after the convulsions commenced. No morbid appearances were found sufficient to account for the presence of convulsions. The lungs were merely slightly congested, and, if we except the existence of convulsions, the constitutional and local symptoms were not those of inflammatory action. Death took place by coma, as if the patient laboured under the influence of a poison.

In the second case death followed on the eleventh day, four or five hours after the convulsions commenced. No morbid condition was apparent which could be considered as in any way connected

with convulsions before death. The spleen was soft and almost diffluent—a frequent condition of the spleen in fever. Lungs were slightly congested—a condition susceptible of explanation by the mode of death—and, if we except the convulsions, there were no constitutional or local symptoms of inflammatory action. There is one interesting feature in the case to which some degree of importance may be attached; namely, an icteroid condition of the skin, and the appearance of convulsions simultaneously with the fading of the eruption about the tenth day of the fever.

The third case terminated fatally about the fifteenth day, six hours after the convulsions commenced. In this case there was the greatest amount of evidence during life, compared with the other cases, of the existence of inflammatory action in the brain or membranes, as far as constitutional and local symptoms could indicate; besides the presence of convulsions, we had an exacerbation of the febrile symptoms, flushing of the face, and injection of the eyes. Death took place by coma.

In the fourth case death took place on the fourteenth day; but he lived twenty-four hours after the first convulsive fit. No morbid condition was found sufficient to account for the occurrence of convulsions. Both kidneys were slightly congested, but the microscope failed to detect any morbid condition of their tubes. Death took place by coma.

The fifth case was of a decidedly typhoid type, but no eruption was visible. At the nineteenth day convulsions proved fatal in twelve hours. The symptoms simulated compression of the brain. The circulation was oppressed, as indicated by a labouring, irregular, slow pulse; there was complete insensibility, and apparently an uneasy sensation, somewhat similar to the condition experienced by a person who, having eat or drank too much, has fallen into a disturbed sleep, and lies writhing and tossing himself in bed. Death took place by coma.

In none of these cases were there premonitory symptoms which could lead to a suspicion that convulsions were about to take place. A restless, anxious, and excited state, however, was common to all for some time before the spasms came on; a feeble intermittent pulse was also sometimes present; disappearance of the typhus eruption in one case, and suppression of urine in another.

It is generally supposed that convulsions are associated with the manifestations of disease either before or after death: but it is not always made evident, as these cases abundantly testify, where the lesion is, to which the occurrence of convulsions might be ascribed. They are found to occur during the invasion, the progress, and the decline of many acute diseases; and accordingly their immediately exciting causes have been divided into two classes, namely,—(1). Causes which act upon some portion of an organic nervous centre, or upon the viscera which it supplies; (2). Causes which act upon the cerebro-spinal system itself. In some of these ways they are

generally excited by irritation in the gastro-intestinal canal; by irritation from the existence of organic disease affecting the nervous centres; by irritation from inflammation of the cerebro-spinal system or its membranes; by irritation from a morbid condition of the blood, in connexion with a more susceptible condition of the nervous system. By this latter source and mode of irritation, we consider that those convulsions have been brought about, in all the cases which are here recorded, except in the case of epilepsy. In *none* of the cases, with one exception, was there evidence of inflammation of the brain, or of its membranes, or even of congestion there. The exceptional case is No. III.; but, as no dissection was permitted, the evidence is incomplete. In *none* of all the cases had we evidence of irritation existing in the intestinal canal. In *none* of those examined after death, had we any evidence of the existence of organic disease which could affect the nervous centres. The presence of jaundice in one case, and the sudden disappearance of eruption immediately followed by convulsions, are conditions which, as evidence of the universality of a morbid process, we must connect with the morbid condition of the blood during typhus fever. Besides these pathological conditions thus shortly noticed, no other source of irritation did examination after death disclose; and medicines, prescribed according to any of these theoretical indications, failed to exercise the wished for therapeutic effect.

Convulsions, we know, are not apt to occur from irritation of the cerebro-spinal system by causes which act upon any portion of an organic nervous centre, unless it possess a marked disposition to disease, or is morbidly susceptible of impressions. We further know, that in cases of typhus fever the cerebro-spinal system is just in such a condition; and, according to the opinions entertained by the continental pathologists, it is now generally believed that typhus fever is accompanied by a peculiar dyscrasia of the blood. A specific poison is received into the system, by which the blood is morbidly altered, and an altered condition of the nervous system ensues, an invariable element in the pathology of typhus fever. The cerebro-spinal system is thus prone to be affected by the blood itself, and to receive impressions from distant parts, such as from the imperfect organization of exuded blood-plasma into various organs and viscera, the consequent softening of textures, the non-elimination of urea from organic change of kidneys, or suppression of urine. Any or all of these states may induce convulsions. In one of the cases, namely, that of the epileptic, the urine was albuminous during the fits; no opportunity occurred of examining the urine in the other cases at these periods; but even although an albuminous condition of the urine had been found to exist in all, it by no means follows that the kidneys were diseased, or that urea was not eliminated in sufficient quantity. The dissections, when obtained, showed the kidneys free from disease; and it has been shown by Dr Finger of Prague (*See Retrospect of this Journal for April 1848*), that temporary

albuminuria is an occurrence by no means unfrequent during the progress of acute diseases, independently of any lesion in the kidneys; and he has shown that, next to tuberculosis, the temporary albuminuria occurs most frequently during typhus fever. With such albuminuria convulsions have no necessary connexion. When they did occur during the puerperal state along with albuminuria, it was found by Drs Christison and MacLagan, that no traces of urea existed in the blood; and we know that, in cases of typhus fever, urea is eliminated in great abundance by the kidneys, together with an albuminous condition of the urine, in many cases unconnected with renal disease.¹ This temporary albuminuria during typhus fever is further consistent with the altered condition of the blood during that disease; for a deficiency in albumen as well as of salts is part of the change which takes place in its normal constitution. We know also the physiological origin of urea,² and we know how the condition of all the tissues is affected by typhus fever,—a condition which may in part be explained by this excessive elimination of urea. When, therefore, an albuminous condition of the urine occurs along with convulsions, whether in the puerperal or in the typhus state, independent of renal disease, both may be regarded as different manifestations of a constitutional disease having no necessary connexion with each other.

In some of the cases recorded, I have mentioned the existence of subarachnoid effusion, which suggests the question, “Of what importance is such an appearance in a pathological point of view?” We know that in cases of typhus fever, whether accompanied by convulsions or not, the lesions within the head most frequently consist of an increase of the serous exhalation from the membranes of the brain, especially within the ventricles and beneath the arachnoid; but the amount of effusion varies much and is seldom considerable, although a considerable amount is not incompatible with health. The importance to be attached to the existence of subarachnoid effusion is, therefore, by no means definite. Much may depend upon changes after death, and in no case can we state that such an appearance indicates disease, unless it exist along with some other apparent lesion, such as congestion and injection of vessels in the brain or membranes, softening of the nervous substance, or a compressed appearance of the cerebral convolutions.

According to Dr Christison (*Lib. of Med.* Vol. I. p. 135), we find that convulsions during the progress of typhus fever may be referred to *two* classes; as connected (1) with the existence of meningitis, and associated frequently with delirium tremens; as connected (2) with the epileptic constitution.

¹ Paper by M. Martin Solon, read at the Académie de Médecine, November 1847, of which notice is taken in the monthly *Retrospect* of this Journal for March 1848.

² See Golding Bird on Urinary Deposits, p. 33.

The cases here recorded are not susceptible of classification under either of these divisions. They must therefore stand alone, constituting a *third* class, and I consider that their occurrence may be consistently explained by the condition of the blood in typhus fever, as containing either a morbid material, or as being changed from its normal constitution, which undoubtedly it is. The stage of the fever at which the convulsions occurred, namely, from the 10th to the 19th day, is also the period when we find the local lesions in organs taking place during typhus fever, such as deposit in the spleen, lungs, and intestines, characterised by imperfect organization.

There is still another feature in the history of the cases I have recorded, which is at least curious if not important; namely, the occurrence of all the cases within twenty-one days. Two happened on the same day in different wards, and about six or seven days intervened between the rest. About the same period also, there was a case of tetanus in the Surgical Hospital; and I have been informed that several cases of puerperal convulsions occurred throughout the town at the same time. Connecting, therefore, all these facts, it is impossible altogether to do away with the notion, that some peculiar condition of the atmosphere, the nature of which is unknown, plays an important part in the induction of convulsions, whether these occur during the healing of a wound, the existence of the puerperal state, or during typhus fever.

ARTICLE IV.—*Contributions to the Pathology of the Kidney.* By WILLIAM T. GAIRDNER, M.D.

PART I.—ON THE PATHOLOGICAL ANATOMY OF THE KIDNEY.

I.—1. *Exudation within the Tubules.*—(Continued from p. 813).

c. Exudations in the form of Pus.—The occurrence in the cortical substance of deposits having all the external characters of pus, is not very uncommon. Their most usual form is that of small abscesses, rarely exceeding the size of a pea, and frequently much smaller, sometimes confluent, and irregularly disseminated through the cortical substance. They are generally surrounded by more or less deep vascular redness; this, however, is limited to a narrow rim around the deposit; the remaining portions of cortical substance being either natural in appearance, or paler than usual. These appearances are well delineated in Plate II. Figs. 1 and 2, of Rayer's work.

The formation of abscesses having a distinct limiting membrane, or surrounded by condensed tissue, is, in the kidney, of extremely rare occurrence. I have already related a case (Observation II.) where a cavity of this sort was found; but the appearances of the contained matters to the naked eye and under the microscope had no resemblance to those of pus.

The following case is of considerable interest in several points of view, and tends, in connexion with other observations, to elucidate the formation of pus in glandular organs :—

OBSERVATION X.—*Purulent Deposits in Kidney and Lung—Sloughing Abscess of Spleen—Peritonitis.*—Cecilia Hall, æt. twenty-seven, shoebinder, admitted Dec. 28, 1847, under Dr Douglas. Had been affected for eight days before admission with considerable fever, with rigors, vomiting, and abdominal pain. On admission the vomiting had ceased, the other symptoms continued. There was tenderness with dull percussion in the hypogastrium and left iliac region. There was also a good deal of cough. This last symptom increased considerably during the next three weeks, and the right lung became somewhat dull on percussion, with tubular respiration posteriorly, and mucous and subcrepitant râles in various parts of the chest. The abdomen became tympanitic, but the tenderness disappeared. She died much exhausted, but without pain, on Jan. 30, 1848, one month after admission.

On dissection, the heart was soft, but not altered in structure. The lungs were emphysematous, and much engorged; the upper lobe of the right lung scarcely crepitated on pressure, but floated in water. In various parts of both lungs were nodules of pulmonary tissue, which were quite dense, of a greyish colour, and some of which contained in their centres yellow creamy pus. The cavities contained in the pus were found by the probe to communicate with the smaller bronchi, and were lined by a membrane having an exact resemblance to mucous membrane. The spleen was slightly enlarged and soft; in its substance, towards the convex surface, there were two or three masses of soft exudation, one of which was surrounded by a line of ulceration, and was in part detached, projecting from the surface of the organ. Around this part, and between the surface of the spleen and the diaphragm, a layer of soft yellow lymph was thrown out, which connected the opposing surfaces of the peritoneum. Stomach and intestines healthy. The kidneys were of the natural size; one of them contained in the cortical substance numerous small abscesses, from the size of a pin's head to that of a pea; several of these occurred in groups towards the surface of the kidney. The abscesses were not surrounded by any indurated substance, but by a vascular rim of a rose colour, and about half a line in diameter. They contained a bright yellow pus. A little pus was also in one or two places infiltrated into the tubular cones, near their base. The pelvis of the kidney was slightly vascular, but contained no fluid. The peritoneum lining the bladder and pelvis was somewhat vascular, and blood was extravasated in considerable quantity in the sub-peritoneal cellular tissue, both in the pelvis and about the situation of the umbilicus.

The microscopic examination of the fluid from the minute abscesses in the kidney showed the following objects :—1st, Spherical granules and molecules insoluble in acetic acid. 2d, Rounded corpuscles (Fig. 12, *b b*) of the usual size of pus corpuscles (1-100th of a millimetre), and of a dark granular appearance. On being treated with acetic acid, they became more transparent, and showed in their interior a greater or smaller number of spherical granules, and occasionally an ill-defined nucleus. On the whole, however, they underwent less change than is usual in pus from an ordinary abscess on the addition of acetic acid. 3d, Larger corpuscles from 1-80th to 1-50th of a millimetre in diameter (Fig. 12, *a a*), of nearly spherical form, and crowded with granules; on adding acetic acid they underwent little change. 4th, Extremely delicate cells (Fig. 12, *d d*) of the same size as the last mentioned, and containing a granular nucleus, about 1-100th of a millimetre, and exactly similar to the pus corpuscles described above (2d); the cell wall extremely attenuated, readily yielding to pressure, and disappearing completely after the addition of acetic acid; the space between the cell wall and the nucleus perfectly free of granules and molecules, and filled apparently with transparent fluid. On scraping the walls of one of the small abscesses gently with the

point of the knife, and examining the adhering tissue, fragments of tubes were discovered, which contained all the above elements, and in which the cells and nuclei described appeared to have taken the place of the normal epithelium. The smaller corpuscles above mentioned (2d) were in greater abundance than the other elements. In the other parts of the kidney, the microscope showed nothing abnormal.

In the lung the pus presented appearances so exactly similar to the above, that the same description will apply to both.

Fig. 12.

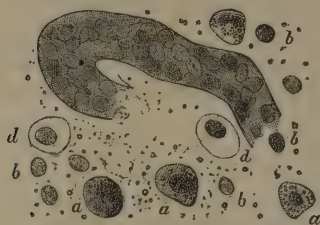


Fig. 13.

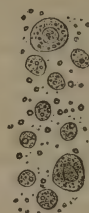


Fig. 12. Pus and fragment of tubule from small abscess in the kidney of Hall, 250 diameters. a a, Granular cells of the size and appearance of granular epithelium. b b, Smaller pus corpuscles, presenting a very granular structure (most of them have been represented too flat by the engraver). d d, Corpuscles like the preceding, but surrounded by a cell with clear contents and an exceedingly delicate wall. The tube is seen to be filled mostly with the bodies b.

Fig. 13. The corpuscles treated with acetic acid.

In the *Monthly Journal* for February 1848 (p. 589), Dr Bennett has described and figured pus corpuscles, which appeared as granular nuclei, surrounded by a delicate and transparent cell-wall. These corpuscles he has found in abscesses of the lung and kidney, and also in grey hepatization of the lungs; and he thinks that at a later stage of their formation the cell-wall disappears, leaving the nucleus as the mature pus corpuscle. From having seen Dr Bennett's previous demonstrations, and had my attention turned to the subject, I had no difficulty in recognising the bodies, d d, as identical with those described by Dr Bennett. But what appears to me worthy of attention in the present case is, 1st, the coexistence of these bodies with the cells a a, which are undoubtedly the granular epithelium cells described in a previous part of this memoir (*May No.*, p. 807); 2d, the existence both of these bodies and the smaller pus corpuscles *within* the tubules, where they appeared to take the place of normal nuclei and cells; 3d, their existence in the lungs, in cavities formed by dilatations of the smaller bronchi: in short, their formation both in the lung and kidney in connexion with a mucous surface.

It is well known to microscopic observers, that the pus formed on the surface of mucous membranes seldom presents the clear and definite reaction with acetic acid characteristic of normal pus corpuscles. It has even been at different times supposed that the pus

corpuscle is formed from the epithelium cell; and though this doctrine is undoubtedly attended with many difficulties, and has never been held by pathologists generally, it seems to be worthy of further investigation in cases like the present. The resemblance in size and form of the bodies *b b* to a granular epithelium nucleus, and of *a a*, *d d*, to granulated and non-granulated epithelium cells, appears, especially when taken in connexion with their position *within* the tubules, and the complete absence of normal epithelium, to be somewhat more than an accidental circumstance.

The symptoms in this case were referrible to the abdominal and pulmonary lesions described; but they were attended from the first by a marked typhoid depression, and a continued languor and exhaustion, which justified a suspicion of idiopathic fever superadded to the local disease.

In several other cases which have occurred in the Royal Infirmary of purulent deposits in the kidneys, a similar typhoid state has existed; in all, however, there have been numerous other lesions, and sometimes abscesses in other parts of the body, indicating a general tendency towards the formation of pus. The blood in these cases presented no unusual appearance.

2. *Exudation within the Malpighian Bodies.*—The granular (oleoalbuminous) form of exudation above described as so frequently occupying the tubes of the kidney, is also occasionally found within the capsules of the Malpighian bodies. When in large quantity in this situation, the tuft of vessels which normally fills the capsule, is completely compressed and shrunk, in most cases invisible. Where the exudation is in smaller quantity, however, it frequently adheres to the interior of the capsule and the exterior of the tufts, without materially affecting their form.

Exudation in this situation is generally accompanied by similar exudation, in greater or less abundance, within the tubes. The anatomical relations of these parts, as now generally understood, would, indeed, entitle us to expect that the pathological conditions of the one should be shared by the other. An exceedingly good illustration of these associated conditions will be found in Fig. 9 (*May No.*, p. 808). While, however, many cases of this sort have occurred to me, I have met with a still larger number which confirm the statement of Dr Johnson (*Med. Chirurg. Trans.*, vol. xxix. p. 4.), that the exudation within the tubes often occurs to a very great extent, without the Malpighian bodies being at all involved. More rarely a limited amount of deposit occurs within the latter, when there is comparatively little within the tubes. The cause of these differences is very obscure; nor does the examination of it appear to promise any results of importance, in the present extremely imperfect state of our knowledge as to the special functions of the Malpighian bodies.

3. *Exudation in the Inter-Tubular Tissue.*—In cases where oleo-albuminous exudation is in small quantity, it frequently appears to be disposed without any distinct relation to the tubes; and, where it is in very large quantity in the tubes, it sometimes appears in the interstices of the areolæ, as is represented in Fig. 9. In kidneys which are the seat of firm opaque granulations, a section of these frequently presents a dark opaque mass, covering a large portion of the field of the microscope, and showing no trace of arrangement; the deposit must, therefore, either have broken up the structure entirely, or completely occupied every vacant place. In all these cases, however, it is extremely difficult to determine by actual observation that the exudation is external to the tubes; and I am not a little disposed to doubt the occurrence of this condition, or at least to consider it as secondary to the complete occlusion of the tubes by exudation.

4. *Partial Distribution of the Oleo-albuminous Exudation. (Plaques Blanches de Lymphé Plastique, Rayer.)*—I have already described the formation of granulations as dependent on the accumulation of deposit in particular groups of tubules in the cortical substance. In such cases, however, the affection is probably at first general; they are very different from the form now to be described, in which the deposit is quite limited in extent, and isolated.

There are occasionally met with on removing the capsule from the surface of a kidney, irregular patches of a paler colour than the rest of the organ, sometimes a little elevated, sometimes depressed below the general surface. Their boundary is quite abrupt, and they are frequently surrounded by a well-marked rose-coloured areola, extending more or less into the surrounding substance. On making a section of these patches, they are found to penetrate into the cortical substance, and sometimes even a certain way into the pyramids. The vascular areola, when present, extends round them in every direction, and is found on examination to consist of highly injected Malpighian bodies and capillaries, with or without extravasation. The colour of the patches varies from yellowish-gray to a gamboge-yellow; their consistence is generally firm. On microscopic examination, they present a large amount of exudation, varying from the molecular to the large granular form. In some cases the tubes may be seen filled with exudation; in others, they appear to be in great part obliterated. In one case I found the Malpighian bodies quite free of exudation; they preserved their usual arrangement, and were readily discoverable by a simple lens on the surface of the section. The parts of the kidney not involved in the deposit, generally present no abnormal appearance.

Various illustrations of this species of deposit are to be found in Rayer's work. (See Pl. I. Fig. 6. Pl. V. Fig. 2. Pl. XXXIV. Figs. 2, 6.) He has figured it in various stages and under different names, as *Nephrite simple* and *rhumatisme*, and *Hémorrhagie*. Its origin and

progress are very obscure, and it has not been satisfactorily connected, either with other morbid states, or with any peculiar symptoms. I have seen it in connexion with fever, with puerperal convulsions, with erysipelas and dementia, and in several cases where no account of the symptoms could be procured. Dr Bennett possesses a most remarkable preparation and drawing of a case in which such deposits were most extensively present, and left very little intervening sound tissue. The affected kidney had a most singularly variegated appearance.—(*To be continued.*)

Part Second.

REVIEWS.

Die Krankheiten der Milz. Eine Pathologisch-Therapeutische Abhandlung. Von Dr C. R. HEINRICH. Leipzig: 1847.

The Diseases of the Spleen; a Therapeutico-Pathological Treatise. By Dr C. R. HEINRICH. Leipsic: 1847.—(*Continued from p. 675*).

HAVING confined ourselves, in our former notice of this work, to the merely introductory portion, we proceed now to lay before our readers a short analysis of the more practical part, that having reference to the diseases of the spleen. As formerly stated, this portion of the work is divided into two parts. The first, devoted to the consideration of diseases of the spleen in general, consists of four chapters. In the first of these, our author treats of the etiology of diseases of the spleen under the following heads:—Tendency of the spleen to take on disease (*Erkrankungs fähigkeit*)—Hereditary transmission—Predisposition from sex, age, constitution, and temperament—Dyscrasias and cachexies—Metastasis—Wounds—Physical causes—Anomalies in normal irritation, originating from nutritive substances—Medicines and poisons—Occupations—Meteorological relations—and Geographical distribution. The second chapter is devoted to the diagnosis and symptomatology. These our author confesses at the outset are beset with difficulties:—

Medical literature contains a large number of observations, and every experienced medical man may add to their number, in which the spleen has exhibited serious material alterations in structure, without a suspicion of such lesion having existed during life, much less having shown any sign by which the particular nature of the disease might have been discovered. Considerable enlargements of the spleen have thus been overlooked. The discovery of tuberculization, cartilaginous transformation, medullary softening, is generally an impossibility; no certain diagnosis of these states can be given: the greatest satisfaction we can attain is probably that afforded by a comparison of various attending circumstances. In other cases, degenerations of the liver, omentum, left kidney, mesenteric glands, &c., have been suspected; while dissection has revealed the true seat of the disease to have been the spleen, which, during life, had shown no symptom of its being the faulty organ. On the other

hand, disease of the spleen has been diagnosed, and suitable treatment adopted, when, subsequently, it has been found free from disease, and that the symptoms had originated from neighbouring organs simulating disease.—P. 76.

The causes of these perplexities may, our author thinks, arise from various circumstances. Among these he first of all mentions the situation of the spleen. The organ is so placed that very considerable alterations of form may entirely escape observation, in consequence of the yielding nature of the parts with which it is surrounded; these being pushed aside, the spleen finds a place in the cavity of the abdomen. Minute examination by means of the hand alone, or in confirmation with the pleximeter, may, it is true, lead to a determination of its exact position; but they furnish us no assistance in arriving at any conclusion regarding the nature of any affection, beyond the mere enlargement, to which the organ may be subject. We have no means of establishing the presence of tubercle, calcareous or bony deposits, small cysts, &c. In addition to the difficulties caused by its position, there are those originating from its functions; the latter may be almost or entirely abolished, and yet exhibit no symptom of their being so. The want of any pathognomonic symptom to assist us in our diagnosis is very much felt. In general, however, our author states, that when any great derangement in the function of the spleen occurs, be it the consequence of an acute inflammatory process, or of some organic change in its structure, there is a perceptible effect upon the digestive organs, or some of the more remote systems. "Many secondary phenomena occur in other parts, and they require the more attention, as local symptoms are frequently absent. These reflex symptoms, on the other hand, are apt to withdraw attention from the true seat of disease, and thus in their turn become a source of error."—P. 78.

On the whole, our author sums up with the following conclusion:—

The specific symptoms we possess to assist in our diagnosis of diseases of the spleen are so few, that we can frequently do little more than merely determine the presence of some affection of the organ, without presuming to arrive at any thing nearer to its true nature than an indefinite hypothesis. Such being the case, as Canstatt justly remarks, so much the more are we bound to give attention to that complexity of symptoms which may lead us to a general diagnosis of an affection of the spleen—splenopathy in general. These general symptoms are made up of the following various diseased states:—disagreeable sensations of pain in the left hypochondrium; sympathetic pains in distant parts; positive results from local examination; predominant affections of the left side; functional disturbances in the gastric, respiratory, circulating, and genital systems; an abnormal state of the blood, whence changes in the colour of the skin; many eruptive diseases, and forms of ulceration; hemorrhoids; hemorrhages; and melæna; anomalies in the secretion of urine; and, lastly, reflex symptoms in the nervous system, giving rise to a general want of harmony in the physical functions.—P. 78.

When local symptoms are present they are generally of the following character:—There is pain more or less acute in the left hypochondrium, close to the epigastrium; and this pain may vary, ac-

cording both to the position of the organ and the seat of the affection. The pain is higher up when there is inflammation in the upper half of the organ, immediately under the diaphragm, lower when it is the under half which is affected.

The nature and degree of the local pain is also different in different diseases. In acute splenitis it is acute and fixed, in chronic less so. It is also stated, that the temperature of the skin over the spleen is sensibly increased in inflammatory affections. When there is hypertrophy, or parasitic affections of large growth, there is a feeling of pressure, weight, and dragging. The formation of abscess is denoted by a sensation of deep boring, stabbing, and throbbing. In a case of aneurism of the splenic artery, there was pain of the loins, and strong frequent throbbing in the left side. Rupture of the spleen is accompanied with a sudden and specific local pain; the patient generally describing the feeling of tearing very accurately. From all the observations collected by our author, he says:—"The presence of pain denotes the existence of previous inflammation; enlargement of the organ and after formations only induce unpleasant sensations, when they have attained such a size as to produce irritation from their pressure."—P. 81.

But local sensations are frequently wanting, and this is specially the case in chronic softening and suppuration. Our author adopts Hamernjk's view,—“that the parenchyma of the spleen possesses no sensibility, and that pain only appears and continues so long as there is distension of the peritoneal coat.”

Sympathetic pains our author believes to be of nearly as much importance as those of a purely local nature. Like the latter, they vary in their seat and degree in different cases.

The most marked of these are pain in the left shoulder, clavicle, and scapula; a feeling of itchiness in the back, and a sensation as if cold water were trickling down its surface; pains have also been felt in the genital organs, pelvis, and other portions of the lower region of the abdomen. A case is given of a dissolute young man, whose spleen became ruptured in consequence of excitement of passion, during the course of an intermittent. During the short time he survived, he suffered fearfully from intense spasmodic pain in the testicles. The cause of these neuralgic pains, our author believes, is to be found in the circumstance of the enlarged spleen pressing upon the coeliac plexus; and from the connexion of this with other nervous centres, reactions ensue in the most varied and distant parts of the body. In addition to the enlargement, however, he conceives increased nervous irritability also necessary, and that this originates from the former.

We pass over the greater number of peritoneal derangements enumerated by the author, under the belief that the majority of them are in no way connected with disease in the spleen, in order that we may dwell upon the changes produced in the blood by lesions of that organ. The spleen being one of the blood-preparing organs, it is

evident that its diseases must have a prejudicial effect upon the properties of the blood, in other words, that a dyscrasia ensues. "It is an undoubted fact," says our author, "that the whole state of nutrition, and many secondary forms of disease, carry the unmistakeable stamp of a peculiar dyscrasia of the spleen."—P. 117. And the question arises, in what way is this peculiar crasis to be distinguished? The want of proper chemical analyses of the blood do not yet permit a solution of the question. And, till these are instituted in the various diseases of the spleen, no satisfactory progress in its pathology can be made. Our author furnishes us, in the mean time, with the following sketch of what is at present known regarding the state of the blood in diseases of the organ.

It is an old observation, that the blood in inflammation of the spleen presents a muddy and milky appearance. The most weighty testimony on this point, is that of Rokitansky, who observed it in intense inflammation of the spleen. Our author informs us, that elsewhere (*Hæser's Archiv.*) he has shown, "that a white, dusky, and milky appearance of the blood may be caused partly by suspended fat, partly by dissolved fibrine or albumen."—P. 119. Hence this appearance may be observed in diseases of very different kinds; in inflammation of the liver, kidneys, bowels, peritoneum, respiratory organs, diabetes, Bright's disease, and other constitutional affections, as well as in inflammation of the spleen; in short, in any diseases which imply disorder in the functions of the portal system. This appearance, however, has merely reference to the external qualities of the blood, without enlightening us either as to the actual state of the blood itself, or the cause of the appearance. When it occurs, therefore, in inflammation of the spleen, the first step is to determine whether it originates from a greater preponderance of fibrinous or albuminous granules, or of fat. "In the former case, a serum overloaded with fibrine especially, indicates an inflammatory dyscrasia. But it is also possible that an admixture of fat and albumen may be the cause of the wheyey appearance. In this case, according to my views of the physiology of the spleen, it is highly probable that an incomplete development of the chyle globules into blood globules, caused by the disorder of the functions of the spleen, and the consequences thereof, constitute the cause of this pathological appearance."—P. 119.

From the inflammatory our author proceeds to the melanotic crasis, a state of the blood which has an especial connexion with that of the spleen. "According to Schultze's description, probably taken from Scharlau, the melanotic state of the blood is caused by an abundance of old, used-up blood corpuscles, which do not become red on the addition of oxygen, and are rich in black fatty colouring matter."—P. 124.

This melanotic state is produced by a sluggish condition of the functions of the portal system, more especially the liver and spleen; and, not finding an exit through these organs, it circulates with the

general mass of the blood. The limits within which this state may exist without injuring the health are various. But when it has once attained a certain height, then, according to peculiarities in individuals, it may become in one the seat of serious disease in the liver, in the other, in that of the spleen. "Thus it happens that not merely tumours of the spleen, but fatty disease, dropsy, chlorosis, scurvy, purpura, intermittent fever, the dyscrasia of drunkards, the cancerous dyscrasia, typhus abdominalis, are all diseased states, which, though differing much from each other in their highest states of development, may yet be related by one common etiological origin."—P. 125. The importance of this subject is so great, that our author enters at some length into the consideration of the more striking of these crases. Into this subject we cannot follow him further than shortly to indicate his views.

The anomalies in the proportion of fat have already been referred to in the case of those suffering from disorder of the spleen and portal system. The researches of Becquerel and Rodier show that, in cases not only of a local, but a general character, the state of the blood is changed, and that a diminution of cholestrine and fat takes place. Future researches must determine the relation between this pathological law, and that other, where there is an increase of the former substance in functional disorders of the portal system. In the mean time, it is only a supposition that this state may be in some way connected with the same disorder of function, which tends to produce an abnormal relation in the quantity of blood fat. It is well known that portal blood is richer in carbon, colouring matter, and fat, than that of other systems; when its functions are deranged, therefore, it is natural to suppose, that the superfluity of fat passes into the general circulation, and is thereby deposited in the cellular tissue. This, we remark, may be all true; but we do not see what it has to do with disease of the spleen, more especially as in the very next paragraph our author states—"Notwithstanding all this, it is not proved that diseases of the spleen primarily predispose to the deposition of fat. A general tendency to fatness is not a common accompaniment of disease of the spleen. In drunkards and others in whom it does take place, a complication with disease of the liver is always to be suspected." We may once for all here state, that there is a confusion in our author's style which is exceedingly perplexing. In examining this treatise we have at times been sadly puzzled to comprehend his statements, or to make out, among the various symptoms and changes he has been at such pains to describe, what he really considered as essentially connected with disease of the organ, and what not. In his anxiety to bring every thing, directly or indirectly, ultimately or remotely, connected with the spleen to bear upon his subject, he has so contrived to manufacture a work which, if not a dissertation upon every disease, is at least a treatise wherein the spleen appears to play a part in every disease known or unknown. There is throughout a sad lack of distinctness and perspicuity, and an over-

whelming mass of German erudition, prolixity, and minuteness of detail.

The second part of this chapter is devoted to the complications, connexions, and differential diagnosis of diseases of the spleen with other organs. The author enters upon the consideration of these with a minuteness of detail, through which we cannot attempt to follow him, and must satisfy ourselves with selecting what he says on the connexion between disease of the spleen and intermittent fever.

Enlargement of the spleen in connexion with intermittent fever has long been known; and the question has arisen whether it is a consequence of the fever, or a cause. The former is the older and more common view; but the latter has been adopted by some French physicians, among whom is Piorry, one of its most strenuous defenders. From all that our author has been able to collect on the subject, he thinks himself entitled to draw the following conclusions:—

The spinal cord is the only central-nervous organ the disturbed functions of which can induce fever, and more especially intermittent fever; and the fever is an affection more of the posterior sensitive portion than of the anterior motory. But the pathognomonic symptom of intermittent fever is its type. Many hypotheses have been framed to account for this. That of Walther, who supposed it to depend on the accumulation of some base which must needs be thrown off in the shape of oxygenous compounds, appears untenable; while that which attributes it to a peculiar change upon the blood effected by miasma, and which, after a certain time, produces febrile irritation, and continues to do so at regular periods so long as any germ of the disease remains, appears the more probable.—P. 197.

Attempts to localize the seat of intermittents have at various times been made, and the researches of Kremer have given them some importance. He describes pain in the superior dorsal vertebræ excited by pressure, as a constant symptom. This has not been confirmed by recent observers; but its absence, our author argues, is no proof that there is no functional derangement of the spinal cord.

The discussion of this question—the seat and essence of the disease, as well as its proximate cause—involves another, that of the true nature of intermittents and fevers generally. In many cases, the state of the blood and fever stand to each other as cause and effect. Hence, intermittent fever, as a general rule, is preceded by a variety of symptoms, showing a deranged state of function in the blood-preparing organs, as well as of the spinal organs, such as want of appetite, headach, &c. But the most striking fact against this view of the disease, is the latent state in which the miasma may continue ere it calls forth the disease. This period is very different in different individuals. It has been latent for eighteen months. But further, Piorry has seen cases of intermittent caused by mechanical irritation of the spleen, blows, &c. Hence, says our author, they are in error who, without additional complications, reckon intermittents among *essential* diseases.

Our author decidedly adopts the view, that enlargement of the

spleen is a consequence of the fever :—but the question remains, does this secondary enlargement originate from the specific effect of the fever upon the spleen, or from the diseased spinal cord, or from any other cause? The derangement in other parts of the digestive apparatus militates against the idea of any specific effect. An etiological specific relation of the spleen to intermittent fever is therefore untenable.

In the section on the connexion betwixt enlargement of the spleen and typhus, our author gives us nothing beyond what we have already seen in Louis and Rokitsansky.

In the second part of the book, which treats of the special diseases of the spleen, our author commences in the first chapter with an exposition of the anomalies depending on increased vital activity—hyperemia with subsequent hypertrophy or neuralgia; and of those depending on diminished vital activity—hyperemia and tumour with change of structure, under which are comprehended the fibrinous, fatty, albuminous, and serous dyscrasiæ—tumours in connexion with degeneration of the lymphatic system, as distinct from those caused by hyperemia. In this head is included the development of whitish or greyish red, soft, creamlike, vesicular bodies, of the size of a millet seed, in the pulpy substance of the spleen. This condition is observed not only in typhus and the typhoids, but in nearly all the exanthematous disorders, especially in scarlatina, variola, erysipelas, acute rheumatism, gout, croup, &c.

Enlargement and hyperemia, although the most frequent, are not the only consequences of diminished vital activity. The capsule of the organ becomes abnormally rigid, and in such cases, instead of hypertrophy, atrophy and phlebitis ensue. To the same cause—diminished vital activity—are also to be attributed, tendinous spots, cartilaginous, osseous, and calcareous deposits. In the second chapter the author treats of the inflammatory affections, under the heads of superficial inflammation of the spleen (episplenitis), and deep inflammation (splenitis), with their origin and course, both in an acute and chronic form, and their various terminations in resolution, hepatization, supuration, softening, and gangrene. In his delineation of these states, he follows closely the descriptions of Gunsberg and Rokitsansky. Under the term hepatization he means a deposition of fibrinous, well-formed exudation in the glandular substance of the spleen. The organ feels rigid and hard, and is incapable of performing its functions. It may either retain its original size, be enlarged, or atrophied. In all the three cases the glandular substance has a fleshy pale red look, in consequence of the gradual dissolution of the blood corpuscles, and absorption of the colouring matter.

The mass of exudation, the result of secondary peripheral inflammation (perisplenitis), is very remarkable. Masses of sharply defined wedge-shaped bodies are imbedded in the peripheral substance, and in such a way that the end in contact with the covering is broad, that in the substance pointed. They are found in number to the extent of two, three, four, or more, and are from the size of a pea

to that of a hen's egg. In rare cases they may even occupy a third part of the organ.

The third chapter is devoted to pneumatosis. The only proof of such a state occurring which he brings forward, is a case by Mr M'Nab, detailed in the *Medical Gazette* for May 1843.

In the fourth chapter our author treats of the various degenerations or after formations—those which have been observed and described hitherto are melanosis, lipoma, steatoma, hydatids, acephalocysts, scirrhus, and tubercle. Of these, the latter is stated to be the most frequent.

The fifth chapter treats of rupture and wounds of the spleen and its vessels. Cases of the former are mentioned as having been seen by various authors occurring in typhus, and in intermittents in the cold stage. The rupture may be straight, angular, or round. It may be confined to one spot, or extend to several, or pass through the whole substance of the organ. The diagnosis is very obscure, and in illustration of this obscurity the following case of lesion of the omentum is given. A hussar, in leaping his horse over a dike, experienced, the instant the animal's feet touched the ground, a feeling as if something had given way in the chest. In the course of the day there was pain under the short ribs, cough, and hemoptysis. The abdomen became swollen, and there was severe pain in the left hypochondrium. In the evening a small rapid pulse, facies hippocratica—death the following morning. On dissection, the omentum was found torn at its left side to the extent of an inch and a half, and there were five ounces of blood extravasated into the cavity of the abdomen.

The sixth and last chapter is a collection of the various congenital malformations of the spleen, as regards form, position, and number.

In conclusion, a few cases illustrative of the doctrines contained in the body of the work are given. These are, first, a case of hypertrophy of the spleen in intermittent fever, by Young, cured by the actual cautery. Second, a case of extirpation of the enlarged organ, by Quittenbaum. The patient, a woman, survived only six hours. Third, a case of aneurism of the splenic artery, with rupture and death, by Nerier; and fourth, a case by the same author of suppression of the catemenia, followed by inflammatory tumour of the spleen, and insanity.

In the chapter on treatment, our author mentions a variety of remedies which have been used in diseases of the spleen. Of these, we can only allude to three: blood-letting, mercury, and iron.

The first of these our author recommends to be used generally in the acute affections, and locally in the sub-acute or chronic, by means of cupping-glasses or leeches. "In our day," he remarks, "statements have been put forth, both by those whose opinion is entitled to some weight, and by those whose opinion is entitled to none, to show that venesection in all inflammatory diseases is a remedy not only useless, but pernicious. This is not the place to enter upon a refutation of this paradoxical and useless (unheilbringenden)

theory." But he is satisfied that, in all acute diseases of the spleen at least, experience in every age contradicts it. Mercury, however useful in diseases of the liver, must, he thinks, be exhibited with caution in those of the spleen, and is only useful in the first stages of the disease. Of iron he speaks highly, and believes, if there be such a thing as a specific for diseases of the spleen, it is to be found in that remedy. It is to be had recourse to whenever the inflammatory stage is over, and before the digestive organs have become too much affected. It acts by exciting the spleen to renewed activity, while it furnishes the organ with the necessary material for the formation of one of the most important elements of the blood-hæmatin, and thus, by its action on the functions of the spleen, it has the most beneficial effect on the whole process of hæmatosis.

We have been led to notice this work at greater length than we originally intended; but the subject is interesting, and little known. To those who wish to pursue it further, we refer to the work itself, assuring them that they will there find every thing that has been noticed on the subject by others, although little has been done by the author. The work, therefore, is deficient in originality, and in that kind of observation which alone gives interest to such a subject. We cannot conclude without the remark, that it is vain to hope for any rational or satisfactory mode of treating the diseases to which the spleen is subject, till we are further and better acquainted with both its physiology and pathology. To these two subjects we would specially direct the attention of our younger readers, as a field in which organic chemistry and microscopic research have yet much to yield.

Practical Observations on certain Diseases of the Chest, and on the Principles of Auscultation. By PEYTON BLAKISTON, M.D., F.R.S., &c. London: 1848. 8vo. Pp. 368.

WE gladly hail the appearance of this work, not only as a fit tribute to the memory of Avenbrugger and Laennec, but as affording at the same time an admirable, though concise and *elementary*, view of the present state of this branch of physical diagnosis. "The principles of auscultation," says the author in his preface, "have been purposely developed in a very elementary manner; for it must have been felt by all those who are engaged in clinical teaching, that one of the greatest difficulties they have to contend with, arises from the incomplete preliminary education of some (he might have said *many*) of their pupils, and their imperfect acquaintance with the laws of physical science." With these remarks—which are very well timed—we entirely concur; and beg to call the attention of examining boards, and of those having the control over medical education, to this lamentable defect in the first principles of scientific knowledge,

that they may forthwith apply the remedy which as yet is in their hands, and thus assist in raising the mass of the profession to that position to which those practising the important art of healing, when under the guidance of scientific truth, are entitled.

The first chapter contains an excellent summary of the laws of sound, especially as bearing upon the subject-matter of the work. In the next chapter the sounds elicited by percussion are considered, very shortly, though in a sufficiently correct and philosophic manner. The author, however, objects to the mapping-out of the chest into different regions, as bewildering to the mind of the student; and, we doubt not, that the first view of this subject should be addressed to the latter as simply and generally as possible; but that the student must afterwards study the chest in a *regional* manner, we have as little doubt, and this in order that he may fix in his mind the connexion existing between the indications elicited by percussion and the physical organisation of the parts beneath, as well as of the superficial parts struck, which together give rise to the various modifications in the signs elicited from different regions. "In the following pages," says the author, "the locality of each sign is simply denoted by its relation to certain points, such as the vertebræ and different ribs, the sternum, the clavicles, the nipples," &c.—P. 52. The use of the pleximeter and percussion-hammer is not recommended, the author having "found the ordinary simple method (with the fingers) sufficient for all useful purposes."—P. 53. In this statement, however, we cannot agree; for cases are frequently occurring in which difficulties arise when diagnosis is attempted with the fingers alone, which, in consequence of the superior power of the hammer to elicit a clearer sound, are at once dispelled by the use of this instrument. Besides, in clinical teaching, where the sounds are to be made known to many persons at the same time, the louder and more distinct result from the hammer must be regarded as a decided advantage. We recommend the author to try it in his wards.

The third chapter contains an account of the "auscultation of the sounds of respiration," and embraces all those acoustic indications which arise during the act of breathing, including the pleural friction, or "attrition sound" as termed by the author. On the "formation of the natural respiratory sound" the following useful remarks occur:—

There are certain movements which take place during respiration, and which may consequently give rise to sound—These are,—

1. The movement of the frame of the thorax, which, turning on its hinges at the spine, rises and falls during respiration.
2. The contraction of the muscles, by which the movement is effected.
3. The gliding of the lungs on the ribs during their expansion and retraction.
4. Movements within the structure of the lungs.
5. The movement of the air rushing in and out of the lungs, and impinging on the air passages, from the mouth to their vesicular terminations.

All these movements may possibly produce sound; but the question to be solved is, which of them produce such an amount of sound as to be appreciable to the ear, placed on the chest and trachea?—P. 17.

In discussing this question, the author arrives, by the method of exclusion, to the following result, “that the pulmonary sound is principally formed by the air rushing through the smaller bronchial tubes.”—P. 21. The views advanced by M. Beau, and supported in a modified manner by several writers on this subject—among whom we may mention Barth and Roger, Sibson and Spittal—receive considerable attention from the author, though his conclusions are by no means favourable to them. “It would appear,” says he, “that the sounds engendered in the mouth and trachea are so weakened by divergence, and by destruction in the non-homogeneous tissue of the lungs, that they do not form a principal part of the pulmonary sound.”—P. 20. Our limits prevent us from entering the lists with the author on this subject, though we are quite prepared to do so—not for the purpose, however, of showing that the respiratory sounds are entirely caused by the aerial friction-sound occurring at the upper part of the respiratory apparatus—chiefly in the region of the glottis—but that a sound thence arises of a very distinct and decided character, being no less than the loud laryngeal sound itself, which, according to the ninth acoustic axiom of the author, must be transmitted throughout the bronchial tree. The axiom is this. “The waves of sound diverge in all directions, and hence the intensity of a given sound decays in receding from its origin as the square of the distance increases.”—P. 5. What, then, is there to prevent the laryngeal murmur from descending throughout the lungs? These organs are composed of a series of tubes and cells containing air, and in direct communication with the source of the sound. In our opinion, *nothing*. Besides, does it not appear that the conditions are very favourable for the transmission of the sound mentioned? In his commentary on the ninth axiom, the author says, “There are two ways in which the decay of sound may be lessened. The one by confining its waves, if aerial or fluid, respectively in tubes containing air or water,” &c.; and further on he remarks, that “the wind has a remarkable effect in thus increasing or diminishing the intensity of sounds in certain directions, according as it blows to or from the point of hearing.”—P. 5. These two circumstances appear to us a sufficient reason why the sound under consideration should not only be conveyed freely through the air-tubes; but, at the same time, why its intensity should be diminished during expiration, when the direction of the current of the air in breathing is altered. We hold, in short, that the laryngeal sound cannot be dismissed as nearly, if not altogether, useless in the production of the inferior respiratory murmurs. This does not seem to us to have been proved; at the same time, we do not insist on more than its being *one considerable* cause of these murmurs, which possibly have their origin *in part* from the

friction of the air in some portion of the inferior bronchial tubes themselves at the same time. We demur to the correctness of the fact, and, of course, to the argument drawn from it, as conveyed in the following passage:—"If the pulmonary sound were formed in the mouth and larynx, and thence transmitted to the surface of the chest, *a fortiori* the stronger sound of the voice would be conveyed there also. It will presently, however, be seen, that in health no resonance of the voice is perceived over the greater part of the chest."—P. 20. While we admit that the voice-sound is very much modified, being muffled, as it were, in passing through the pulmonary structure; at the same time, we cannot agree to the above statement, which is contrary to the observations of all previous writers, that *no* vocal resonance is perceptible over the general vesicular tissue of the lung. The vesicular voice-sound is undoubtedly very obscure compared with the bronchial or tracheal, and this may be accounted for perhaps in four ways:—1st, The direction of the current of the air during speaking, has a tendency to carry the sound from the walls of the chest.

2^d, As speaking almost invariably takes place during expiration, the vesicular tissue at the pulmonary surface will most likely be the first to lose the air from its cells, by which the aerial connexion with the bronchi will be destroyed; and this remark is also applicable to the case of the respiratory sounds, and may explain in part the diminution of the vesicular expiratory sound.

3^d, The musical character of the voice sounds, as to pitch especially, may have some influence in modifying the vesicular resonance; for, as is well known, the normal vocal resonance of the chest is, *cæteris paribus*, greater according to the graveness of the voice.

4th, Without care in observing, the vesicular vocal resonance may be regarded as very obscure, or altogether overlooked, in consequence of the intensity of the sound of the voice as it arrives at the ear that is *not* on the stethoscope. To observe it distinctly, the latter should be closely stopped by the finger.

We pass over the fourth chapter on the "sounds of the voice," to make a few remarks on the next, which embraces the "auscultation of the sounds of the heart." There, as with respect to the respiratory sounds, the author commences by pointing out the "movements of the heart which may be supposed capable of producing sound:" viz.—

1. The rubbing of the heart on the pericardium.
2. The striking of the heart against the ribs.
3. The collision of the particles of blood with each other, and with the interior of the heart and large vessels.
4. The collision between the internal surfaces of the ventricles after the expulsion of the blood.
5. The motion of the different valves.
6. The movement of the muscular fibres of the walls of the ventricles during their contraction and dilatation.—P. 36.

After analysing these different movements, the author arrives at

the following conclusions :—That the first or “systolic sound of the heart is caused by the friction of the muscular fibres of the ventricles, *inter se*, and the tightening of the auriculo-ventricular valves ; strengthened in certain cases by the impulse of the heart against the ribs, and by the collision of the blood against the orifices of the aorta and pulmonary artery,” p. 41 ; and that the second or “diastolic sound is produced by the unfolding and tightening of the arterial valves, and the stroke of the whole column of the blood against them,” p. 38,—views which do not materially differ from those of recent writers on this subject.

Though generally pervaded by clear and philosophic views, the work is not free from error—and, as an example of this, the result, probably, of haste in composition—we make the following quotation :—“It is hardly necessary,” says the author, “to remark, that the sounds of the heart are *propagated* up to the ear with greater intensity than usual, in proportion as the walls of the chest are thin and elastic, and as the spongy lung between the heart and the ribs is replaced by solids or liquids, as when the lung is consolidated, or when fluid is effused between the pleural surfaces. In these cases the sounds of the heart are often heard with great distinctness over spots of the chest which they do not in general reach.”—P. 42. That liquid effusion into the pleural cavity, has the effect of propagating the sounds of the heart to the surface of the chest, is contrary to the observation of all previous writers, and not borne out by the cases recorded by the author, in not one of which was this indication stated to have been observed. Indeed, except in the case of a very limited amount of liquid effusion disposed in a laminar manner between the pleural surfaces—when *ægophony* is usually perceived—all such effusions act in a muffling manner, not only to the heart sound, but also to those of the voice, the respiration, and cough.

The greater portion of the work under consideration embraces an extensive and interesting series of cases illustrative of the principles laid down. Into an analysis of these our space will not permit us to enter ; and, in now concluding this imperfect notice of the work, which we sincerely recommend to the attention of the profession, we beg in an especial manner to address the following admirable observations of the author on the practice of auscultation, to the managers of all public hospitals connected with medical schools, as well as to medical boards generally, that they may know the requirements of the times and make their arrangements accordingly :—

What splendid opportunities for practical study occur in the wards of many of our hospitals ! And yet how often are they left unimproved ! And how frequently does the physician painfully feel, that he is unable fully to perform one of the most important and pleasing duties of his office, the instruction of the student in the art of applying the various branches of knowledge to the discovery and treatment of disease ! This, in some degree, arises out of the present system of medical education. Compelled to attend lecture after lecture during a great part of the day, the student has not time to devote both to medical and surgical practice at the hospital, without exercising such an

amount of self-denial as few are disposed to submit to. This evil might, to a certain extent, be remedied by causing the lectures on medicine and surgery to be delivered at the hospital, and by making them consist, in part, of examinations of each pupil in rotation, and in presence of the whole class, on certain cases in the wards which he had previously been directed to watch; and also by conducting a part of the examination for diploma at the bed-side, when the examiners would be enabled, among other things, to test the practical knowledge of auscultation possessed by the candidate.—Pp. 59, 60.

Erelong some such system as this, in contradistinction to the comparatively irksome and useless book-learning picked up in the closet and at lecture merely, or crammed by the grinder, must be adopted, and the sooner the better.

The Plant; a Biography. In a series of Popular Lectures. By M. J. SCHLEIDEN, M.D., &c. Translated by ARTHUR HENFREY, F.L.S., &c.; with Five Coloured Plates, and Thirteen Wood Engravings. London: 1848. 8vo. Pp. 365.

As is well observed by the distinguished author of these lectures, a large proportion of the uninitiated, even among the educated classes, are still in the habit of regarding the botanist as a dealer in barbarous Latin names, a man who plucks flowers, names them, dries and wraps them in paper, and whose whole wisdom is expended in the determination and classification of this ingeniously collected hay. To all such we especially recommend the perusal of this volume, containing as it does the most masterly sketches of the subjects discoursed on, the most penetrating and far-seeing scientific truths, and a playful and imaginative style that cannot fail to interest the reader. It is without exception the most interesting botanical work, both for the man of science and the general reader, that has ever appeared from the press. The former will derive from it views of utility; the latter, ideas which must direct his mind to scientific thought, and as a general result both will derive benefit.

There are twelve lectures, each of which is complete in itself, and independent of the others, although of course there is a certain identity of subject which connects them together.

The first lecture is entitled the Eye and the Microscope. A plant, says the author, is not like a crystal, a homogeneous body, to fathom the nature of which it suffices to know the substance composing it, and its external form. It is built up of many minute and most curiously formed cells, filled with most varied matters, an examination of which must precede all other considerations. These little bodies cannot be seen by the unassisted eye, and hence the microscope is as necessary for the botanist as it is to the physiologist. Now, there are many persons labouring under the delusion, that for microscopical researches merely an eye and an instrument are necessary, and all may be done. But not only is the use of the microscope an art to be acquired only by considerable pains; but

scientific vision, with even the naked eye, has its difficulties. Hence the necessity of indicating the point of view from which the use of the eye and microscope is to be regarded.

The illustrations brought forward by our author, as proving the utility of optical instruments, are very happy, and well capable of arresting the attention of an educated audience.

We have accompanied a parting friend as far as the hill beyond the town, once more we embrace him, once more gaze long and deeply on his countenance, to impress more firmly on our soul each dear, familiar feature. At last he leaves us, hastens thence, while we stand lingering, gazing after him. He turns, and still we recognise the well-known face. But the distance continually increases, and by degrees the peculiarities of shape vanish. A turn in the road hides him from us for a while; then he emerges yet again on the slope of the farthest hill, a little, moving, black point; he stops, waves his handkerchief, but we are scarcely able to distinguish this motion, and at last he disappears wholly in the distance. The farther our friend retreated from us, the less distinctly could we see him, the smaller he appeared, till at last a pin's head held before the eye would have been the larger. While we here remark how an object well known to us becomes gradually smaller, and at last totally disappears, we become aware of the means by which we can enlarge an object, so as to see it more distinctly and distinguish a greater quantity of separate parts in it, namely, by bringing it nearer to the eye.

It is assumed, that on an average, the human eye sees distinctly at the distance of ten inches, but not when the object is brought nearer. Now, if I use a glass which permits me to see the object distinctly at a distance of five inches, it appears twice as large; at a distance of two inches and a half, four times; at one tenth, a hundred times as large, and so on; in a word, the enlargement depends alone upon the degree of proximity to the eye into which the object is brought. * * * —Pp. 26-28.

On the 26th of January 1843, a great crowd collected at the Round Down Cliff, near Dover, in anxious expectation to witness the event of the grandest and most daring blasting ever attempted by the skilful combination of human ingenuity. The labour of years had been expended on the preparations, in the opening of shafts and galleries. The largest quantity of powder ever yet used, 185 cwts., was ignited at once by means of a gigantic galvanic battery. Almost in silence was the enormous cliff hurled into the sea; in one minute were a million tons of chalk torn away, and a surface of almost fifteen acres covered twenty feet deep with its fragments. From this may be estimated the tremendous force which must have been exerted. And with what did the power of the human mind enter into this gigantic struggle? With the remains of creatures, a thousand of which might be annihilated by the pressure of a finger. We wonder, and ask ourselves: What does "small" mean, in Nature?

There can, however, be no possible doubt that it indicates a most barbarous age, or a very low state of refinement, when the value, the importance of a thing is measured by great and small, a standard indeed which finds no application in all that we know most essential and valuable; for the human mind is not to be defined by foot, inch, or length. Physical magnitude imposes only on the sensuous nature; cultivated man seeks to know the object of his contemplation perfectly in all its relations; and then only, from the perfect knowledge, does he permit himself to judge as to the essential and inessential; very frequently this leads him to declare that the most significant which has the smallest dimensions.—Pp. 35, 36.

In the same manner it must be clear, that a knowledge of the minute structures in man, as well as plants and natural objects generally, must throw a new light on all the processes which go on

within the body, whether in its healthy or morbid conditions. And it is equally evident that the microscope, which enables us to study these, must constitute a distinct "turning point" in the knowledge of the more minute organisms of the animal and vegetable worlds.

Passing over the next four lectures on the internal structure of plants; on the propagation of plants; the morphology of plants and about the weather, we arrive at the sixth and seventh lectures, which treat of what does man live upon? The answers, says the author, will be various enough.

The Gaucho who in the wide Pampas of Buenos Ayres, managing his half-wild horse with incredible dexterity, throws the lasso or bolas to catch the Ostrich, the Guanaco, or the Wild-bull, consumes daily from ten to twelve pounds of meat, and regards it as a high feast-day, when in any hacienda he gains a variety in the shape of a morsel of pumpkin. The word bread does not exist in his vocabulary. The Irishman, on the other hand, regales himself in careless mirth on his "potatoes and point," after a day of painful labour—he who cannot help making a joke even of the name he gives to his scanty meal. Meat is a strange idea to him, and he is happy indeed, if four times a-year he can add a herring to season the mealy tubers. The hunter of the Prairies lays low the buffalo with sure bullet; and its juicy, fat-streaked hump, roasted between two hot stones, is to him the greatest of delicacies. Meanwhile, the industrious Chinese carries to market his carefully fattened rats, delicately arranged upon white sticks, certain to find a good customer among the epicures of Pekin; and in his hot, smoky hut, fast buried beneath the snow and ice, the Greenlander consumes his fat, which he has just carved, rejoicing over the costly prize, from a stranded whale. Here the black slave sucks the sugar-cane and eats his banana, there the African merchant fills his wallet with sweet dates, his sole sustenance in the long desert journey; and there the Siamese crams himself with a quantity of rice, from which an European would shrink appalled. And wheresoever over the whole inhabited earth we approach and demand hospitality, in almost every little spot a different kind of food is set before us, and the "daily bread" offered in another form.—Pp. 132-3.

Yet all these varied kinds of nourishment are, in point of fact, principally composed of four simple elements, viz. nitrogen, oxygen, hydrogen, and carbon:

"Four elements
In one firm band
Give form to life,
Build sea and land."

"Vier elemente
Innig gesellt
Bilden das Leben,
Bauen die Welt."—SCHILLER.

This subject leads the author into a consideration of vegetable food, and its importance to the existence of the animal world. He next speaks (Lecture VIII.) on the milk sap of plants, and its use in the arts, of which caoutchouc offers a well-known example, and this induces him to speak of one curious tribe of plants in particular, namely, the Cactus tribe (Lecture IX). The work closes with two most interesting lectures on the Geography of Plants, and the History of the Vegetable World.

We have read this book with unabated interest throughout, and cordially recommend it to all our readers. Both from the subject-matter and elegance with which it has been got up, it is well adapted for the table of every well-educated individual, whether in "hall

or bower;" but to such places, we fear, any praise contained in our dry medical pages will not be considered as a fitting or appropriate introduction.

Du Cœur, de sa Structure est de ses Mouvements. Atlas de 9 Planches.

Par M. PARCHAPPE, Medecin-en-Chef de l'Asile des Alienés de la Seine Inferieure, &c. Paris: 1844. P. 192.

Of the Heart, its Structure and its Movements. Atlas of 9 Plates.

By M. PARCHAPPE, M.D., &c.

IN this work M. Parchappe has given a very elaborate description of certain parts of the structures of the heart, and discussed some points in its physiology. He has described very minutely the external configuration of the heart, and the interior of its auricles and ventricles, and has evidently bestowed much labour and patience in attempting to unravel the muscular fibres of this organ; but he has passed over the tendinous structures, the mode of attachment of the arteries to the heart, the relative thickness of the different chambers of the heart, and their relative capacities, the relative size of its arterial and auriculo-ventricular orifices, &c. He has carefully studied the phenomena which attend the movements of the heart, and discusses at some length the causes of the two sounds that attend those movements; but he only notices cursorily, or not at all, various points usually discussed in dissertations on the physiology of the heart. Of course M. Parchappe was well aware of all this, and appears to have written upon those parts only of the structure and physiology of the heart, which he had made an object of special study. At the end of the description of the structure and movements of the human heart, he has given us an anatomical description of the organ in the eel, the turkey, and some of the more common mammalia. He has also, before entering upon his account of the movements of the heart in the human species, described the anatomical arrangements and the movements of the heart in the frog, the toad, and the rabbit. We were a little surprised to find that M. Parchappe, in his description of the heart in the batrachian reptiles mentioned, viz. the frog and toad, still adhering to the old opinion, that it consists of a single ventricle and a single auricle. In describing the auricular part of the heart in the *Rana temporaria*, he says, "Une bride membraneuse antéro-postérieure, constitue, au-dessus de cette ouverture, dans la cavité auriculaire, une trace de cloison."—P. 96. No doubt he could cite the authority of Cuvier and Meckel in favour of the statement he here makes, that there is only one auricle in the heart of the frog; but it was demonstrated first by John Hunter, afterwards by Dr John Davy and Mr Owen, that the heart of this animal consists of two auricles and one ventricle.

M. Parchappe's description of the external and internal configuration.

ration of the auricles and ventricles of the human heart, is very correctly drawn up, and is exceedingly minute, in our opinion considerably too minute for any useful purpose. We are no advocates for sketchy descriptions in anatomy; on the other hand, we have always inculcated the necessity of entering carefully and correctly into full details in describing either healthy or diseased structures; but it is obvious that this may be overdone, the description may be loaded with too many minute and trivial details, which burden the memory without adding in the least to the clearness of the conception by the reader of the objects described.

The power of describing clearly and vividly anatomical arrangements of parts, is an art which appears to be too little studied by anatomists. Every one who has occasion to read much of their writings must feel, that in many cases there is a want of lucidness and order which sadly fatigues the mind in attempting to follow them. We have not so much to complain of want of clearness of description on the part of M. Parchappe, as of describing at length parts of objects which it is totally unnecessary to describe so much in detail.

The description of the arrangement of the muscular fibres of the heart is very carefully drawn up, and is illustrated by several well-executed lithographs. It is well known that the intimate arrangement of these muscular fibres, particularly those of the ventricles, is exceedingly complex; and, notwithstanding the labours of Winslow, Wolff, Gerdy, the late Dr Andrew Duncan, jun., and many others, it has long been considered a kind of gordian knot in anatomy. As it is necessary that the contractions of this organ during its systole should not take place in one direction only, but in all directions, otherwise the blood could not be efficiently driven from its interior along the arteries, we would, *a priori*, predicate, what is actually the case, that the muscular fibres should run in all directions. After having read the very minute descriptions given of the fibres of the heart by Wolff and others, and worked patiently ourselves at this subject upon boiled hearts, we came to the conclusion that, in examining the course of the fibres of the ventricles, it is unnecessary to describe each particular band of fibres, and that the description should be confined to their general arrangement. The fact is, that when anatomists begin to describe particular bands of fibres in the heart, the size and extent of each band described is so much a matter of choice that we find no two descriptions the same.

In his account of the movements of the heart, our author enters at some length into a consideration of the manner in which the auriculo-ventricular valves perform their office, and he has adopted the view—of the correctness of which, we believe, there can be no doubt—that the auriculo-ventricular valves are approximated by the contraction of the columnæ carneæ, to which the cordæ tendineæ are attached—a view advocated several years ago in this country by Mr Mayo, and afterwards supported by Bouillaud and others.

M. Parchappe discusses at considerable length the various explan-

ations which have been given of the two sounds of the heart. He seems to have come to the conclusion, that the sounds of the heart are due to the shock of the fluid passing through it; the first sound being caused by the shock of the blood against the ventricular walls, and the second sound by the shock of the blood against the arterial walls.—P. 156. This theory, he says, of the normal sounds of the heart, will be in perfect conformity with the opinion which regards the impulse of the heart against the chest and the ventricular diastole as synchronous. We could not resist a feeling of deep regret at reading the above passage; for we had fondly hoped that the time had arrived when the cause of the second sound would, without further disputation, be referred to the sudden approximation of the semilunar valves, and that the impulse of the heart and the systole of the ventricle had been beyond a doubt considered to be synchronous.

A Dispensatory or Commentary on the Pharmacopœias of Great Britain; comprising the Natural History, Description, Chemistry, Pharmacy, Actions, Uses, and Doses of the Articles of the Materia Medica. By ROBERT CHRISTISON, M.D., V.P.R.S.E., &c. &c. Second Edition, revised and improved, with a Supplement containing the most important new remedies. Edinburgh: 1848. 8vo. Pp. 1013.

THE rapid exhaustion of the first edition of this work, which has been long out of print, will cause the present volume to be received with the greatest eagerness by the professional and pharmaceutical public. Considering the enormous amount and variety of matter it contains, he must indeed be a bold critic who will venture to grapple with its contents. A commentator on the first edition arose in the person of Mr Richard Phillips, who, we are informed in the preface, with a pertinacity of criticism unexampled in these days, has during several years exercised his censorship on the book, from time to time, in three distinct periodicals, and in an express publication. This in itself is no small proof of the extreme value of the Dispensatory; and we can assure our readers, that the present edition contains an enormous amount of new matter, with a supplement containing an account of the more important new remedies not noticed in the last Edinburgh Pharmacopœia. But what gives it a distinctive value, and a superiority over all other similar works, is the condensed information, comprising only what is valuable in the subject treated of, which characterises it throughout. It is this judicious selection, and the evidence every where evinced of personal knowledge and research, that stamp upon the book a degree of importance and authority, that is tacitly and irresistibly acknowledged by all who consult its pages. We are therefore of opinion, that in drawing the attention of our readers to this second and improved edition, we do all that is necessary, and indeed all that the medical press is capable of.

Treatise on the Falsifications of Food, and the Chemical Means employed to Detect them. By JOHN MITCHELL, M.C.S., &c. London: 1848. 12mo. Pp. 334.

NEARLY thirty years have elapsed since the celebrated "Death in the Pot" was published by Mr Accum, which clearly demonstrated that every article of food was subject to some form or other of poisonous admixture. Since then, our author informs us that many new adulterations have been introduced which demand a chronicler, while the advances of chemistry has enabled us to attain more ready and certain means of detecting them. Hence the present work. We regret, however, that Mr Mitchell has confined himself to the chemical means only of discovering impurities in food, because there are many cases in which the microscope enables us to do this with much greater facility. How easy, for instance, may the adulterations of milk with flour and other substances, be thus determined?

Nearly all the substances used as food are adulterated to an extent that would at first sight be deemed almost impossible. These are practised with a view—1st, of making the substance more saleable, by improving its appearance by the addition of some body, either innocuous or otherwise; 2d, of depreciating its quality, by adding to it some substance which will diminish its real, without altering its *apparent* strength or general appearance. This is generally a very deadly fraud. 3d, Of depreciating its quality by the addition of some simple substance, as water; or, if it be a solid body, as plaster of Paris, sand, &c. These comprise the quality of frauds, not mentioning, however, short weight and measure, both of which are too well known to need much comment.

The substances most subject to adulteration are wines, spirits, beer, tea, coffee, sugar, bread, chocolate, milk, cream, pickles, oil, flour, cheese, mustard, pepper, confectionary, and vinegar. Each of these our author passes under review in distinct sections, treating previously, however, of the vessels in general use for the preparation and preservation of articles of food, and of water and its impurities.

We can confidently recommend this little work of Mr Mitchell's as one of a very useful character; and, so far as the chemical means for detecting adulterations in food are concerned, it is written in a very able and judicious manner.

Ventilation Illustrated: a Tract for the Schools of Rich and Poor.
London: 1848. Small 8vo. Pp. 36.

It has always been a doubtful question with us, whether the numerous tracts, put forth by philanthropic societies for the instruction of the people, are really beneficial or the reverse. Certain it is that popular views of science are generally deserving very little attention, and originate more in the speculations of booksellers, or in the desire

of obtaining notoriety, than in a sincere desire to benefit the class to whom they are addressed. At the same time it must be confessed, that there is no class of society better acquainted with the evils engendered on mankind by an ignorance of the simplest physiological truths, than medical men. It arrests them in every stage of their duties, in the palace and in the cottage, among rich and poor; and it may be said that ignorance of the laws of nature, more especially as applied to the animal body, is one of the chief obstacles to the progress of rational medicine.

Those who have seen much of disease among the poor, but more especially among the Irish poor, will feel satisfied that much would be done to relieve their sufferings if they were only enlightened on one point. That point is ventilation. Now, if the circulation of a tract be the means likely to arrive at that end, we cannot conceive it possible to produce one better adapted for the purpose than the one before us. The style is plain and easily intelligible. The subject-matter consists of a sketch of the respiratory and circulatory apparatuses in man, showing the necessity of fresh air for the proper performance of the animal functions, and pointing out how this can easily be secured in all inhabited rooms. The whole is profusely illustrated with excellent woodcuts.

We can truly say that there is no class of persons, high or low, that may not benefit by the perusal of this admirable tract, and we should like to see similar concise sketches on cleanliness and food, from the pen of the same able author.

Part Third.

MEDICAL NEWS.

EDINBURGH OBSTETRIC SOCIETY.

SESSION VII.

MEETING IV. March 12.—Dr SIMPSON, President, in the Chair.

ON RUPTURE OF THE UTERUS FROM HYDROCEPHALUS IN THE FÆTUS. BY DR SIMPSON.

Dr Simpson described two cases to which he had recently been called, and in which fatal rupture of the uterus had previously occurred in consequence of hydrocephalus in the fœtus. Both mothers had borne large families without difficulty. In both cases their present labours were very prolonged, and the pains severe before the laceration occurred. And, it might perhaps be laid down as a general principle, that when a mother who had previously borne children with the usual facility and safety, suffered a long and difficult labour, with the head never properly entering the brim, dangerous enlargement of the head from hydrocephalus, or other causes, should be suspected, and the most careful means instituted in order to make out a correct diagnosis. In this, as in other important cases, if the patient were put into an anæsthetic state, great facilities would be given for making a careful and leisurely diagnostic examination of the child's head with the hand, without suffering or resistance on the part of the mother. The unusual size and dimensions of the head might be thus ascertained; but one source of fallacy was to be guarded

against, namely, that the sutures and fontanelles were not, as was usually described, always preternaturally open and enlarged in hydrocephalic cases; for the cranial bones were in some instances, where the internal effusion was very great, so largely and abnormally developed as to destroy this supposed pathognomonic sign, and form an almost complete osseous covering for the enlarged head. In one of the two cases described by Dr Simpson, the cranial bones were, in this way, as large as in a child two or three years old. When hydrocephalus was attended with imperfect ossification, the enlarged fluid head sometimes moulded itself so readily to the maternal passages as to pass without much difficulty even when of very great size. But the danger and difficulty was much increased when to this was added the impediment arising from enlargement of the bones themselves.

The danger of rupture of the uterus, and consequent death of the mother, was much greater under hydrocephalus than obstetric authors commonly described. In a collection of cases of intra-uterine hydrocephalus made this year by Mr Thomas Keith for his thesis, out of 74 instances of the disease which he had found on record, in 16 the uterus ruptured during labour. In fact, the diseased head of the infant, distended by the effused fluid, acted, under the impressions conveyed to it by the body and spine of the infant during labour, like a hydraulic bag or machine, pressing equally and in all directions on the cervix uteri, or parts with which it was in contact, with a force under which these compressed structures were almost certain to rend, provided the pressure were of any great duration, because the force itself contained the sum and concentration of the whole power of the uterine contractions bearing on each point with which the bag of the head was in contact. Under such compression any weak point, or fissured point, was almost certain to yield. And if any hydrocephalic head or fluid bag of this kind passed into the vagina and remained there, the kind of universal compression of the vaginal walls to which it gave rise, was apt, if it did not lead to direct rending and laceration, to produce, if long continued, sloughing inflammation in these parts.

Hence labours, in which the child's head was hydrocephalic, should not, if possible, be allowed to become prolonged; but it was not perhaps necessary to destroy entirely such an infant during labour by using the crochet or other mortal operative procedure. In some instances it was a matter of legal importance for the parents that the child should be born alive, though not capable of surviving. And a small trocar passed into the hydrocephalic collection was capable of evacuating the contained fluid as efficiently as a more extensive and fatal laceration by the usual obstetric perforator or scissors; nor was such an evacuation by the trocar necessarily fatal to the foetus, for we know that in the operation for hydrocephalus on the child after birth, by Dr Conquest and others, the simple puncture of the head was by no means a fatal operation. Those operated on seldom or never died of the operation itself, but of the subsequent re-accumulations and consequences.

The hydrocephalic foetus not infrequently presents by the feet or breech. In a case of this kind which occurred some time ago to Mr Girdwood of Falkirk, and where the child was dead and putrid, the spine and skin over it lacerated at the neck in making the usual tractions to bring down the head. A great quantity of water escaped from the laceration, and the enlarged head, which had previously resisted the force applied to it, was now easily extracted in its collapsed state. This case afforded a suggestion, that under a similar complication, with the body already born, instead of passing our hand and perforator to the brim of the pelvis in order to puncture the head, which was sometimes a matter of no small difficulty, we might attain the same end, of the evacuation by the hydrocephalic effusion, by the more simple and safe measure of opening the vertebral canal, in any part of the course of the spine.

MEDICATED PESSARIES. BY DR SIMPSON.

In diseased states of the cervix uteri and vagina, medicinal substances had been

applied locally to those parts under various forms, but principally, either in a solid state (as nitrate of silver, potassa, &c.), or in a liquid form (as in the great varieties of medicated injections in common use in leucorrhœa, &c.) When thus used, the local application was temporary, and applied for a few minutes only. But in various forms of disease it seemed an indication of no small importance to have the medicated substance applied continuously, and not temporarily. Medicated pessaries, which Dr Simpson had first introduced into practice several years ago, and which had since been extensively adopted by various practitioners in London and elsewhere (see descriptions of them published by Dr Stafford Lee, Dr Oldham, &c.), enabled us to fulfil this indication. By their use, for instance, we could keep the cervix uteri, when ulcerated and indurated, constantly embedded in mercurial or iodine ointment for weeks, and sometimes with the most marked benefit and success. They fulfilled another indication in cases of irritation and inflammation of the mucous membrane of the cervix uteri and vagina. They kept the opposed diseased surfaces from coming in contact, and it was well known how important a matter this was in the pathology of mucous and cutaneous surfaces.

Dr Simpson had been in the habit of applying a variety of substances in the form of medicated pessaries, particularly zinc and lead ointment, &c., as simple emollients; mercury and iodine as discutients (and particularly the iodide of lead); tannin, alum, and catechu, as astringents; opium, belladonna, &c., as anodynes. The pessaries were made of the size of walnuts, and could be easily introduced by the patients themselves; one or two in the twenty-four hours. They were composed of the medicine used, mixed up in the form of an ointment, and brought to a requisite degree of consistence with one or two drachms of yellow wax to the ounce of ointment. Messrs Duncan and Flockhart, druggists, had found the following proportions requisite in the subjoined forms, (those in most frequent use in Edinburgh); and they might serve as models for the others. After being made up in the proper form, they were usually coated by the druggists with a firmer covering, by dipping them into an ointment made up with wax and resin, kept liquid by heat. About an ounce of the different ointments made four balls.

1. *Zinc Pessaries*.—℞ Oxydi Zinci ʒj, Ceræ Albæ ʒj, Axungiæ ʒvj, Misce, et divide in pessos quatuor.

2. *Lead Pessaries*.—℞ Acet. Plumbi. ʒss, Ceræ Albæ ʒiss, Axungiæ ʒvj, Misce.

3. *Mercurial Pessaries*.—℞ Unguent. Hydrarg. Fort. ʒij, Ceræ Flavæ ʒij, Axungiæ ʒss, Misce.

4. *Iodide of Lead Pessaries*.—℞ Iodidi Plumbi. ʒj, Ceræ Flavæ ʒv, Axungiæ ʒvj, Misce.

5. *Tannin Pessaries*.—℞ Tanninæ ʒij, Ceræ Albæ ʒv, Axungiæ ʒvj, Misce.

6. *Alum and Catechu Pessaries*.—℞ Sulph. Aluminis ʒj, Pulv. Catechu ʒj, Ceræ Flavæ ʒi, Axungiæ ʒvss, Misce.

7. *Belladonna Pessaries*.—℞ Extr. Belladonnæ ʒij, Ceræ Flavæ ʒiss, Axungiæ ʒvi, Misce.

PLASTER-BELT IN ABDOMINAL TUMOURS, &c. BY DR SIMPSON.

Dr Simpson stated, that patients affected with pediculated ovarian tumours, large fibrous tumours of the uterus, &c., often suffered from the morbid masses being loose and mobile, and impinging on the bladder, &c., in different positions of the body. Patients sometimes instinctively applied their hands to the tumours, under such circumstances, to steady and fix them. In these cases different means had been tried, with the view of preventing the tumours rolling and moving—such as various bandages, air-pads of Mackintosh cloth, &c. The best and simplest means, however, consisted in surrounding the whole trunk with a continuous belt of lambskin or chamois leather, eight or ten inches deep, and shaped and sewed so as carefully and exactly to fit the loins and lower part of the abdomen of the patient, like a common abdominal bandage, and embossed in front so as to contain and include, as in a bowl or cup, the protuberant portion or portions of the tumour. To fix the belt, its interior

was spread with a plaster composed of one part of adhesive to two parts of soap plaster. It generally gave the patient much relief; abated the feelings of abdominal weight, and pressure and pain in the back; held the tumour steady; and could be applied so as even to compress it. In other cases where no tumours were present, but the abdominal parietes and contents were relaxed, or the spine weak, the same form of plaster often afforded a great degree of comfort and relief, and enabled patients to take exercise, &c., when, otherwise, they could not without fatigue and suffering. They generally required to be removed and renewed every four or six weeks.

NOMA—RECOVERY FROM, AFTER EXFOLIATION OF THE JAW AND EXTENSIVE SLOUGHING OF THE CHEEK.

Dr Keiller read a communication on *cancrum,oris* or *noma*, and related the particulars of several cases, in two of which recovery had taken place after exfoliation and sloughing of considerable portions of the jaw and cheek. One of the successful cases had occurred in the practice of *Dr Paton* of Dundee, the other was attended by *Dr Keiller*, when officiating some years ago at the Royal Dispensary. The following is *Dr Paton's* case, as communicated by himself:—

William Durward, æt. twenty months, born of healthy parents, with no scrofulous taint in either of them, has been always healthy until the above age, when he had an attack of measles, then epidemic, but of so mild a kind that no medical advice was required until a week or two after, when he was brought to me (May 7, 1845) for a troublesome diarrhœa, with apthous sores on the tongue, for which some powders of rhubarb and sulphate of potass, and a solution of nitrate of silver, were prescribed. The mouth got soon well, but the diarrhœa continued, and on the 22d May, a mixture, containing Tinct. of Hyos., Calomba, and Carb. Sod., with powders of rhubarb, aromatic and Dover's powder, with about half a grain of Hydrarg. Creta, was given every night for six nights. The discharges became much improved, but with extreme emaciation, and quinine was added to the mixture. From this time to the 7th June I had seen him daily, but he was then so much improved that I did not visit him for two or three days, when his mother directed my attention to his gums, which I found separating from the incisores of the lower jaw, and looking black. I applied the strong muriatic acid freely to the parts, and increased the quantity of quinine, giving a small quantity of port wine, and attending to the bowels and keeping the mouth very clean; in two days the sloughing process had extended to the lower lip, and on the 20th June there was a large hole through the lip; the applications were continued, but the process extended rapidly to the left side, and in a few days involved the whole cheek, when a line of demarcation appeared, and we were in hopes that the gangrene had stopped, but it again commenced, and extended upwards as far as the lower eyelid, and backwards to the angle of the jaw. At this time the case was considered one of a very hopeless nature, indeed recovery was not deemed desirable. Still the tonic treatment was continued, and it was wonderful with what avidity the little patient swallowed and relished his wine, beef-tea, &c. About the 26th June the gangrenous process stopped, and the sloughs began to separate, exposing the whole of the mouth, and leaving the malar bones bare. The granulating process soon filled up the gap, leaving, however, a very unseemly scar, and about the end of August the external surface was entirely healed over, and he continued to improve until a few weeks after, when his mother, observing something white in his mouth, put in her fingers and removed the accompanying large portion of jaw, containing two molar teeth. He is now a fine healthy child, and the deformity is not so great as might have been expected. The only inconvenience he suffers from is the opening of the parotid duct externally, and the consequent escape of the saliva; but this I mean to remedy so soon as he is able to see the benefit of it, and can be controlled during the treatment. A hard substance resembling gum now supplies the place of the exfoliation, there being of course no teeth on that side.

Dr Keiller exhibited the exfoliated portion of the jaw, with teeth, to the Society, and stated, that the case which terminated favourably under his care at the Royal Dispensary, in many respects resembled that of Dr Paton. The child was two years of age, feeble and emaciated, had a large belly, and deranged bowels some time after a mild attack of scarlatina. The breath soon became extremely offensive, and on examining the mouth the upper gum of the left side was found partially detached from the alveolar arch, the ulcerative process having already crept along the edge of the upper, and even extended and implicated that of the lower jaw.

The mucous membrane of the left cheek in a few days assumed a dark colour, and became studded over with greyish spots, which rapidly enlarged, causing copious and very offensive salivation. The application of nitrate of silver and the ordinary washes, which were used at this stage, had no effect in checking the disease; the alveoli were soon laid quite bare by the separation and destruction of the gums, and several of the teeth dropped out; the substance of the cheek gradually became more and more involved, being black and ulcerated internally, and swollen, hard, and of a somewhat pale and shining appearance externally, this palish colour gradually afterwards deepening until it became almost black, when an immense slough commenced to detach itself, and which, on removal, laid the cavity of the mouth on the left side extensively open. The teeth of the upper jaw dropped out, but no part of the bone exfoliated.

The treatment consisted in the application of nitric acid once, and muriatic acid twice, freely to the sloughy mass, and ultimately removing it with forceps and scissors; wine, quinine, and nourishing diet, together with a little nitric acid, being at the same time occasionally administered.

The child throughout seemed to suffer little or no pain, even when the gangrenous erosion was most extensive, and ultimately got quite well, an obvious retraction of the angle of the mouth being the only deformity left.

No *mercurial* preparation was given in the above case, nor in other cases which were referred to by Dr Keiller, who remarked that the occurrence of *noma* ought scarcely to be ever attributed to the incidental use of a few grains of *hydrargyrum cum creta* during the treatment of any of the numerous infantile diseases of which it generally appears as a mere *sequela*, such as measles or the other exanthemata, hooping-cough, dysentery, or other diseases inducing great debility in children of originally delicate constitutions, whose systems, with or without *mercury*, are little able to withstand the continued irritation and debilitating influence of prolonged disease.

Dr Keiller concluded from what he had seen and read of this exceedingly fatal malady (19 out of every 20 attacked by its more severe forms falling victims to its ravages), that the most efficient means of cure consisted in the most tonic and invigorating treatment that could be adopted; wine, quinine, nourishing diet, and nitric acid internally, and the free application of the most powerful acids, or even the actual cautery to the *surfaces subjacent* to the gangrenous sloughs, in order to excite to healthy action the vessels of the diseased and poisonous tissues.

Dr Burn had met with two cases of *noma* in children. In one, mercury had been given previously, but none in the other. They both did well under a nourishing diet, quinine, and the free local application of nitric acid.

Dr Moir lately met with one fatal case after scarlatina. Dr Lee had seen the disease in a woman who was seized with fever while labouring under scurvy. He had also seen it in a child with hooping-cough. In this case it proved fatal in three days.

Dr Simpson had seen a well-marked case of *noma*, which came on with no preceding fever, and no mercury had been given previously. The cheek became white and waxy in colour. A small vesicle or two then formed on its surface, at first clear, but soon filled with bloody serum; gangrene and sloughing then followed, and spread rapidly till the child sank.

CASES OF SPONTANEOUS AMPUTATION OF THE FOREARM, AND SUBSEQUENT RUDIMENTARY REGENERATION OF THE HAND IN THE FETUS. BY DR SIMPSON.

Dr Simpson showed the society a girl, aged eleven, who had been born wanting the left upper extremity from a short way below the elbow-joint. The arm of this side was of the natural size and form; but the forearm consisted merely of a stump about two inches long. It had all the appearance of having been amputated about the union of its upper and middle third, the surface having subsequently healed over in a very perfect manner. No appearance of cicatrization was visible except over the ends of the two bones, where the skin was puckered and drawn in in an umbilical form. Midway between, and a little in front of these two points, was a raised cutaneous tubercle, divided on the surface into five minute nodules, on two of which small points of nail could be detected. This projection *Dr Simpson* stated various reasons for believing to indicate an effort of nature to replace the lost portion of the limb,—he considered it to be in fact a rudimentary hand, and a curious illustration of the power of regeneration of even compound parts in the embryo and fœtus in utero.

Dr Simpson showed a great number of casts and drawings of other similar cases. The general resemblance of the cases to each other was very remarkable. In all, the amputation seemed to have happened at precisely the same situation; in all, the cicatricies over the ends of the two bones were well marked; and in all, there was a more or less marked indication of an attempt of reproduction of the lost portion of the member.

Dr Simpson also showed, as an illustration of the mechanism or production of spontaneous amputations, a child born in the Maternity Hospital recently, whose fingers and toes were in several parts semi-amputated by bands of coagulable lymph or false membrane—the result of inflammation of the cutaneous surface of the fœtus. The bands still existed at some points. There were the following deformities:—

In the *right* hand, the second, third, and fourth fingers were joined together laterally, in a somewhat conical mass. The index finger, the longest, ended in a transversely furrowed tuberculated mass. To the index was joined the fourth or ring finger at its apex, and, filling up the triangular interval between them, lay the third finger, having only the remains of one phalanx. The fifth or little finger ended abruptly at about the middle of its length, and had an osseous nodule representing the second phalanx. On its apex is a small crack, and a long dry filament is attached. All the fingers of the *left* hand presented circular constrictions of inconsiderable depth over their first phalanges. In addition, the fourth or ring finger seemed merely to possess a remnant of the second phalanx, and then abruptly terminated in a constricted tubercle. The *right* foot was normally formed. The great toe of the *left* foot was almost entirely absent, being represented merely by some irregular tubercles of skin. The second toe was supplanted by a soft bag of integument. The third and fourth toes had each, in place of their terminal phalanges, two constricted tubercles, without vestige of a nail. The fifth or little toe was well formed.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVII.

MEETING X.—*May 3, 1848.*—*Dr R. HAMILTON*, President, in the Chair.

CASE OF FISTULOUS COMMUNICATION BETWEEN THE INTESTINES AND URINARY BLADDER, WITH SOME REMARKS ON FISTULOUS COMMUNICATIONS BETWEEN VARIOUS VISCERA. BY *DR ALEXANDER WOOD*.

THE object of the paper was to show the means which nature adopted to prevent perforation of the intestinal canal, and the escape of the contents into the peritoneal sac.

The author first pointed out the frequency of ulceration of the coats of the canal, and attempted to show why this local consequence of inflammation was more liable to occur there than in the bronchi. He next attempted to ascertain the relation in point of frequency between ulceration and perforation, showing the comparative frequency of the one, and rarity of the other.

The effect of perforation, rapid and incurable peritonitis, was next adverted to; after which, the means taken by nature to prevent so serious a lesion were considered under four heads. I. The natural state of the tissues of which the coats of the canal are composed. II. The natural progress of the diseases by which ulceration is effected. III. The changes induced on the tissues by disease. IV. The changes produced on contiguous viscera by the disease.

AN ACCOUNT OF SOME INTERESTING CASES OF SPECTRAL ILLUSIONS, WITH REMARKS, BEING A SEQUEL TO A PAPER FORMERLY READ TO THE SOCIETY. BY DR ROBERT PATERSON.

Dr Paterson read a communication on spectral illusions, in which he detailed a series of curious cases, illustrative of the principle, that illusions of the senses are nothing more than renewed impressions, or vivid ideas of the mind assuming a bodily or external form, to the surprise and often fear of the individual himself. He also stated that various medicinal substances had the effect of producing appearances of this kind; and that it was most probable that such were extensively used in ancient times for assisting in preparing apparatus of their heathen gods.

Several cases were brought forward by the members present, having reference to the subject of spectral illusions.

Dr Alexander Wood, to illustrate the fact, that in some cases phosphorescent lights were visible to some persons and not to others, alluded to a case recorded in the criminal jurisprudence of the country. A weaver residing in Fife had been murdered by his servant; for some time the murder was undetected, till a servant, riding past the garden at night, saw a phosphorescent light hovering over it. The soil was turned up, and the body of the murdered man detected, concealed a few feet below the surface. The person by whom the light was first observed, had in vain attempted to draw to it the attention of a medical man by whom he was accompanied.

Dr Laycock of York stated, that after a severe accident, by which his scalp was half torn off, he became subject a few days afterwards to illusions, which he minutely studied. Sometimes the figures were very small and numerous; at others very large. Occasionally they presented the most beautiful forms; but were always defective about the extremities. He laboured also under auditory illusion, the slightest noise of the poker produced innumerable visions of blacksmiths in regular rows, hammering. A washing basin, or any idea of such a vessel, caused him to see a variety of people making pots, and so on.

Dr Seller called attention to illusions which may lead to practical error as to the identity of persons in courts of justice.

TRIAL OF ANDREW PATERSON FOR THE MURDER OF HIS WIFE, BY WOUNDS ALLEGED TO HAVE BEEN INFLICTED IN THE GENITAL ORGANS.

ANDREW PATERSON was tried at the Ayr Circuit, April 26, before the Lord Justice-Clerk and Lord Wood on a charge of murdering his wife by wounds inflicted in the genitals. The prosecution was conducted by Mr Deas, advocate-depute, the defence by Mr Logan and Mr Thomson, advocates.

Paterson was a farm-overseer and cattle-dealer; he also occasionally acted as a cow-doctor, and was in the practice of cutting pigs. He had eight children by his wife. It appeared in evidence, that Paterson and his wife lived happily enough together, unless when she became intoxicated, which happened not unfrequently. On these occasions he often beat and even kicked her very severe-

ly, and used threatening expressions, such as—"That he would be her butcher," "That he would be hanged for her yet," &c. Paterson himself indulged rather freely in liquor. Eighteen or nineteen days before her death Mrs Paterson was delivered of her eighth child. She was attended by the same midwife who had been with her in all her previous confinements. By the evidence of the midwife it appeared that she had a good recovery; that neither on this nor on any of the former occasions had she shown any unusual tendency to hemorrhage; that the after-birth had come away quite entire; that she had got up four days after the delivery; that about the sixth day, owing to some exposure to cold, the lochial discharge had become somewhat suppressed, and pains arose, but that it was restored by the application of hot cloths, and in two days more she was free from complaint; that she did not consider her in any danger at this time, and that she had no subsequent relapse; that she saw her about a week before her death, and then considered her as quite recovered. On the eighteenth day after her delivery, Mrs Paterson was observed to be slightly the worse of liquor; she went to bed early in the evening. It did not appear whether Paterson had been in his house that evening; but towards eleven o'clock at night he met an acquaintance and accompanied him to a public-house, and there drank rather freely. Mrs Paterson, soon after this time, got up and dressed herself, and went to the same public-house inquiring whether her husband was there; and being told that he was, she purchased some whisky, which she carried away with her, saying to the people of the house that she would return for him. In a short time she did return, and insisted on her husband accompanying her home. At this time she had so far recovered from the effects of any liquor she had taken, that the people of the house considered her sober. They likewise stated, that Paterson was not intoxicated. The parties set out together quietly, and though the distance between the public-house and Paterson's house was no more than between four and five minutes' walk, they were not heard of for more than an hour. At the end of that time Paterson was heard calling on the inmates of his house, and on answering his call they found he had brought his wife to the outer-door dead, or apparently dead, from loss of blood. After some time Mr Sloan, a surgeon, was sent for, who found her quite dead. Paterson made a great demonstration of grief for the loss of his wife; and he told pretty much the same story to his family and neighbours, as he afterwards somewhat more fully embodied in his judicial declaration. He confessed to having upbraided her with her habit of drinking, upon her complaining on the way home of being ill; he said, he had wished to go back to the public-house for some fish he had purchased and left; that after he had gone a few paces in that direction she called to him to come back, as she had been suddenly seized with a bleeding; that on coming back he found the bleeding was from the genitals, and that he attempted to stop it by applying his hand, but that when he withdrew his hand the blood gushed out excessively, and that she then fell upon the private parts at the edge of the ditch on the roadside; that he again attempted to stop the bleeding, by applying his hand, without effect; that she hardly spoke after this, and that he succeeded in getting her to his own house, partly by carrying her, partly by trailing her along with her feet on the ground. It was shown, that between the place where the blood began to flow and his own house, he might have obtained assistance at the houses of relatives if he had called upon them. When daylight came in, pools of blood were seen on parts of the road homewards from half-way between the public-house and Paterson's house, and at some places the blood could be traced zig-zag across the road. In the first part of his conversation with the surgeon, Mr Sloan, immediately after his wife's death, Paterson gave him to understand that he wished an inspection of the body to clear up the cause of death; but on finding Mr Sloan expressed an opinion on the probability of death having occurred from a natural hemorrhage, he became desirous to get a certificate from him that the opening of the body was unnecessary, and he afterwards urged Mr Sloan

on this point on more than one occasion with much earnestness, but without success. The dissection was performed under warrant of the sheriff, on the second day after death, by Messrs Orgill and Sloan, who found the internal organs generally healthy, but drained of blood—"the uterus was the size of a large orange, and contained a little mucous matter tinged with blood; its internal surface was in one or two places slightly mottled with small vascular spots; its orifice admitted the little finger with some pressure; the lips of the orifice were tumid and of a bluish colour. The thighs and external parts of generation were smeared with blood. On the outer part of the right labium pudendi, near the perineum, there was a dark spot the size of a sixpence, and on the inner side of the same labium, and more anteriorly, a similar smaller spot of a blue colour—the labium itself was swollen, and in its substance there was a large extravasation of blood posteriorly an inch deep, anteriorly three inches deep. Within the vagina, and half an inch from its orifice on the right side, there was a superficial lacerated wound three quarters of an inch in length, running parallel with the labia." Within the vagina, an inch and a half from the orifice of the urethra, and running transversely across the course of the urethra, there was a somewhat lacerated wound penetrating to the region of that canal, and dipping to the left of it to the depth of above an inch. This wound was an inch and a half in length, and several long scratches with inflammatory margins were seen running on the inner side from the knee nearly to the top of the right thigh. There was a slight contusion, with ecchymosis, on the outer part of the right eyebrow, and two similar contusions on the left side of the forehead. Paterson was in an adjoining apartment during the inspection of the body, and showed an extreme anxiety to learn what the surgeons were discovering, frequently looking through a hole by which he could see into the chamber. On being apprehended, he made a declaration, part of which is before referred to; and saying further, that his wife had repeatedly told him that, after her last confinement, she had not had the usual after-birth, and that the midwife had upwards of a week before her death attempted to bring it away by means of warm cloths, but without complete success. In Dr Orgill's evidence, given in court, some additional particulars were brought out; he said that there were no clots of blood in the uterus, that no portion of the placenta adhered to its inner surface, and that the hemorrhage could not have taken place from the uterus. He considered the wounds in the vagina to be sufficient to cause death, or attributed the death to the larger of these wounds. An iron instrument, the holdfast of a signboard, had been found in a field a short distance from the first pool of blood. The one end of this holdfast fitted exactly to a rusty mark on a signboard which lay under a bed in Paterson's house, and the other end fitted with equal exactness a hole in the wall upon which Paterson had previously used that signboard. A holdfast precisely similar was proved to have been seen under the bed beside the signboard about two months before the night in question, and on a search being made for it after the death, it could no longer be found. Dr Orgill, two days after the first inspection of the body, had fitted the sharp chisel-like extremity of this holdfast into the larger wound in the vagina, and found it to correspond exactly. Dr Orgill could not detect blood on this holdfast—but he obtained distinct evidence of the presence of iron in the dust found in the pocket of the coat Paterson wore, which easily admitted the instrument. Dr Orgill had never heard of a case of natural hemorrhage so long after delivery, which proved fatal in so short a time, and had no doubt that the hemorrhage was the result of violence, and the cause of death. In this opinion, he was joined by Mr Sloan and Dr Sellar of Edinburgh, to the latter of whom a part of the medical report had been submitted, these medical witnesses concurring in the belief that the hemorrhage had not resulted from natural causes, and that the wound in the vagina was sufficient to cause death. Dr Orgill gave a distinct opinion, that the larger wound in the vagina must have been produced by an instrument, and could not have been made by the fingers. Dr Orgill

was examined at great length, and was complimented by the bench for the distinctness of his testimony.

In defence, it was not contended that the hemorrhage was natural, or that it had been rendered fatal by the improper interference of the prisoner; but that the wounds had arisen accidentally from the woman falling upon some sufficiently sharp substance, and Dr Haldane of Ayr gave it as his opinion, that it was possible such wounds might take place if a female fell in a certain direction on a sharp stone, or sharp piece of wood fixed in the ground—and he mentioned a case which he had read, but he did not remember in what book or journal, where a man had fallen from a height on a spike, which penetrated the rectum without wounding its outer border.

The Lord Justice-Clerk, the presiding judge, suggested to the consideration of the jury, whether the death might not have been occasioned by the prisoner ignorantly and unskilfully applying improper means to stop a hemorrhage which had arisen from natural causes, or perhaps from a kick inflicted by him on the lower part of the abdomen; and whether, therefore, the verdict should not be one of culpable homicide instead of murder, as contended for by the Advocate-Depute. The jury, acting apparently on this suggestion, brought in a verdict of culpable homicide, and Paterson was sentenced to two years' imprisonment.

This case differs from the cases before recorded of murder by the infliction of wounds in the genital organs, in the circumstance that the wounds were not simply incised wounds. If the two wounds described in the medical report had been incised wounds, such as are produced by a cutting instrument with a fine edge, it would have been at once apparent that these wounds had been designedly inflicted. We are acquainted with no more than four recorded cases in which murder was committed in this secret manner, and all those occurred in Scotland. Indeed, Orfila remarks, in noticing these cases, that Dr Watson, in recording them, had disclosed a crime before unknown in medical jurisprudence. In the first of these cases, the wound was on the inner side of the right nympha, an inch and a quarter in length, and penetrated in each of four directions to the depth of two and a half inches. It presented externally a clean straight incision, and appears to have been inflicted with a razor; it did not extend beyond the cellular tissue, and none of the great arteries were wounded. The husband of this woman was condemned, but committed suicide before execution. In the second case, the fatal wound was on the middle of the left labium pudendi. Externally it was a clean incision, about three quarters of an inch in length, which led to a bloody cavity about the size of a small hen's egg, and from this cavity the finger passed in three directions to a greater depth, being at one place between two and three inches deep. When the wound was laid open, the divided orifices of several pretty large arteries and veins were seen, and particularly the divided extremities of the artery going to the clitoris. The division of the bloodvessels, and the whole inner surface of the wound, showed the appearance of a very clean cut with a sharp instrument. Two men were tried in this case, and there was no doubt but that one or other of them had inflicted the wound. It being, however, uncertain which had done it, they were acquitted of the capital charge, and transported for the assault. In the third case, there were three wounds, two of which were about an inch long, at the right labium and orifice of the vagina, the greatest depth being an inch and a half. The husband of the woman was executed at Glasgow. In the fourth case, two wounds were found, one on each side of the right nympha. They were clean cuts with a sharp instrument, about half an inch long, and one an inch and a half deep. The husband of the woman was executed at Glasgow. These cases, then, unequivocally prove that incised wounds in the genital parts of no great depth, and involving no considerable vessel, may give rise to a fatal hemorrhage; and the case of Paterson's wife, whatever view be taken of it, further shows that a wound not perfectly

incised, but somewhat lacerated, may give rise to a fatal hemorrhage, at least within a short time after delivery at the full period. The large quantity of blood, however, poured forth in this case (and it seems to have been larger than in the cases just referred to), is a strong presumption that the wound partook more of the character of an incised wound than of a rent or lacerated wound. Lacerations of the vagina under some circumstances are not unknown; but there is, we believe, no case in which suddenly fatal hemorrhage arose from any such laceration of that canal as could be produced, in its ordinary healthy state, by the fingers, by a kick, or the like. The details of a frightful case which occurred in France last year, have been published lately, where a husband, apparently for the purpose of causing abortion in his wife about the seventh month of pregnancy, tore the uterus and drew out with the fœtus a portion of the intestines, at the same time causing laceration of the vagina. And, notwithstanding the nature of these injuries, it appears that the woman lived about three quarters of an hour after this, and that no such hemorrhage occurred as would of itself have proved fatal.¹ In another case which occurred at Lausanne in 1836, the woman being in the fourth month of pregnancy, the posterior wall of the vagina was torn, the womb was inverted and projected from the vulva, and the peritoneum was torn through an extent of seven inches, the intestines being laid bare but not injured. The hemorrhage was considerable, yet the woman lived seven hours after the infliction of these injuries.² In this case the marks of the nails were thought to be discoverable adjacent to the principal wound. Numerous cases of laceration of the uterus, with or without laceration of the vagina, some accompanied with other severe injuries, are recorded where there was no very remarkable hemorrhage; in fifteen cases of this kind in pregnancy, collected by Dézeimeris, the period of death varies from one hour to six weeks.³ In May 1847, a woman was brought to the Royal Infirmary of this city whose case turned out to be one of rupture of the bladder, with laceration at the orifice of the vagina, the effect of a kick. She was placed under Dr H. Douglas' care. She was not admitted till more than forty-eight hours after the occurrence, and she lived for several hours after, with no sign of great hemorrhage. Just within the orifice of the vagina there was a lacerated wound an inch long and a quarter of an inch in depth, and there was a similar wound half an inch long immediately above the orifice of the urethra. The bladder was ruptured at its upper part, some blood was found in the abdominal cavity, and the cellular tissue adjacent to the bladder was infiltrated with blood.

It may be laid down, then, on the evidence of these cases, that lacerations of the vagina and the other external parts of generation, are as little liable to severe hemorrhage as the lacerations of other equally vascular parts; and, therefore, when a fatal hemorrhage occurs from a wound in these parts, that there is a *prima facie* argument against its being a mere rent or tear, such as could occur from a kick, or violence done with the fingers. A rent, however, in the vagina, undoubtedly can be produced by the fingers; and when Dr Orgill said the wound in Mrs Paterson's case could not be so produced, he undoubtedly merely meant that the wound which he had seen and particularly examined could not be so produced. And this opinion he was perfectly entitled to give. Considering Dr Orgill's intelligence and experience in medico-legal cases, we are bound to attach a due weight to his opinion, that the instrument found near the first pool of blood exactly fitted the wound—and as this instrument possessed a degree of sharpness like that of a stone-cutter's chisel, there is no doubt that the wound made by it, though not quite a clean cut, would partake more of the nature of an incised wound than of a laceration. It is also but justice to Dr Orgill to suppose, that, had there been any natu-

¹ Annales d'Hygiène, Jan. 1848.² Annales d'Hygiène, tome xix. p. 425.³ Journal l'Experience, iii. 241; also Annales d'Hygiène, Jan. 1848.

ral morbid appearance in the vagina predisposing to hemorrhage, it would not have escaped his notice. We have heard conjectures as to the possibility of the bursting of varicose veins followed by fatal hemorrhage; but though varicose veins might make no appearance after death, particularly after a fatal hemorrhage, we are sure no one will seriously maintain that the bursting of varicose veins without external violence could produce breaches in the vagina like those described in Mrs Paterson's case. Some years ago a case occurred in Edinburgh, where a woman died of hemorrhage from the vagina, the effect of a kick directly over its orifice, and on that occasion it was proved, by experiment on the dead body, that rents could be so produced in that canal. There it is supposed that the veins were in a varicose state, and hence that the hemorrhage proved fatal; other cases of a like kind are referred to, on which our limits forbid us to enter, as one which occurred at Bristol and another at Berlin. Again, fatal cases of hemorrhage have occurred before parturition, where it has been proved that the loss of blood arose not from the uterine vessels but from laceration of the vagina. Dr Doherty of Dublin has given the details of a case of this kind, where no sufficient cause could be discovered for the rupture, except that the structure of the vagina may have been much weakened by several very difficult previous labours—he also refers to a fatal case in which a laceration of the vagina occurred with fatal hemorrhage, near the end of pregnancy, from a fall. The woman, who was a cook, had been standing on a chair with a broken back, and in falling the short extremity struck her in the private parts.

When, however, the whole circumstances of the case of Paterson's wife are considered, we think the idea of accident wholly out of the question; and though it is not unlikely that the first violence to which she was subjected was a kick in the right labium, yet that of itself would be insufficient, and the absence of all marks of injury near the external orifice of the vagina, shows satisfactorily that no kick was given in that region.

TRIAL OF ELIZABETH LAIRD FOR INFANTICIDE, AND CONCEALMENT OF PREGNANCY;
PRESUMED IDENTIFICATION OF AN INFANT'S BODY FROM MONSTROSITY, SUPPOSED
HEREDITARY, IN THE DIGITAL EXTREMITIES.

ELIZABETH LAIRD or STEWART, a widow, the mother of two illegitimate children, and of one younger legitimate child, was charged at the Ayr Circuit Court, April 25, alternatively with child-murder or concealment of pregnancy. It was proved by several witnesses, that, prior to the 3d of February, she had all the appearance of being with child; that she kept as much as possible out of the sight of her neighbours; that a few days after she was seen walking in the village with her size much reduced, and was met on her way to Beith, which is a short distance from Gateside, the village in which her father resides, having on a large cloak, and under it a bundle. Soon after the time when she must have reached Beith, the body of a new-born infant was found within a burying-ground, near the open gate of which she must have passed in her way to a shop where she purchased some necessaries. The whole difficulty in the trial lay in the identification of the infant found in the burying-ground in Beith, with that of which Elizabeth Laird plainly had been delivered. She herself indeed affirmed in her declaration, that she had had a false conception, which came away with much hemorrhage at the time of her supposed delivery. The medical report made by Drs Paxton and Miller, on the contrary, declares that she had recently given birth to a child, the grounds stated being—that the breasts were hard and full, and contained a considerable quantity of milk; that the nipple was firm and turgid, the areola around it large and well defined, the papillæ in the areola being large and distinct; that the belly was enlarged and wrinkled, the vagina relaxed and flabby, also unusually moist; that the uterus felt larger than in the normal state; and that the os uteri was flabby and enlarged, so as to be capable of admitting the points of one or two fingers.

The medical report by the same gentlemen, on the state of the infant found dead in the burying-ground in Beith, declares their belief that the infant had reached maturity ; that it had been born alive, and that it had breathed. The normal appearances indicated are numerous livid marks and abrasions on the surface of the body. "There was an abrasion of the cuticle of the size of the point of the finger on the left side of the front of the throat, and a smaller one on the right side, corresponding nearly to the situation of the top of the windpipe." The bloodvessels and membranes of the brain were in a highly congested state, and a considerable quantity of extravasated blood was found under the scalp in the situation of the anterior fontanelle. The lungs crepitated and floated in water, were of a light red colour, and filled three-fourths of the cavity of the chest. The other organs were normal, and there were no signs of putrefaction. The umbilical cord presented the appearance of having been torn across, and had not been tied ; it remained attached to the body of the infant to the extent of twenty-one inches and a half. On each foot there were six toes, the great toe projecting somewhat like a thumb. In the report no mention is made of the cause of death ; but in their examination the two gentlemen who signed it ascribe the child's death to suffocation produced by compression of the windpipe with the fingers, the marks of which on the fore part of the neck they considered to be distinct.

The identification rested mainly, 1. on the coincidence between the time of the prisoner's visit to Beith with a bundle under her cloak, and that at which the infant found in the burying-ground must have been laid down ; 2. on the close resemblance which the coverings of the infant found in the churchyard bore to articles known to have belonged to the house in which the prisoner resided ; 3. on the presence in the infant found of a monstrosity in the digital extremities, not exactly coincident with, but parallel to one prevailing in the prisoner's family. In explanation of this last point, it should be stated that the prisoner's mother has two thumbs on one hand, and a double thumb on the other ; and that a brother of the prisoner, now deceased, had a double thumb on both hands, these facts being spoken to by one of the medical witnesses as having fallen within his own knowledge.

The infant found in the churchyard had, as noticed above, six toes on each foot. The court held the questions put for the crown on this point competent questions, whatever might be the value of the answers in evidence.

The advocate-depute passed from the capital charge, and the jury accordingly found a verdict of concealment of pregnancy—punishment fifteen months' imprisonment.

This case is highly interesting in several respects. The failure as regards the capital conviction arose, as it would seem, from the defect of legal proof as to the identification of the infant's body. The admission by the court, of an argument in favour of the identification from the parallelism of the monstrosity in the infant's toes, with the monstrosity in the fingers prevalent in the alleged parent's family, is a curious subject for consideration by medical jurists. The prisoner's plea of a false conception or mole, as respects a person thirty-nine years of age, and who had borne several children, might have created much difficulty in the case. We suppose no one will doubt the prisoner's guilt, yet a conviction on the charge of murder would not have been satisfactory without evidence of a more direct kind.

TRIAL OF DR GIBSON OF GLASGOW FOR CULPABLE HOMICIDE, GIVING A FALSE CERTIFICATE, AND NEGLECT OF DUTY—ACQUITTAL—DANGERS INCURRED BY MEDICAL MEN FROM CORONERS' INQUESTS.

This was a trial originating out of a Coroner's Inquest which took place at the Millbank Penitentiary in February last, on the body of Patrick M'Ginty, a convict sentenced to transportation, who had been sent from Glasgow to Lon-

don in January, with the following certificate granted by Dr Gibson, surgeon to the Glasgow prison—"I hereby certify, that Patrick M'Ginty (with thirty-six others), about to be removed, under sentence of transportation, from the prison of Glasgow to the Millbank prison, are free from infectious disease, insanity, idiocy, acute inflammatory attacks, or fever, and that their health will not be endangered by removal thither." The man laboured under pulmonary phthisis, and on arriving at the Millbank prison, was stated by the physician to that institution, Dr Baly, to be unfit for reception, and at the same time too weak to be sent back to Glasgow. He was placed in the Infirmary, where he died on the 10th of February. A *post-mortem* examination was made of the body by Mr Hall, resident surgeon of the prison, in the presence of Dr Baly and Dr Basham. A coroner's inquest followed, in which these gentlemen very strongly stated their opinions, that the deceased ought not to have undertaken the voyage, and that his death was accelerated by it. The jury in consequence returned a verdict, "that the deceased died from natural causes;" but added, "that the deceased ought not to have been removed from Glasgow jail, and that the surgeon of Glasgow jail ought not to have permitted the deceased to take the journey; and that death was accelerated by the journey." The Lord Advocate, therefore, indicted Dr Gibson for the offences named at the head of this article, and the trial took place before the Lord Justice-Clerk, Lord Cockburn, and Lord Wood, on the 18th of last May. The prosecution was conducted by Mr Crauford and Mr Deas, and the defence by Mr Moncreiff and Mr Millar. The agents for the defender were Mr Leishman of Edinburgh, and Mr Crawford of Glasgow.

As it is the medical facts only which are important to our readers, all that we think it necessary to state of the other evidence is, that three forms of certificate have been issued by the secretary of state, in reference to the removal of prisoners. In the first certificate, phthisis pulmonalis and a variety of other affections were to be considered as incapacitating the individual from being sent to London. The effect of this was to cause a great accumulation of convicts in the local prisons. In the second certificate, therefore, phthisis pulmonalis and several disorders were excluded, and this was the document Dr Gibson signed. Experience proved, however, that the effect of this certificate was indeed to free the local prisons, but to cause an inconvenient accumulation of prisoners at Millbank penitentiary, in such a state of disease that they could neither be transported beyond seas, nor be employed in any useful work. The form of certificate was therefore again changed, with a view of preventing individuals labouring under a variety of diseases (and phthisis pulmonalis among the number) from being sent to London.

Now, it unfortunately happened, that this third change in the certificate was unknown in Glasgow when M'Ginty was sent away. The governor of Millbank prison, Mr Groves, it is true, stated that he had received prisoners with the last form of certificate from the Lanark prison, four months before the arrival of M'Ginty from Glasgow, and it would seem that the new form had been sent every where but to Glasgow. If fault existed any where, therefore, it must have been with the government officials, who forgot to forward the proper document to Glasgow. At the same time, it need not excite surprise, that though Dr Gibson was in no way to blame, the London officials, who had been for months acting on the new regulations, should consider that those in Glasgow exhibited great carelessness in forwarding to them a person, who clearly in their opinion ought not to have been sent. This circumstance doubtless tinged all their evidence before the coroner, and perhaps insensibly led the medical witnesses on that occasion to make statements, which, as we shall subsequently see, were neither warranted by circumstances nor by the present state of medical science.

The only other facts deserving of notice, as proved in evidence, were, that M'Ginty before his departure was known by all the officials in the Glasgow prison to be in ill health, although they thought he could bear the voyage

well enough. It was proved by one of the wardens that Dr Gibson had made a most careful examination of Patrick M'Ginty, with the trunk of his body naked. The notes of Dr Gibson himself show that he detected the phthisis pulmonalis, and had treated the man for a diarrhoea, which was easily checked. He, however, was very anxious to leave Glasgow, considering that change of air would be beneficial to him. His strength, though not great, was still good. It was found that on the morning of his departure he dressed himself, and descended the stairs with slight assistance, according to some, no assistance; that, at all events, he could go up stairs unassisted. He walked without difficulty or pain, and made no complaint whatever. Under such circumstances, as stated by Dr Gibson in his declaration, he gave the certificate advisedly, conceiving that he would die any where; but that the voyage, change of air, and superior accommodation of the Millbank prison would tend to prolong his life, rather than shorten it. On the morning of his removal, the sheriff's officer, although he was informed of M'Ginty's weak state of health, ironed him in the usual way by the leg to another prisoner. He was removed by railway to Edinburgh, from thence in a steamer to London. The voyage was a good one. M'Ginty lay in bed the whole time, but unfortunately eat several oranges, which he had received from a female friend before leaving Glasgow. The diarrhoea returned, and is said to have been distressing, although on arriving in London he was not observed to be weaker than when he set out. The officials of the Millbank prison noticed nothing particular about him until his emaciation became evident under examination, and he made no complaint.

We shall now condense the medical evidence given at the trial, from which it will be observed that the opinions of the medical men in London were directly at variance with those of Glasgow and Edinburgh.

Dr William Baly is physician to the Millbank prison. Examined a prisoner named Patrick M'Ginty, who arrived from Glasgow on the 21st of last January. He was in such a bad state of health, that, according to the rules of the prison, he ought not to have been received. Neither could he from weakness be safely returned, and in consequence of a certificate given by the witness he was sent to the Infirmary and retained. The prisoner, on being examined with his shirt off, was found to be emaciated in an extreme degree; his abdomen was tumid, his pulse quick and feeble; on feeling the abdomen fluctuation could be distinguished; signs of disease in the lungs were detected, although the characters were not so marked as those in the abdomen. The prisoner stated, that his bowels had been very loose for some weeks, and especially so during the voyage from Glasgow; that he had cough, and for the last three months had been losing flesh. This account, with the signs previously detected, induced the witness to believe that he laboured under tubercular disease in the abdomen and in the lungs. He considered that the complaint had been of considerable standing, at least three months; was of a dangerous nature, and most certainly fatal. The fluctuation in the abdomen must also have existed some time. He was of opinion that the prisoner ought not to have been sent from Glasgow, and that his health must have been such as rendered it very dangerous to remove him from there. The abdominal swelling was perceptible to the eye, and the fluctuation was detectable by any one of medical skill. He died on the 10th of February. He could not say positively whether death had been accelerated by his removal, not having seen the man before he left Glasgow, but had no doubt that before leaving that city he was in a hopeless state. No reasonable hope could be entertained of his recovery. Considered that M'Ginty must essentially have been in the same condition three days before he saw him, although he might have been weaker.

The medical witnesses for Dr Gibson were then called into court to hear the account of the

Post-Mortem Examination.—This was performed the day after death by Mr Hall, in the presence of witness and in that of Dr Basham. The body was extremely emaciated, the abdomen appeared distended; there was a slight

green colour on the surface of the skin of the abdomen ; in the head there was no disease in any part ; in the chest, the heart was small, and the membrane covering it free from disease. The membrane lining the heart was stained with blood, but in every other respect was in a natural state ; the right lung was permanently adherent to the chest, and there were adhesions posteriorly to a considerable extent ; the lining of the air tubes of this lung was of a deep red colour, and at the summit of the lung there were many tubercles, some of them softening at their centres ; there were masses of chalky matter mixed with these tubercles, and the tissue of the lung at this part was much condensed ; other parts of this lung were loaded with fluid blood, especially posteriorly, but its tissue was every where crepitant. In the false membrane covering this lung there were also tubercles. The upper surface of the diaphragm on the right side, was covered with a false membrane containing numerous tubercles. The left lung was firmly adherent to the walls of the chest, posteriorly ; at its summit there was an irregular ulcerated cavity about the size of a walnut, and there were numerous smaller cavities in the upper part of this lung, the texture of which around and between these cavities was solidified by tubercular deposit. The lining membrane of the air tubes in the rest of this lung were in the same state as in the opposite lung.

In the abdomen the great omentum was adherent to the walls of the abdomen anteriorly, and was much thickened by deposit of lymph and tubercles. Similar deposits also covered the membrane lining the abdominal walls. Between the omentum and the bowels there was a free cavity filled with serous fluid. The smaller bowels were firmly glued together into one mass, by deposit of lymph and tubercle, and the under surface of the diaphragm, which was in contact with the liver, was covered with a nearly continuous layer of these. The liver was soft, but otherwise healthy. The spleen on the surface of the stomach was covered with tubercles. The stomach presented no appearance of disease. On the mucous surface of the ileum were seven ulcers of various sizes, the medium size being that of a sixpence. All these ulcers were of a chronic character, and contained tubercular matter at their margins. At the commencement of the large bowel there were more extensive ragged ulcerations. The mesenteric glands were much enlarged ; some were the size of an egg. They were full of tubercular matter, almost of creamy softness. The other regions presented no appearance of disease.

Mr Edward Hall is the resident surgeon of the Millbank prison, and made the examination of the body of Patrick M'Ginty, together with Drs Baly and Basham. (Here witness repeated the substance of the foregoing *post-mortem* examination).

Cross-examined.—There were symptoms of perforation of the gut the night before he died, when he was suddenly seized with pains in the bowels, sickness, and fainting. On dissection, an ulcer was discovered covered over with lymph, which would have ruptured on removing the intestines. This was seen before the bowel was opened. There was some serum in the cavity of the abdomen, but not much. It was rather turbid, of a darkish dirty yellow colour.

Dr William Richard Basham.—There were many ulcers in different parts of the abdomen, but certainly no perforation. A stream of water was directed into the bowel, and if any perforation had existed witness would have seen it. There was serum in the abdomen. It was turbid, and of a dark dirty colour. An ulcer might heal by the deposition of lymph ; but if perforation had taken place it would prove fatal. (Here witnesses for Dr Gibson retired). The cause of death was tubercular peritonitis. Judging from the *post-mortem* appearances it must have been of considerable duration, perhaps months. A person with such a disease certainly could not be removed from Glasgow without danger. If he saw an emaciated person with a tumid abdomen, he would form a strong opinion that he laboured under tubercular peritonitis.

Purging is not a necessary symptom. Assuming the symptoms of purging, tumid abdomen, and emaciation, the journey from Glasgow certainly could not be performed with safety.

Cross-examined—Disease of the lungs existed to a considerable extent, but he might have lived for months with it. Ultimately it would have caused death. He might also have lived for some time with the disease in the bowels. The symptoms of tubercular peritonitis are not always well marked; but if concomitant circumstances were present, he would not hesitate to pronounce the disease to be present. He was constantly in the habit of forming opinions on such cases, and acting upon them. There are some cases undoubtedly obscure. Symptoms sometimes come on rapidly, but at others just as slowly. Swelling of the abdomen may come on rapidly; but, as a general rule, emaciation and swelling do not so come on. The statement by Louis (translation by Walshe, p. 226), that “the symptoms of chronic peritonitis, an affection I have only met with in tuberculous subjects, are, generally speaking, of slight consequence, few in number, and pretty frequently pass unnoticed,” refers only to the earlier stage; but in the advanced stage the symptoms disclose themselves so that any physician might form an opinion. The swelling of the abdomen arises from the absorption of lymph, and from the effusion of fluid of a very peculiar character. In scrofulous peritonitis it is almost impossible for the serum to form in the abdomen suddenly—it is contrary to the nature of the disease that pure serum should be evolved, and except pure serum, did not think there would be any sudden effusion in the course of a few days. It is rare for an acute attack of peritonitis to supervene upon chronic peritonitis. It may happen, and, the effusion coming on rapidly, would depend upon the previous disease. The progress from the chronic to the acute form is possible. Diarrhœa is a common symptom in pulmonary phthisis. Eating fruit or any indigestible substance would aggravate any disease. The symptoms of perforation of the bowels are vomiting, with a rapid sinking of the vital powers, but not necessarily pain. He decidedly thought that if tubercles existed in the lungs alone, not to speak of their presence elsewhere, it would be most hazardous to send a man a voyage in that season of the year.

Re-examined by the Advocate-Depute.—The disease was of old standing. Thought the disease must have been perceptible on the 14th of January. If detected, the patient could not safely be removed to London.

Examined by the Lord Justice-Clerk.—The man must have died, although not removed from Glasgow. There were no appearances of acute peritonitis.

Dr Baly recalled.—The cause of death was the weakness produced by the peritoneal disease, together with that of the lungs, pleura, and intestines. The disease was certainly of long standing. There were no appearance of recent disease. The examination of the body confirmed his opinion as to the impropriety of removing the man.

Cross-examined.—The serum found in the abdomen was of a straw-colour, and perfectly clear. It was not turbid, to the best of his recollection. Did not think it would have formed within three weeks. Thought that the symptoms must have been the same on the 19th of January, as when he saw the patient on the 21st. The fluctuation must necessarily have been discernible two days previously. Considered the fluid must have existed four weeks before the dissection, judging from its character. Firmly believed that the fluid existed longer than a month. It must have been perceptible all that time at least, though cannot say positively how long previously. The quantity may have increased latterly. The abdominal distension from the fluid might come on in a week; but that from the deposit of lymph and tubercle must have existed many weeks. Tubercles may form rapidly, but not such tubercles. In the majority of cases, a medical man will not be mistaken as to the existence of fluctuation. The symptoms of perforation are great and sudden pain in the abdomen, which is almost uniform at the moment of rupture; it is followed by great depression of the powers, faintness, and sickness. The symp-

toms preceding M'Ginty's death were increasing weakness, loss of appetite, and anxiety of countenance. M'Ginty's diet in the prison was farinaceous. He could not take animal food; once, at his own desire, a mutton chop was ordered, but he could not take it. He had aromatic mixture, with laudanum, to stop the diarrhoea, and afterwards steel pills.

Re-examined by the Advocate-Depute.—There was no indication of recent disease. He had faintness and vomiting the day before his death, but there were no appearances of perforation discovered afterwards, although one ulcer had gone through all the coats; but it was not a complete perforation.

Examined by the Lord Justice-Clerk.—Was quite sure that diarrhoea for two days during the voyage would not have produced any important change on the appearance of the abdomen.

Mr Hall recalled.—The only point elicited from Mr Hall, in addition to the evidence of the last witnesses, was, that on the evening of the 9th, about six o'clock, M'Ginty complained of pain in his abdomen, with sickness and vomiting. From this time he continued to sink, notwithstanding the free administration of stimulants, and died about nine o'clock on the morning of the 10th. Was fully of opinion that the journey endangered his life.

This closed the evidence for the prosecution.

(We have waited up to the hour of going to press, in the hope that the shorthand reporter would furnish us with the conclusion of this trial. In this we have been disappointed, and are therefore compelled to give the medical evidence for the defence, with our remarks on this important case, in the next Number).

NEW MUSEUM OF MATERIA MEDICA OF THE ROYAL COLLEGE OF PHYSICIANS IN
EDINBURGH.

WE announced some time ago, that the museum of materia medica collected by Dr Martius of Erlangen, long known as one of the most complete in Germany, had been purchased by the Royal College of Physicians of Edinburgh. This almost unrivalled collection is now arranged and laid out for inspection in a commodious part of the Edifice lately erected by the College in Queen Street. We recommend it to the attention of such of our readers as can command an opportunity of visiting it. The articles are placed in elegant plate glass cases, admirably adapted for easy exhibition, and the perfect display of their characters. The collection contains all the substances that can be conceived to belong to the subject. It is strictly, however, a museum of the materia medica; not one, of those objects of natural history from which the materia medica is derived, but solely of the substances which have a just title to a place in the laboratory. And, as might be anticipated, in point of minuteness and variety, the collection bears unmistakable marks of its German origin.

Not content with enriching the number of barks with seven specimens of the well-known envelope of the bark of the *Quercus suber*, the collector has added twenty-four groups of corks variously cut, for stopping all manner of casks and bottles. In another part of the room, sixteen varieties of sponge excite our surprise; and not far distant twenty-six forms of isinglas. Here also the eye is met by sugar of milk in loaves, oyster shells, Matres Perlarum, and actual pearls, Chelæ Cancrorum, Lapilli Cancrorum, Umbilici maris, masses of ivory, and a genuine hart's horn. We hardly get over our surprise at seeing, among specimens of the materia medica, the familiar pincers of the great claw of the common crab, when "*sanguis hirci*" draws attention, and the memory busies itself with Celsus and Pliny, and their catalogues of drugs "*hircinus sanguis adamantem rumpens*." We fear, however, even Martius has omitted the remedy spoken of by Celsus, "*sanie jecinoris hircini, seu minus caprini*."

But enough of such trifles, which we have referred to merely to prove how well the collection comes up to the standard of German completeness. The

same spirit of minuteness, carried into those subjects which have long been sources of dispute and embarrassment in materia medica, is what gives a practical value to this collection. And, with the aid of the numerous varieties of drugs which are here exhibited, a hope is afforded that the Edinburgh College, in the next edition of their Pharmacopœia, will be able to clear up some of the contested points as to the origin and distinctive characters of many medicinal substances.

This collection contains, among the barks, thirty commercial varieties of Cinchona, well illustrating the latest views taken on the subject by botanical and pharmaceutical authorities ; the bark of the *Strychnos Nux vomica*, now established as the false *Angustura*, the *Cortex Capalchi*, from the *Croton pseudo-China*, believed by some to be the principal commercial variety of *Cascarilla* ; the bark of the *Mispilodaphne pretiosa*, called also *Casca pretiosa*, remarkable for its odour and taste, resembling cinnamon mixed with orange flowers, or oil of bergamot ; numerous varieties of cinnamon and cassia ; the bark of *Guaia-cum*, once officinal, and still held by some more efficacious than the wood, the *Bebeerine bark*, from the *Nectandra Rodiæi* ; the *Cortex Gogo*, from the *Mimosa Lebbeck*, and many others, with the names of which we are hardly acquainted.

There are fifteen varieties of senna, and as many of tea, among the leaves. There are twelve forms of aloes, and as many of catechu ; and there are fifteen varieties of the caoutchouc bottles of commerce ; of kino twelve varieties ; of opium twelve varieties ; of guaiacum resin seven varieties. There are eight varieties of jalap, besides the spurious kinds ; eleven varieties of rhubarb ; eight of sarsaparilla. The collection of woods is extensive and beautiful. Of seeds there is a very ample display. In the purely chemical department, the specimens are very numerous and well chosen. In short, this contribution of German industry, together with the considerable collection before made, has put the Edinburgh College of Physicians in possession of a museum of materia medica which, we think, it will be difficult to match in any part of the world.

GUTTA PERCHA FOR ARTIFICIAL PALATE.

A CORRESPONDENT tells us that he has used gutta percha for an artificial palate with entire success. The substance, after some months' use, has proved to be suitable in all respects. In all cases, when the membrane within the nostrils is too tender to bear the sponge, a plate of gutta percha will answer very well—a very thin plate of it not being softened by the temperature of the mouth. It keeps quite clean. From a mould of the parts the artificial palate of gutta percha can be formed with great ease, requiring very little time and expense. When bones are coming, or have recently come away, and the parts are tender, this substance as a temporary instrument will be useful to the surgeon and his patient ; and for the number of poor persons who suffer from congenital and other deficiencies of the palate (much greater than is usually imagined), and who are prevented from obtaining relief from a truly distressing condition by the expense of gold and platina palates, this application of the Protean gutta percha will be a boon.

A prepared caoutchouc can be easily and firmly attached to the gutta percha when warm. Our correspondent thinks that a soft palate of the former substance applied to a hard one of the latter, would relieve a very large number of those unhappy persons who suffer from fissures and deficiencies of various kinds.

RETURN OF MR SYME TO EDINBURGH.

MR SYME is about to return to Edinburgh, having resigned the chair of Clinical Surgery he has held so short a time in University College, London. It

seems that the Council of the College, on the resignation of Professor Cooper, requested him to occupy the chair of Systematic Surgery in addition to his own, and intimated that, in the event of his non-compliance, they would be greatly embarrassed in directing the surgical instruction of the school. Under these circumstances, Mr Syme has considered himself bound to withdraw from the institution,—a step which, however unnecessary it may seem to some, is rendered unavoidable by a variety of considerations, into which we need not enter. We sincerely sympathize with the fortunes of University College on the occasion, which has thus lost a clinical professor it will find very difficult to replace. The miserable cry in favour of the students of the school, may now be again raised with advantage, and we shall wait impatiently to see what surgical leviathan it will stir up from the quiet and undistinguished stream of pupils it has poured forth for some years past.

BOOKS RECEIVED.

1. Recent Advances in the Physiology of Motion, the Senses, Generation, and Development. By William Baly, M.D., F.R.S., &c., and William Senhouse Kirkes, M.D. London. 8vo. 1848.
 2. Observations on the Cultivation of Organic Science, being the Hunterian Oration, delivered February 14, 1848. By Richard Dugard Grainger, F.R.S., &c. London. 8vo. 1848.
 3. The Nature and Treatment of the Epidemic or Asiatic Cholera, &c. By Robert Venables, A.M., M.B., &c. Fourth Edition. London. 12mo. 1848.
 4. Lehrbuch der Pathologischen Anatomie mit Rücksicht auf die Anwendung am Krankenbette. Von Dr Carl Ernst Bock. Professor der Pathologischen Anatomie an der Universität zu Leipzig. Leipzig. Small 8vo. 1848.
 5. On the Symptoms, Causes, and Treatment of Puerperal Insanity. By James Reid, M.D., &c. (*from the Psychological Journal*.)
 6. On the Cure of Cataract, with a summary of the best Modes of Operating (Continental and British). By Hugh Mill, &c. Liverpool. 8vo. 1848.
 7. The Plant; a Biography. In a series of Popular Lectures. By M. J. Schleiden, M.D., &c. Translated by Arthur Henfrey, F.R.S. Five Coloured Plates, and Thirteen Wood Engravings. London. 8vo. 1848.
 8. Treatise on the Falsifications of Food, and the Chemical Means employed to detect them. By John Mitchell, M.C.S., &c. London. 12mo. 1848.
 9. Principles of Physics and Meteorology. By J. Muller, Professor of Physics at the University of Freiburg. 530 Engravings on Wood, and Two Coloured Plates. London. 8vo. 1847.
 10. An Elementary Treatise on Crystallography. By M. V. Regnault, &c. With 108 Wood Engravings. London. 8vo. 1848.
 11. A Dispensatory, or Commentary on the Pharmacopœias of Great Britain, &c. Second Edition, Revised and Improved, with a Supplement, containing the most important new Remedies. By Robert Christison, M.D., V.P.R.S.E., &c. &c. Edinburgh. 8vo. 1848.
 12. The Journal of Psychological Medicine and Mental Pathology. Edited by Forbes Winslow, M.D. No. 2. London. 8vo. 1848.
 13. Mémoires des Concours et des Savants Etrangers, Publiés par l'Académie Royale de Médecine de Belgique. Bruxelles. 4to. 1847.
 14. Mémoires de l'Académie Royale de Médecine de Belgique. Tome premier. Bruxelles. 4to. 1848.
 15. Report regarding the Affairs of the Royal Infirmary of Edinburgh, from 1st October 1846, to 1st October 1847. 8vo.
 16. Physician's Annual Report to the Managers of the Royal Edinburgh Asylum. 1847. 8vo.
 17. Statistical Tables of the Royal Infirmary of Edinburgh. Seventh Series. Compiled by John Hughes Bennett, M.D., &c. Edinburgh. 1848.
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